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Nature

A WEEKLY

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"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

*"To the solid ground
Of Nature trusts the mind which builds for aye."*—Wordsworth.

THURSDAY, MAY 3, 1888.

VOLAPÜK, PASILINGUA, SPELIN, LINGUALUMINA.

Volapük or Universal Language. By Alfred Kirchhoff. (London: Swan Sonnenschein and Co., 1888.)

Key to the Volapük Grammar. By Alfred Kirchhoff. (London: Swan Sonnenschein and Co., 1888.)

Elementar Grammatik zur Weltsprache (Pasilingua). By P. Steiner. (Berlin: Louis Heuser, 1887.)

Spelin, Eine Allsprache. By G. Bauer. (Agram: Franz Suppan, 1888.)

Lingualumina, or Language of Light. By F. W. Dyer. (London: Industrial Press, 1875.)

"IF only we had been consulted at the creation of the world, good as the general working of the machine is, how many little improvements might have been introduced!" This remark, not meant to be irreverent, is often heard when people suffer from toothache either at the arrival or at the departure of their molars, or when a sudden frost sets in and destroys the blossoms on all the fruit-trees in their garden. Volapük seems suggested by the same kind of sentiment. Languages, the adherents of Volapük seem to say, are all wonderful machines, but, if we could only have been consulted by the original framers of human speech, how many little irregularities might have been eliminated, how much might the whole working of the machine have been simplified, and what a saving of fuel might have been effected if instead of a thousand of these linguistic machines, each having its own gauge, there had been one engine only, taking us from Fireland to Iceland without any change of carriages.

Those who lament the imperfections of human speech may claim, however, this advantage over the grumblers at the world at large, that they are quite prepared to produce a better article. Again and again has the world been presented, not only with new alphabets and new systems of spelling, but with brand-new languages. Of late, however, there has been quite a good measure of them pressed down and running over. At the head of our article we have mentioned four only, called respectively *Volapük*, *Spelin*, *Pasilingua*, and *Lingualumina*. But there have been several more proposals for a universal language sent to us lately from various quarters of the world, all equally ingenious, though we are sorry we cannot disinter them from beneath that mighty cairn of pamphlets which is growing up from week to week in our library.

All these proposals have one thing in common. They start from a fact which cannot be disputed, that life is too short to learn more than four or five languages well, and that it is perfectly wicked to write books on scientific subjects in any language but English, French, German, or Latin. They then go off into raptures about the days when "the whole earth was of one language and one speech," and they even appeal to prophecy that it has been promised "that a pure language will be turned to the people, that they may all call upon the name of the Lord, to serve him with *one* consent."

And how is that prophecy to be fulfilled? Here the answers begin to vary a little. Some people say, Let everyone learn English, and the problem is solved at once. So it would be, so perhaps it will be, when the leopard shall lie down with the kid. But till that comes to pass different kinds of compromise are suggested. First of all, as to grammar, there is no excuse for any irregular nouns or irregular verbs, for gender as different from sex, for obsolete degrees of comparison, or for any involved syntactical constructions. These ought all to be abolished. Secondly, as to the dictionary, it is quite clear that if 15,000 words sufficed for Shakespeare, a dictionary of 250,000, like the English dictionary now being published by the University of Oxford, is the most fearful extravagance ever known. Here all inventors of a new language insist on retrenchment. The inventor of Volapük was satisfied at first with a dictionary of 10,000 words, but we are now promised a new one of 20,000.

There is a great difference of opinion, however, when the question arises from what source these words ought to be derived. Some draw their words at random from a number of the best-known languages, others confine themselves, as much as possible, to words common to German, French, and English. *Volapük* draws on several banks, chiefly on English, but it clips its coins fearfully. Thus, its very name, *Volapük*, is taken from German and English. *Vol* represents the German *Volk*, *pük* the English speech, so that *vola-pük* means originally folk-speech. In the same manner *appetite* has been replaced by *potit*, *abundance* by *bundan*, *silver* by *silef*, *Jew* by *yudel*, *house* by *dom*. In many cases these borrowed words have been so much changed that it is difficult to recognize them. Here *Pasilingua* has a great advantage. All its words remind us of a Teutonic or Romanic prototype, or of English, which has amalgamated these two elements in its dictionary. Volapük often requires a commentary, where Pasilingua allows us to guess with a good chance of success. Thus—

What o'clock is it? is in Volapük *Düp kimid binos?* in Pasilingua *Quota hora er al?*

Where do you live? is in Volapük *Kiplace lödens?* in Pasilingua *Ubi habitirs tüs?*

The sentence, Advertisements are to the man of business what steam is to industry, has been rendered in Volapük by *Lenunc binoms jafaman otos kelos stemplo dustor*; in Pasilingua by *Annóncius ers pro tos affäriros qua ta vapora pro ta industriu.*

After Volapük has once chosen what may be called its stems, which consist mostly of a consonant, a vowel, and a consonant only, everything else becomes easy enough Thus if *fat* stands for father, we get a simple declension:—

	Singular.	Plural.
N.	*fat*, father	*fats*
G.	*fata*	*fatas*
D.	*fate*	*fates*
A.	*fati*	*fatis*

Pasilingua declines:—

	Singular.	Plural.
N.	*mortu*, the death	*mortas*
G.	*mortude*	*mortasde*
D.	*mortuby*	*mortasby*
A.	*mortun*	*mortan*

Spelin declines:—

	Singular.	Plural.
N.	*mik*, a friend	*mikoes*
G.	*doe mik*	*doe mikoes*
D.	*tu mik*	*tu mikoes*
A.	*mik*	*mikoes*

It is clear that there are ever so many ways by which the same result might be obtained, so long as the principle is strictly adhered to that each case shall have but one sign, and that the same sign is to be used in the plural and the singular, while the plural again is indicated by a sign of its own. In Bengali and many other languages the same principle is carried out with considerable consistency. What applies to declension applies to conjugation, to degrees of comparison, and to derivation. All becomes regular, simple, intelligible, whatever set of suffixes, prefixes, or infixes we adopt. Thus, to have is *lab* in Volapük. Hence:—

Singular.	Plural.
labob, I have	*labobs*, we have
labol, thou hast	*labols*, you have
labom, he has	*laboms*, they have
labof, she has	
labos, it has	
labon, one has	

By assigning to each suffix one peculiar power, Pasilingua distinguishes: *mortu*, death, *morto*, dead, *morte*, dead (fem.), *morta*, dead (neut.), *mortiro*, dying, *mortaro*, murderer, *mortamenta*, instrument of murder, *mortana*, poison, *mortarea*, battle-field, *mortitarea*, churchyard, *mortiblo*, mortal, *mortablo*, fatal, *mortoblo*, easy to kill, *morter*, to be dead, *mortir*, to die, *mortar*, to kill, *mortor*, to be killed, &c.

These few extracts will give our readers an idea of what they have to expect from *Volapük*, *Pasilingua*, and *Spelin*. *Spelin* has nothing to do with spelling. It is derived from *lin*, the abbreviated stem of *lingua*. *Pe* (from Greek *pas*) means all, *s* on account of its continuous buzzing sound is used to form collective nouns; hence *s-pe-lin* means all-language, or *Pasilingua*.

The study of these systems is by no means without interest and advantage. It will help to clear people's ideas about the great complexity of language, and show how simple a process grammar really is. If more generally adopted, as Volapük seems likely to be, such a system of writing may become even practically useful, particularly for telegraphic communication. That it could ever supplant our spoken language is out of the question, and Dr. Schleyer, the inventor of Volapük, distinctly disclaims any such intention ("Hauptgedanken," p. 10, note). One protest only we have to enter before leaving the subject. Nothing could be a greater mistake than to imagine that these clever and amusing experiments have anything in common with Leibniz's conception of a philosophical language. What Leibniz had in his mind may be guessed from the "Essay towards a Real Character and a Philosophical Language," by Bishop Wilkins, London, 1668, of which an abstract is given in Max Müller's "Lectures on the Science of Language" (vol. ii. p. 50). This is as different from Volapük as the *Kriegspiel* is from real warfare. For spending a dreary afternoon pleasantly, an experimental study of *Volapük*, *Pasilingua*, or *Spelin*, may safely be recommended. *Lingualumina* is a more serious matter. It is built on an exhaustive analysis of the notions that have to be expressed, and thus approaches nearer to the ideal which Leibniz had conceived of a perfect and universal language.

BRIDGE CONSTRUCTION.

A Practical Treatise on Bridge Construction: being a Text-book on the Design and Construction of Bridges in Iron and Steel. For the Use of Students, Draughtsmen, and Engineers. By T. Claxton Fidler, M.Inst. C.E. (London: Charles Griffin and Co., 1887.)

THIS book is principally intended for practical use by engineers and draughtsmen, who are now being called upon to design and construct bridges of unprecedented magnitude, like the Forth Bridge, which the introduction of iron, and latterly more especially of steel, has rendered possible. The execution of these requirements has brought forward a number of new problems to be solved in Statics, and the Elasticity and Strength of Materials, and has invested old problems with an importance which they did not before possess. Evolution in this branch of creation has gone on so rapidly that the Darwinian student of the "survival of the fittest" might turn to this book for striking exemplifications of his theories, which he would find in the classification of bridges, described and illustrated in the second section of the work. But while in the animate kingdom the mammoth animals have become extinct from insufficient mobility and relative strength to carry their own weight, the converse operation is observable in engineering construction. Bone and muscle are of the same strength as formerly, but the improved manufacture of steel has placed in the hands of the engineer a material with which he can safely attempt his mammoth creations; and should metallurgical science provide commercially for the engineer a new metal, as strong as, or stronger than, steel, but of less weight—say, aluminium—then we may expect to see still more marvellous developments in bridge building.

The bridge, on a large scale, resembles the mammoth or giant in requiring its whole strength to keep itself upright; and one of the most interesting theoretical questions discussed in the present treatise is the consideration of the maximum span possible with the material in hand—say, steel. When the span is large, the greatest economy in details must be practised, as the chief stress is due to the dead weight of the bridge, and not to the relatively insignificant weight of the moving load. Thus in the Forth Bridge a weight of 20,000 tons of steel is required in a single span to provide it with the necessary strength to hold itself up, so that the stresses due to a train of 200 tons running across may be left out of account.

The weight of metal worked into a bridge is at once a measure of the stresses in the material, and also of the quantity, and consequently the cost, of the materi al used. The author employs the customary units of engineers, the pound or ton as a measure of force and of weigh, and measures stresses in pounds or tons per square inch. He does not find it necessary to express his stresses in poundals per square foot, nor does he measure quantity of material in units of mass, which are *g* pounds or tons, as we are taught in theoretical text-books.

The mathematical student, to whom the book is partially addressed, will find it, while valuable as a handbook for a practical engineer, at the same time stimulating to his imagination in the realms of pure Abstract Mechanics, which at present run the risk of wandering away from reality, because the writers of modern text-books of mathematics do not look to the wonderful creations of modern engineering science for illustrations of theory. Thus the methods of Graphic Statics, largely employed in this treatise, arose out of the requirements of an engineer's office: a draughtsman was found using the method, and Prof. Maxwell seized upon it and elevated it to the rank of a new method in Mechanics.

Scientific treatises on Practical Mechanics are more common in America, where the requirements of opening up a vast continent have given great employment to the engineer and the bridge-builder; and it must be owned that these treatises are far superior to our own. But we hope the present treatise will do something to take away this reproach.

We may flatter ourselves that the Forth Bridge now in progress is the greatest thing of the kind in the world, but a rival in the Poughkeepsie Bridge is projected. These two bridges will exemplify the difference of practice of the Old World and the New. In our practice the whole bridge is riveted up into a rigid structure as much as possible; while in America the articulated system of triangular cells, with pin joints permitting rotation, is adopted, the stress in individual members being thus a simple pull or thrust. So far the American system has scored one in securing the contract for the Hawkesbury Bridge in Australia. This system affords the best theoretical illustrations of elementary Statics—the subject of Part I. of the present treatise—until the question of the bending moment (it is gratifying to find the term "tendency to break" of the abstract treatises discarded) comes into consideration, when the Old World bridge affords the requisite illustrations.

In Part III., on the "Strength of Materials," the author begins with the resistance of columns and struts to flexure, and here theory and practice have long worked together almost in harmony. The expression "breaking load" of a column—to mean the load which just starts flexure of the column—is apparently usual, but like the expression "tendency to break" should now be discarded for something more suitable. The theoretical strength of a column, according to Euler, which requires the assumption that the column is initially *perfectly* straight, and the actual strength against flexure, are represented in a diagram (p. 160); and the author has shown very ingeniously how the actual state of things encountered in practice can be imitated theoretically by a strut composed of two flanges of unequal elasticity (p. 163). Such a strut will begin to curve immediately as the load is gradually applied, and will thus represent very closely the actual behaviour of a continuous column, as great variations are found experimentally in the elasticity of iron or steel in specimens cut from one piece of metal (p. 167). When crushing or tearing takes place from continually applied pressure or tension, only empirical formulæ are suitable; but, as in actual structures the stress is kept by Board of Trade rules much below the elastic limit, the theoretical equations depending essentially on Hooke's law, that Tension and Extension are in the ratio of the Elasticity of the material, may be employed. Even with the low stresses permissible by law, Wöhler's researches on the fatigue of metals show that permanent deformation may keep on accumulating, and, in consequence, modern engineering practice is in some respects not so daring as formerly. Gordon's empirical rules (§ 124) (originally due to Tredgold) have been shown by Prof. J. H. Cotterill to rest on a theoretical basis, if the compression of the material due to the thrust previous to flexure is taken into account.

For very long spans, the only two rival methods of construction are the cantilever and the suspension principles, of which the Forth Bridge and the Brooklyn Bridge are the great respective examples. In the Cantilever method we build out equally on each side of a pier, so as always to preserve stable equilibrium, while in the suspension method the roadway is suspended from the chains or steel ropes. The chief drawbacks of the suspension principle, its defect of stiffness and great sensibility to changes of temperature, are shown by the author to be avoidable by the system of bracing in his "rigid suspension bridge" (Fig. 22).

The disastrous fall of the Tay Bridge Viaduct in a hurricane has forcibly redirected the attention of engineers to the importance of the theory of wind-pressure and wind-bracing (Chapter XXIV.), and now we may feel secure that in the new Tay Bridge of Mr. Barlow, as well as in all recent structures, ample allowance of strength is provided for against the effect of wind.

The book is copiously illustrated with excellent diagrams of real practice in the construction of bridges, based on the theories of the text, and should prove not only an indispensable hand-book of the practical engineer, but also a stimulating treatise to the student of mathematical mechanics and elasticity.

A. G. GREENHILL.

TWO FRENCH BOOKS.

Les Pygmées. Par A. de Quatrefages.

Les Ancêtres de nos Animaux, dans les Temps Géologiques. Par Albert Gaudry. (Paris : J. B. Baillière et Fils, 1887–88.)

THESE two works form two volumes of Baillière et Fils' "Bibliothèque Scientifique Contemporaine." The first, by the eminent Professor of Anthropology at the Jardin des Plantes at Paris, treats of the Pygmies, a diminutive race of mankind known to the ancients, alluded to by Homer, insisted upon as really existing by Aristotle, next believed to be but myths, and now established as a veritable race of the human kind. The author accepts for them the terms, suggested by Hamy, of Negritos and Negrilles, the latter being confined to the African Pygmies, and the former to those of the Asiatic Isles.

Avowedly a compilation, this little volume has all the peculiar charm that distinguishes Prof. Quatrefages' writings, and abounds with much curious and interesting details. The first chapter treats of the Pygmies from an historic point of view; the second, third, and fourth, of the Negritos, they being exclusively insular. The Negritos are to be found in New Guinea, and all over the Melanesian Archipelago, as far as Fiji; but, while the typical Negrito is confined to this area, conquest, emigration, and slavery have spread the race to Timor, Ceram, Bouro, Gilolo, to the western shores of Borneo, and so to other islands of the Pacific Ocean. Northwards they can be traced to the Carolines, and southwards to New Zealand where they preceded the Maoris. Mr. Ten Kate reports a Melanesian skull found in the little Isle of Santo Spiritu, off the coast of California. To the northwards they can be traced to the Loochoo Isles, Formosa, &c., while their western limits seem to be the Nicobar and Andaman Islands.

The question of the mixing of races on the borders of their distribution is discussed, and a good deal of recent information on this subject is given. The various modifications dependent on the wide range of distribution are also investigated, and the manners and habits of the several groups are described at some length. Good copies of photographs of native heads and figures are appended.

Chapter VI. treats of the Negrilles, or African Pygmies, the details of the Akkas, Tobbo and Chairallah, reared in Italy by Count Miniscalchi Erizzo being full of interest. The last chapter is devoted to the Bushmen of the Cape, and in connection with them there is an account of the Hottentots. The volume has thirty-one figures intercalated with the text.

The second work is by an equally well-known writer, —though of a very different school from that of Prof. Quatrefages—Prof. Albert Gaudry, also a Member of the Institute, and the Professor of Palæontology at the Museum. Well known for his able writings, and for his liberal and modern views on science, he has in this little volume given us a most delightful account of his ideas on the origin and development of the Mammalia during geological time. The volume begins with a chapter on the history of the progress of palæontology, followed by one on evolution and Darwinism. Though a disciple of D'Archiac, who was a strong opponent of Darwin's views, Prof. Gaudry read "The Origin of Species" with the most passionate admiration, and his labours since then have very materially helped to complete the palæontological record. The third chapter is devoted to the subject of the evolution of the Mammalia in geologic time; the fourth introduces us to the author's researches at Pikermi, where, as he tells us, he spent some of the most pleasurable moments of his life, engaged in excavating the remains of the quadrupeds which in times long ago roamed at liberty over the plains of Greece. Here were found an assemblage of animals of large size, such as has never been found before within so limited an area. Beautiful figures of many of these are given, and their relations to existing forms are explained. In another chapter we find an account of similar researches carried on at Léberon, near Cucuron (Vaucluse), where the remains were chiefly those of Herbivores, and an interesting table is added of the succession of the terrestrial Mammalia in France during the Tertiary period. In a concluding chapter there are some short sketches of the well-known palæontologists of the Museum: Alcide D'Orbigny, D'Archiac, Edouard Lartet, followed by a description of the fine new gallery for fossil forms at the Museum.

OUR BOOK SHELF.

The Elements of Graphical Arithmetic and Graphical Statics. By John Y. Gray and George Lowson, M.A. (London and Glasgow: W. Collins, Sons, and Co., 1888.)

IN the year 1871, Prof. Crofton, F.R.S., explained before the London Mathematical Society his diagrams illustrative of the stresses in Warren and lattice girders, and in the course of his remarks said that he had not found anything to help him in English text-books, and referred to papers by Profs. Rankine and Clerk-Maxwell. It was at this meeting (April 13) that Prof. Henrici drew attention to a work then little known in this country, viz. Culmann's "Graphische Statik"—"l'excellente 'Graphische Statik' de M. Culmann" (Prof. Cremona)—and showed that Prof. Crofton's constructions had been anticipated and the methods applied to a very wide range of subjects. On this occasion also Prof. Henrici illustrated the subject by a simple and ingenious notation. He subsequently drew up an abstract of Culmann's work (1866), which was printed in the Appendix to vol. iii. of the above-named Society's Proceedings (pp. 320–22). The work is now well known, and its methods are very generally employed by engineers, and are the subject of lectures in more than one of our Colleges.

The object of the book before us is to give an elementary account of the fundamental principles of the subject in a handy and cheap form, as well as to discuss some simple examples of their application.

The first part—which gives an explanation of graphical methods, illustrates graphical arithmetic, and shows how to represent areas and volumes by lines—is very carefully and clearly worked out, and leads one to see that this part of the subject might well come in at a fairly early date in school-work. Our idea is that the second part, "Graphical Statics," would be improved by more fullness of detail. It comprises an account of the following matters: kinematics, forces in one plane acting at a point, the funicular polygon, resolution of forces, moments, couples, bending moment and shearing force in a simple beam, rolling loads, framed structures, effects of wind-pressure on roofs, bridge-girders, and centres of gravity.

We have noted only two or three typographical errors. The notation employed is one most frequently termed "Bow's notation" in this book, from its having "been brought into use by Robert H. Bow, Esq., C.E.," but a note states that "the method seems, however, to have been first suggested by Prof. Henrici." We presume that Prof. Henrici's notation was the one we have referred to in the opening paragraphs of this notice. The immediate object of the book is to furnish help to students preparing for the South Kensington Examinations and for those of the City and Guilds of London Institute.

The Manual Training School. By C. M. Woodward. (Boston: D. C. Heath and Co., 1887.)

MR. WOODWARD has by no means a high opinion of the results of the efforts that have hitherto been made in European countries to promote technical education. In 1885 he spent five months in examining "trade schools" on this side of the Atlantic, and all the schools visited by him, with the exception of the French Government school at Châlons, disappointed him. He admits that they have "many excellent features"; but their manual training is generally, he holds, "very narrow," and he condemns "their long daily sessions, their long terms, and the conventional nature of their curricula." Manual training, according to Mr. Woodward, is in a much more flourishing condition in America. There it has been introduced "not for a trade or a profession, but for the healthy growth and vigour of all the faculties, for general robustness of life and character"; and he is of opinion that it has been developed in a way that places it "far in advance of any model in a foreign land." Whether or not this comparative estimate is accurate, no one who reads Mr. Woodward's book will dispute that the Americans have begun to understand thoroughly the importance of technical instruction, and that the leaders of opinion on the subject have done much to diffuse enlightened ideas as to the true aims and methods of manual training. Unfortunately, Mr. Woodward has not the art of presenting facts and arguments in an attractive style. He has, however, brought together a great mass of useful information about a subject of pressing importance, and his work, although relating chiefly to institutions founded in his own country, ought to find readers in England as well as in the United States. He does not enter, in detail, into the theory and practice of manual training in primary and grammar schools. He limits himself to the training of pupils beyond the age of fourteen. The value of the work is increased by a number of good woodcuts illustrating shop exercises in woods and metals.

The Method of Creation. By Henry W. Crosskey. (London: The Sunday School Association, 1888.)

THIS little volume belongs to a series of "Biblical Manuals," edited by Prof. J. Estlin Carpenter. With the polemical parts of the book we have, of course, nothing to do. In the chapters in which Mr. Crosskey devotes himself simply to the exposition of scientific truths he writes with full knowledge of his subject and in a clear and pleasant style. "How 'dry land' was formed" is the subject of an excellent chapter, in which the writer brings together some of the more striking of the facts which prove that rocks have been formed by various agencies, that there is no single period at which any kind of rock has been specially produced, that the crust of the earth consists of rocks in ordered succession, and that there has been an unvarying order in the succession of rocks. There are also good chapters on the history of plants and animals, and on the antiquity of the human race.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

"Coral Formations."

DR. GUPPY's letter shows that I have not been sufficiently explicit on the subject of the formation of atolls, yet I cannot well understand that I have been obscure on the subject of his first question. Surely it is a sufficient reason for rejecting the theory of subsidence as applied to the Chagos Group that I fancy myself, in conjunction with M. Spurs, to have detected evidences of elevation in Diego Garcia. Darwin laid great stress on the character of the Great Chagos Bank as affording evidence of his theory of subsidence; he considers it to be an atoll drowned by a too rapid act of subsidence; but, as I have pointed out, if this were so it is impossible to understand how two atolls such as the Great Chagos Bank and Centurion's Bank could have been thus destroyed without Six Islands or Egmont's Atoll, which lies directly between them, being involved in their destruction. Further, the raised atolls north of Madagascar are unquestionable proofs of upheaval in this region, yet in the same region are low-lying atolls, atoll-shaped reefs awash, and submerged atoll-shaped banks. Clearly the theory of subsidence does not apply to these groups, and I do not see any reason for supposing that the Laccadive and Maldive Islands have been formed differently to the other atolls in the Indian Ocean, though I am unable to bring forward any fresh arguments with regard to them.

Secondly, because I do not agree with Mr. Murray in thinking that lagoons are due largely to the solvent action of sea-water, it is no reason that I should disagree with other parts of his theory. Indeed, after Dr. Guppy's striking observations at Santa Anna and other islands, it would be idle to deny that organic deposits have formed the bases of many atolls, perhaps of all. It did not seem to me necessary to deal with this part of the subject, because as a resident on an atoll without the means of making sectional soundings I had nothing new to say on the subject.

Perhaps you will allow me space to add that before reading my paper I had not had the advantage of meeting Mr. Murray. I have since had that advantage, and on comparing notes with him I find that I am much more in accord with him than my paper would seem to show. I still maintain my point that the rate of organic growth in the lagoon of Diego Garcia is sufficient to counterbalance the solvent action of the sea-water. In other points I agree with him, and believe that my observations confirm his view that atolls tend to spread outwards like a fairy-ring. Mr. Murray has convinced me that I laid undue stress on the direct influence of currents in determining the growth of corals, and this section of my paper was in consequence omitted in the account which appeared in the columns of NATURE. Judging from the local effects which I observed at Diego Garcia, where currents often swept through narrow channels with great force, and from Prof. Moseley's account of the oceanic currents sweeping past St. Paul's rocks, I was led to an exaggerated estimate of the rate of oceanic currents. No doubt a current running at the rate of some thirty-five miles in the day would modify or retard coral growth, but such currents are only found in narrow passages. G. C. BOURNE.

I LATELY discussed Murray's theory of coral formation with a class of boys and girls (fourteen to sixteen years of age), and *they* raised two questions which I am unable to answer. (1) If sea water dissolves the coral near the surface at such a rate as to form a lagoon, why does it not dissolve the limestone foundation even more rapidly? (2) After a reef has progressed a considerable distance from the shore, and a channel of open water is formed between, why should not the reef extend back again shorewards? How could such a channel as exists between Australia and its Great Barrier Reef ever have been kept open? These seem to be valid and serious objections: will some expert be kind enough to answer them? CHARLES R. DRYER.

Fort Wayne, Indiana, U.S.A., April 16.

Density and Specific Gravity.

THE point raised by Mr. Cumming in last week's NATURE (vol. xxxvii. p. 584), as to the use of the words density and specific gravity is, it seems to me, of some importance. For many years past I have, in my lectures, taken the law into my own hands in this matter, and, defining density as the mass of unit volume, I have defined specific gravity, in the way Mr. Cumming suggests in the last paragraph of his letter, as the weight of unit volume (or rather, lest I should cause any to offend against the examiner, I have thus defined *absolute* specific gravity, or specific gravity proper, and have pointed out that the definition commonly given was the definition of *relative* specific gravity). We thus get the parallel relations—

$$M = \rho V \text{ and } W = sV,$$

also

$$W = gM \text{ and } s = g\rho.$$

Thus regarded, specific gravity is to density just what weight is to mass. When force is expressed in absolute units of any kind, specific gravity and density must of course have different numerical values, just as weight and mass have. But in the very large number of cases in which weights are the only forces that have to be considered, and in which it is not needful to take account of the small changes of weight dependent on changes of geographical position, the local weight of the unit of mass may be conveniently taken as the practical unit of force—that is, we may take $g = 1$. In all such cases we have, *numerically*, weight = mass, and specific gravity = density, though the idea of weight is essentially different from that of mass, and the idea of specific gravity from that of density.

Of course, as Mr. Cumming points out, when specific gravity is defined as weight of unit volume, its numerical value for a given substance depends on what is taken as unit of weight and what as unit of volume. With the weight of 1 pound avoirdupois and the cubic foot as units, the specific gravity of water becomes 62·5, and that of platinum 1312·5, instead of 1 and 21 as given in the ordinary tables of (relative) specific gravities. If, on the other hand, we take as unit of weight the weight of unit volume of the standard substance, as is done when weights are expressed in grammes and volumes in cubic centimetres, or weights in kilogrammes and volumes in litres, absolute specific gravities and relative specific gravities become equal, and the ordinary specific gravity tables can be used for practical purposes, which is *one* of the great advantages to be gained by using the metrical system of weights and measures. With any other system, the numbers given in the tables require to be multiplied by the specific gravity of water—that is, they must be translated into absolute specific gravities—before they are of use for almost any real calculation, such as occurs either in experimental physics or in engineering practice. For instance, we weigh a measured length of copper wire and want to know its diameter, or we weigh the quantity of mercury that fills a glass bulb of which we require the capacity, or that fills a measured length of a tube of which we require the bore; or an engineer compares his pressure-gauge against a mercury-manometer in order to convert its indications into pounds-weight per square inch; or he has to calculate the pressure exerted by a brick wall so many feet high, or the weight of a mass of rock of so many cubic feet. In all these cases it is the absolute specific gravity that comes into account; it is no use to tell us that copper is 8·9 times as heavy as water, and mercury 13·6 times as heavy, unless we are told how heavy the unit volume of water itself is.

I maintain, in short, that the weight of unit volume of a substance is a quantity of very great practical importance, for which specific gravity is a very suitable name, whereas the ratio usually defined as specific gravity is of little or no use outside examination questions, and that if it needs a name it should be called relative density.

Further, my experience is that the definition here advocated presents considerable advantages from the point of view of systematic teaching. G. CAREY FOSTER.

University College, London, April 21.

JE crois que la notion de *specific gravity* donnée par M. Cumming dans NATURE du 19 avril (vol. xxxvii. p. 584) est de nature à puzzler les étudiants plus encore que la *vraie* définition physique de la densité.

La densité d'un corps est le rapport de sa masse à son volume—

$$\rho = \frac{M}{V}.$$

Dans le système C.G.S. la densité doit donc être exprimée en *grammes masse par centimètre cube* (voy. Everett, "Units and Physical Constants"). Le poids spécifique est le rapport du poids d'un corps à son volume et devrait être exprimé, dans le système C.G.S. en *dynes par centimètre cube*. Mais il y aurait alors le grave inconvénient pratique à cette définition *rigoureuse* que le poids spécifique varierait avec *g*, accélération due à la pesanteur, tandis que la densité resterait constante.

La confusion provient de ce que le mot *weight*, comme le mot *poids* en français, s'applique indistinctement à la *masse* d'un corps en grammes-masse et à la force qu'exerce la pesanteur sur le corps exprimée en grammes.

La solution logique est de supprimer le mot *poids* du langage, à cause de son double sens, et de ne parler que de la *masse* ou de la *force* exercée par la pesanteur, suivant que l'un ou l'autre facteur intervient dans les calculs.

En tout cas, exprimer le poids spécifique en livres ou en grammes est aussi absurde que d'exprimer les vitesses en mètres, et la puissance (*power*) d'une machine en ergs ou en *foot-pounds*. Le respect de l'homogénéité des formules est la condition essentielle des définitions des quantités physiques, et cette homogénéité n'est pas respectée dans la définition donnée par M. Cumming. E. HOSPITALIER.

Paris, le 23 avril.

The Ignition of Platinum in Different Gases.

AN abstract appeared a few weeks ago in NATURE relating to the "Occlusion of Gases by Platinum and their Expulsion by Ignition," which induces me to mention some curious results obtained by Mr. Lowndes and myself by the ignition of platinum in different gases. We were led to the experiments by another investigation on the behaviour of carbon at high temperatures in various gases. We find that when a platinum wire is heated to nearly melting by a current in an atmosphere of chlorine, the walls of the glass vessel become covered with a yellow deposit, which is insoluble in water, but dissolves in hydrochloric acid, and then, after addition of a little nitric acid, gives all the reactions of platinic chloride. The yellow deposit is in fact platinous chloride. At the same time the thick part of the platinum wire conveying the current, and which was not heated very highly, became incrusted with very fine long crystals of platinum. Some of these were more than the sixteenth of an inch in length, and apparently considerably more were located on that end of the thick wire leading to the negative pole than on the other.

There was also a very decided but lambent flame playing around the ignited and part of the cooler wire during the passage of the current. The arrangement used was a wide-necked flask, stopped with a glass bulb, through which a delivery-tube for the chlorine, and the two No. 12 platinum wires leading the current, passed. The ignited parts of the wire are little coils of No. 24 wire separated by a 1-inch piece of No. 12. On heating the flask externally up to the softening of the glass, the appearance of a flame around the wire increased slightly.

On repeating the experiment with bromine, very nearly the same effects were observed. The amount of platinous bromide was much less than in the case of the chloride, but the flame appearance was very much more pronounced. On passing chlorine into the bromine, so as to form chloride of bromine, both the flame appearance and the action on the platinum were largely increased. With iodine in the flask, vaporized by heating externally, little chemical action on the platinum was observed, only the slightest deposit being formed of a platinum-iodine compound on the glass; but, on passing chlorine into this also, a still more vigorous action on the metal took place, the deposit containing only chlorine and platinum. The flame appearance filled the entire flask. The spectrum of these flames shows no lines in any case. They are all continuous. The largest crystals of platinum were obtained with the ICl_3. Bromide of iodine behaved like iodine.

We have tried a number of other substances in a similar manner. Oxygen, sulphur, sulphur dioxide, nitric oxide, mercury vapour gave negative results as far as we could see. With hydrochloric acid some $PtCl_2$ was formed, but no flame appearance. Phosphoric chloride gave a slight flame, and some $PtCl_2$; but phosphorus is liberated, and then unites with the platinum, melting it. A current of very dry hydrogen fluoride was passed through the flask; before the wire was ignited no action on the glass of the flask was apparent, but almost immediately on passing the current the glass became much corroded by, probably, liberated fluorine. Owing to the flask breaking, we cannot say if platinous fluoride was formed.

With silicon fluoride a singular action took place, the wire, especially the negative half, becoming covered with long semi-transparent crystals of, we think, silicon. The silicon fluoride was very dry, and passed for a long time through the flask without any action until the wire was ignited, when simultaneously with the production of these crystals the glass vessel became much corroded. A small quantity of a soluble platinum salt was formed at the same time. We are continuing these experiments.

We do not think the platinum salts formed in this way are simply shot out by "volcanic" action, as they are quite uniformly spread over the sides of the glass vessel, and seem to be really volatile at the temperature and under the conditions. We have failed to find any record of platinum salts being volatile when heated under ordinary conditions, but it is probable that in the presence of free halogen they would be volatile.

Whether there be any true electrolytic action in these cases we are not at the moment prepared to say.

Royal Military Academy. W. R. HODGKINSON.

"The Nervous System and the Mind."

WILL you allow me to account for one or two of the discrepancies in my book which your very able reviewer points out in the current issue of NATURE?

He cannot reconcile the statement that "everyone nowadays admits that the evolution of mind and the evolution of the nervous system have proceeded *pari passu*, and are indeed but two aspects of the same process," with the further statement that "this way of studying them is so greatly neglected, is indeed derided and scouted." It is pointed out, however, in the passage from which he quotes, that the latter charge is laid at the door of my brother alienists only; while the former statement applies to psychologists at large.

Were it worth while, I could substantiate my charge by chapter and verse, but as the general movement is at last beginning in the direction I advocate, to do so would be to cause the cry from the wilderness to approximate too much to the character of the voice of chanticleer.

Your reviewer states, as if in controversion of my doctrine, that "experienced alienists tell us they find it necessary to admit a moral insanity with an average amount of intelligence." This I have never denied. My position is not that in "moral insanity" intelligence is deficient in amount. What I say is, that in "moral insanity" intelligence is always *disordered.* Disorder of intelligence is very different from deficiency of intelligence. CHAS. MERCIER.

Catford, S.E., April 23.

I AM glad that Dr. Mercier has found so little to complain of in the review of his recent work. I am bound to accept his explanation of the discrepancy I ventured to point out, although, on re-reading the two apparently antagonistic passages again, I do not find the distinction between psychologists and alienists, to which he now refers, clearly stated. The expression "everyone" (p. 4) appears to include both. Dr. Mercier's "brother alienists" are, it seems, excluded from the class that can grasp the truth that the evolution of mind and the nervous system are but two aspects of the same process, and belong to that uninformed class that "deride and scout" it. I certainly should have hesitated to understand this to be the author's meaning, but, being so, I must leave his benighted *confrères* to settle their account with him. They may perchance think that in this reading of the passage, "the voice of chanticleer" has already become associated with the *vox clamantis* in the wilderness!

In regard to the association of moral insanity with an average amount of intellect, I would only observe that the brother alienists of Dr. Mercier, including Dr. Maudsley, contend that, not only may this be met with, but that moral insanity may co-exist with an undisordered intelligence. Dr. Mercier's contention that "in moral insanity intelligence is always disordered" would therefore be still in conflict with the experience of some experienced alienists, which was the position I took.

Both these points, however, are only small matters compared with the general subject-matter of the work under review, and I repeat that it is gratifying to find there does not appear to have been any important mis-statement of Dr. Mercier's views in the friendly criticism of THE REVIEWER.

April 24.

Nose-Blackening as Preventive of Snow-Blindness.

MY friend Mr. Edmund J. Power sends me the following account of what appears to me to be an interesting fact. I should like to obtain suggestions from physiologists as to the possible explanation of the phenomenon, on the assumption that the blackening of the nose and eyelids really does prevent the injurious action of sunlight on the eyes; and further, I should like to know whether (quite apart from the fact of its utility or futility) the custom has possibly a remote origin in some ceremony or ritual. E. RAY LANKESTER.

"Can you or some of your friends explain the following?

"When in Colorado shooting the end of last year, my friend had a very bad attack of snow-blindness, caused by a long march on snow with bright sun. My eyes also were very bad the next day and caused much pain.

"Some days after I was under similar circumstances, when my guide stopped, and taking some burnt wood from a stump blackened his nose and under the eyes well down on the cheek-bone.

"On asking him the reason, he told me it stopped snow-blindness, and as the glare was very strong I did the same, and found immediate relief.

"I did this all the time I was out, and never found the snow affect my eyes in any way.

"Everyone I spoke to about it could give no reason for it, but all used it on the march. Some use glasses, but, as my man remarked, 'glasses cost dollars, dirt nothing.'

"Perhaps some of your friends can enlarge on the subject, as it is of great interest to me, and may be so to Alpine people, as glasses are hot to climb in, and from my own experience it is not easy to stalk in glasses and then take them off and shoot."

"Antagonism."

THE author of "The Correlation of the Physical Forces" has, I am sure, our sympathy when he relates how he has been forestalled by Prof. Huxley.

As Sir William Grove subsequently says that "it is always useful to know the truth," he will, perhaps, excuse my suggesting that his views upon antagonism as pervading the universe have been anticipated in a work published more than a quarter of a century ago. I allude to "First Principles," and more especially to the chapter in it upon "The Rhythm of Motion," in which the effects of antagonist forces are shown to be everywhere present, and are copiously illustrated and expounded from the stand-points of astronomy, geology, biology, psychology, and sociology. After reading this chapter, and especially its concluding sentence—"Given the co-existence everywhere of antagonist forces, a postulate which, as we have seen, is necessitated by the form of our experience"—we cannot, I think, but add another eminent name to that of Prof. Huxley as anticipating Sir W. Grove: it is that of Mr. Herbert Spencer.

F. HOWARD COLLINS.

Churchfield, Edgbaston, April 29.

Sense of Taste.

THE curious difference between male and female observers in detecting feeble traces of quinine, sugar, acid, &c., in water as mentioned in NATURE on p. 557 (vol. xxxvii.), is possibly owing to the sense of taste being injured in the males by the use of tobacco.

I have had occasion to apply delicate tests of smell and taste, and I find that even moderate smokers are unable to detect odours and tastes that are quite distinct to non-smokers.

Dunstable. W. G. S.

SUGGESTIONS ON THE CLASSIFICATION OF THE VARIOUS SPECIES OF HEAVENLY BODIES.[1]

III.

III.—SUB-GROUPS AND SPECIES OF GROUP I.

I. SUB-GROUP. NEBULÆ.

HAVING, in the preceding part of this memoir, attempted to give a general idea of that grouping of celestial bodies which in my opinion best accords with our present knowledge, and which has been based upon the assumed meteoric origin of all of them, I now proceed to test the hypothesis further by showing how it bears the strain put upon it when, in addition to furnishing us with a general grouping, it is used to indicate how the groups should be still further divided, and what specific differences may be expected.

The presence or absence of carbon will divide this group into two main sub-groups.

The first will contain the nebulæ, in which only the spectrum of the meteoric constituents is observed with or without the spectrum of hydrogen added.

It will also contain those bodies in which the nebula spectrum gets almost masked by a continuous one, such as Comets 1866 and 1867, and the great nebula in Andromeda.

In the second sub-groups will be more condensed swarms still, in which, one by one, new lines are added to the spectra, and carbon makes its appearance; while probably the last species in this sub-group would be bodies represented by γ Cassiopeiæ.

Species of Nebulæ.

I have elsewhere referred to the extreme difficulty of the spectroscopic discrimination in the case of the meteor-swarms which are just passing from the first stage of condensation, and it may well be that we shall have to wait for many years before a true spectroscopic classification of the various aggregations which I have indicated, can be made.

It is clear, then, from what has gone before that in each stage of evolution there will be very various surfaces and loci of collisions in certain parts of all the swarms, and we have already seen that even in the nebulosities discovered by Sir Wm. Herschel, which represent possibly a very inchoate condition, there are bright portions here and there.

If the conditions are such in the highly elaborated swarms and in the nebulosity that the number of collisions in any region per cubic million miles is identical, the spectroscope will give us the same result. In the classification of the nebulæ, therefore, the spectroscope must cede to the telescope when the dynamical laws, which must influence the interior movements of meteoric swarms, have been fully worked out. The spectroscope, however, is certainly at one with the telescope in pointing out that so-called planetary nebulæ are among the very earliest forms—those in which the collisions are most restricted in the colliding regions. The colour of these bodies is blue tinged with green; they do not appear to have that milkiness which generally attaches to nebulæ, and the bright nebulous lines are seen in some cases absolutely without any trace of continuous spectrum. In higher stages the continuous spectrum comes in, and in higher stages still possibly also the bands of carbon; for in many cases Dr. Huggins in his important observations has recorded the weakness of the spectrum in the red, or in other words the strengthening of the spectrum in the green and blue exactly where the carbon bands lie.

But in all the bodies of Group I. which possess forms visible to us in the telescope, it would seem proper that their classification should depend mainly—at present at all events—upon their telescopic appearance, and there is very little doubt that a few years' labour with the new point of view in the mind of observers armed with sufficient optical power, will enable us to make a tremendous stride in this direction; but it seems already that this must not be done without spectroscopic aid. For instance, if what I have previously suggested as to the possible origin of the planetary nebulæ be accepted, it is clear that in those which give us the purest spectrum of lines, one in which there is the minimum of continuous spectrum, we find the starting-point of the combined telescopic and spectroscopic classification, and the line to be followed will be that in which, *cæteris paribus*, we get proofs of more and more condensation, and therefore more and more collisions, and therefore higher and higher temperatures, and therefore greater complexity in the spectrum until at length true stars are reached.

When true stars are reached those of the cluster appear nebulous in the telescope in consequence of its distance; the spectroscope must give us indications by absorption.

It is not necessary in this connection, therefore, to refer to undoubted star clusters, as the presence of absorption will place them in another group; but the remark may be made that it is not likely that future research will indicate that new groupings of stars, such as Sir Wm. Herschel suggests in his paper on the breaking up of the Milky Way, will differ in any essential particular from the successive groupings of meteorites which are watched in the nebulæ. Space and gravitation being as they are, it is not necessary to assume that any difference of kind need exist in the method of grouping formed stars and meteoric dust; indeed there is much evidence to the contrary.

II. SUB-GROUP. BRIGHT-LINE STARS.

It might appear at first sight that the distribution of bright-line stars among various species should be very easy, since a constant rise of temperature should bring out more and more lines, so that the species might be based upon complexity of spectrum merely.

But this is not so, for the reason that the few observations already recorded, although they point to the existence of carbon bands, do not enable us to say exactly how far the masking process is valid. Hence in the present communication I content myself by giving some details relating to maskings, and the results of the discussions, so far as they have gone, in the case of each star. I shall return to the line of evolution in a later paper.

Masking of Radiation Effects produced by Variations of Interspacing.

I have already stated that carbon bands are apt to mask the appearance of other spectral phenomena in the region of the spectrum in which they lie. In this way we can not only account for the apparent absence of the first manganese fluting, while the second one is visible, but it is even possible to use this method to determine which bands of carbon are actually present. There is another kind of masking effect produced in a different way, and this shows itself in connection with sodium. It is well known that when the temperature is low, D is seen alone, and if seen in connection with continuous spectrum the continuous spectrum is crossed by either dark or bright D, according to the existing circumstances.

I showed some years ago that the green line of sodium, not the red one, is really visible when sodium is burned in the bunsen burner. It is, however, very much brighter when higher temperatures are used, although when bright it does not absorb in the way the line D does.

Now, if we imagine a swarm of meteorites such that in the line of sight the areas of meteorite and interspace are

[1] The Bakerian Lecture, delivered at the Royal Society on April 12, by J. Norman Lockyer, F.R.S. Continued from vol. xxxvii. p. 609.

equal, half the area will show D absorbed, and the other half D bright; and in the resulting spectrum D will have disappeared, on account of the equality, or nearly equality, of the radiation added to the absorption of the continuous spectrum. The light from the interspace just fills up and obliterates the absorption.

But if the temperature is such that the green line is seen as well as D; in consequence of its poor absorbing effect there will be no dark line corresponding to it in the resulting spectrum, but the bright green line from the interspace will be superposed on the continuous spectrum, and we shall get the apparently paradoxical result of the green line of sodium visible while D is absent. This condition can easily be reproduced in the laboratory by volatilizing a small piece of sodium—between the poles of an electric lamp. The green line will be seen bright, while D is very dark.

In the bodies in which these phenomena apparently occur—for so far I have found no other origin for the lines recorded 569, 570, and 571—the wave-length of the green sodium line being 5687, such as Wolf and Rayet's three stars in Cygnus and in γ Argûs, the continuous variability of D is one of the facts most clearly demonstrated by the observations, and it is obvious that this should follow if from any cause any variation takes place in the distance between the meteorites.

In all meteoric glows which have been observed in the laboratory, not only D but the green line have been seen constantly bright, while we know in Comet Wells most of the luminosity at a certain stage of the comet's history was produced by sodium. It is therefore extremely probable that the view above put forward must be taken as an explanation of the absence of D when not seen, rather than an abnormal chemical constitution of the meteorites—that is to say, one in which sodium is absent. This may even explain the fact that up to the present time the D line of sodium has not been recorded in the spectrum of any nebula.[1]

Detailed Discussion of the Spectra of some Bright-Line Stars.

These things then being premised, I now submit some maps illustrating this part of the inquiry, although it will be some time before my investigations on the bright-line stars are finished. These maps will indicate the way in which the problem is being attacked, and the results already obtained. To help us in the work we have first of all those lines of substances known to exist in meteorites which are visible at the lowest temperatures which we can command in the laboratory. We have also the results of the carbon work to which reference was made in the previous paper; and then we have the lines which have been seen, although their wave-lengths have in no case been absolutely determined, in consequence of the extreme difficulty of the observation, both in stars and in comets, which I hold to be almost identical in structure.

In the case of each star the lines which have been recorded in its spectrum are plotted in the way indicated in the maps. The general result is that when we take into account the low temperature radiation, which we learn from the laboratory work, not only can we account for the existence of the lines which have been observed, but apparent absorptions in many cases are shown to be coincident with the part of the spectrum in front of a bright carbon fluting.

[1] In the lecture the author here referred to the spectrum of *o* Ceti, as photographed by Prof. Pickering for the Henry Draper Memorial, the slide having been kindly placed at his disposal by the Council of the Royal Astronomical Society. All the bright hydrogen lines in the violet and ultra-violet are shown in the photograph; with the exception of the one which is nearly coincident with H. The apparent absence of this line is in all probability due to the masking effect of the absorption-line of calcium. In this case, then, it appears that the calcium vapour is outside the hot hydrogen, and this therefore was being given off by the meteorites at the time.

A continuation of this line of thought shows us also that, when in these stars the spectrum is seen far into the blue, the luminosity really proceeds first from the carbon fluting, and in the hotter stars, from the hydrocarbon one in addition, which is still more refrangible. In the stars which have been examined so far, the dark parts of the spectrum, which at first sight appear due to absorption, are shown to be most likely caused by the gap in the radiation in that part of the spectrum where there is no continuous spectrum from the meteorites, and no bright band of carbon.

All the observations, it would appear, can be explained on the assumption of low temperature.

Notes on the Maps.

Lalande 13412.—Both Vogel and Pickering have observed the spectrum of this star and have measured the wave-lengths of the bright lines.

Vogel gives a sketch of the spectrum as well as a list of wave-lengths.

Vogel mentions a dark band at the blue end of the spectrum, and gives the wave-length in his sketch as from 486 to 473.

Both observers measure the bright 486 hydrogen (F) line.

Vogel measures a bright line at 540, while Pickering's measure is 545; but Pickering in another star, Arg.-Oeltzen 17681, has measured this line at 540, so there can be little doubt that is the correct wave-length.

Vogel measures a line at 581, but this has not been noticed by Pickering.

The bright part of the spectrum extending from 473 towards the blue with its maximum at 468 is, I would suggest, the carbon band appearing beyond the continuous spectrum, the rest of the carbon being cut out by the continuous spectrum, although 564 asserts itself by a brightening of the spectrum at that wave-length in Vogel's sketch, and by a rise in his light-curve.

The line at 540 is the only line of manganese visible at the temperature of the bunsen burner, while the 581 measurement of Vogel is in all probability the 579 line, the strongest line of iron visible at low temperatures.

In this star therefore we have continuous spectrum from the meteorites, and carbon bands, one of them appearing beyond the continuous spectrum in the blue as a bright band; bright lines of hydrogen, manganese, and iron being superposed on both. There is no absorption of any kind, the apparent dark band being due to defect of radiation.

Vogel's results are given in the *Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 17.

Pickering's are published in the *Astronomische Nachrichten*, No. 2376; *Science*, No. 41; and quoted in *Copernicus*, vol. i. p. 140.

2nd Cygnus.—B.D. + 35°, No. 4013.—Messrs. Wolf and Rayet, in 1867, first observed the spectrum of this star, and measured the positions of the bright lines. Micrometer readings and reference lines are given by them from which a wave-length curve has been constructed. The wave-lengths of the bright lines in the star thus ascertained are: 581 (γ), 573 (β), 540 (δ), and 470 (α); the relative intensities being shown by the Greek letters.

"La ligne β est suivie d'un espace obscur; un autre espace très-sombre précède α."

Vogel afterwards examined the spectrum, measured the positions and ascertained the wave-lengths of the bright lines, drew a sketch of the spectrum as it appeared to him, and a curve showing the variation of intensity of the light throughout the spectrum.

The wave-lengths given by Vogel are 582 and 570, and of a band with its brightest part at 464, fading off in both directions and according to the sketch having its red limit at 473. In the light curve Vogel not only shows the 582 and 570 lines, but al-o bright lines in positions which by a curve have been found to correspond to wave-lengths 540 and 636. Vogel indicates in his sketch a dark band extending from 486 to the bright band 473, and an apparent absorption on the blue side of the 570 line, this absorption being ended at 564. These two bands agree in position with the dark spaces observed by Messrs. Wolf and Rayet. The bright band in the blue at 473 is most probably the carbon band appearing bright upon a faint continuous spectrum, this producing the apparent absorption from 486 to 473. If the bright carbon really

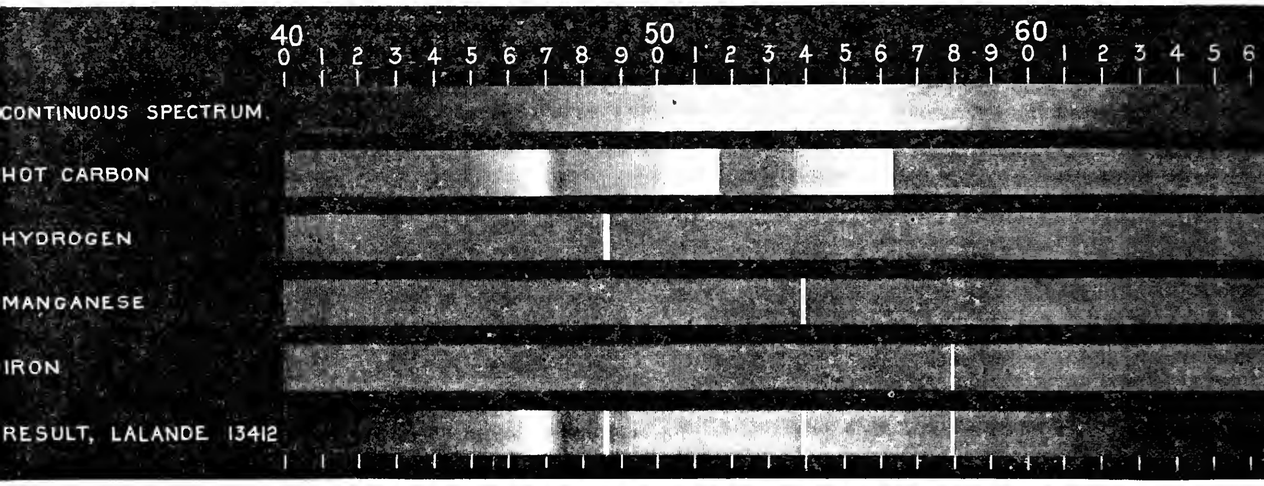


FIG. 4.—Map showing the probable origin of the spectrum of Lalande 13412.

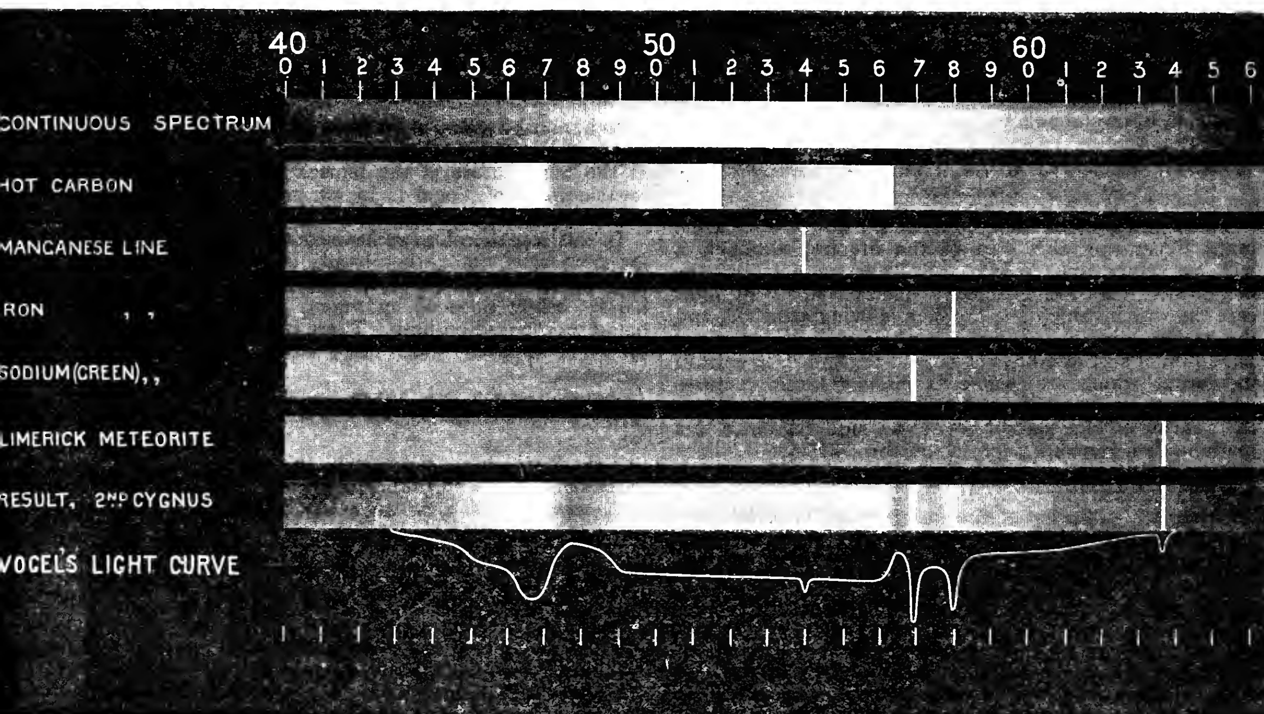


FIG. 5.—Map showing the probable or'gin of the spectrum of Wolf and Rayet's 2nd star in Cygnus.

accounts for the appearance of a dark band between the bright 570 and 564 in this star, all the apparent absorption is explained as due to contrast of bright bands on a fainter continuous spectrum due to red-hot meteorites.

The line at 540 is the only line of manganese visible in the bunsen burner, and the 580 line is the strongest low-temperature iron line. The 570 line is most probably the green sodium line 569, the absence of the yellow sodium being explained by the half-and-half absorption and radiation mentioned in the discussion of the causes which mask and prevent the appearance of the lines in a spectrum.

The line at 636 is in the red just at the end of the continuous spectrum, and as yet no origin has been found for it, although it has been observed as a bright line in the Limerick meteorite at the temperature of the oxyhydrogen blow-pipe.

This star therefore gives a continuous spectrum due to radiation from meteorites, and on this we get bright carbon (with one carbon band appearing separate in the blue), with bright lines of iron, manganese, sodium, and some as yet undetermined substance giving a line at 636 in the oxyhydrogen blow-pipe.

Wolf and Rayet's results are given in the *Comptes rendus*, vol. lxv. p. 292.

Dr. Vogel's are from the *Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 19.

The above are only given as examples of the seven bright-line stars explained in the lecture.

(*To be continued.*)

THE ROYAL SOCIETY SELECTED CANDIDATES.

THE following fifteen candidates were selected on Thursday last by the Council of the Royal Society to be recommended for election into the Society. The ballot will take place on June 7, at 4 p.m. We print with the name of each candidate the statement of his qualifications:—

THOMAS ANDREWS, F.R.S.E.,

F.C.S., Assoc.M.Inst.C.E. Ironmaster and Metallurgist. Awarded by the Institution of Civil Engineers, for original metallurgical and physical researches, a Telford Medal and a Telford Premium, Session 1884; again a Telford Premium, Session 1885: and another Telford Premium, Session 1886. Author of the following eighteen papers:—In Proc. Roy. Soc. Lond. (four papers), "Electromotive Force from difference of Salinity in Tidal Streams," "Action of Tidal Streams on Metals during diffusion of Salt and Fresh Water," "Reversals of Electromotive Force between Metals of High Temperatures in Fused Salts," "Observations on Pure Ice and Snow" (a determination of their relative conductivity for heat, and the great contraction of ice at extremely low temperatures, &c.); Trans. and Proc. Roy. Soc. Edin. (four papers), "On Relative Electro-chemical Positions of Iron, Steels, and Metals in Sea Water," "Apparent Lines of Force on passing a Current through Water," "Resistance of Fused Halogen Salts," "Electromotive Force between Metals at High Temperatures"; Proc. Inst. Civ. Eng. (four papers), "On Galvanic Action between Metals long exposed in Sea Water," "Corrosion of Metals long exposed in Sea Water." Author of an investigation on "Effects of Temperature on Strength of Railway Axles," Part I., II., and III., conducted by the author at a cost of nearly £800, to determine on a large scale the resistance of metals to a sudden concussion at varying temperatures down to zero F. Author also of papers "On Variations of Composition of River Waters" (Chem. Soc., 1875), and "On Curious Concretion Balls from Colliery Mineral Waters" (Brit. Assoc. Rep., Chemical Section, 1879), and "On Strength of Wrought Iron Railway Axles" (Trans. Soc. Eng., 1879; a premium of books awarded for this paper). At present engaged on a research "On some Novel Magneto-Chemical Effects on Magnetizing Iron," and "On the Construction of Iron, Steels, and Cast Metals at Low Temperatures, −50° F.," and "On the Viscosity of Pure Ice at −50° F., &c."

JAMES THOMSON BOTTOMLEY, M.A.,

Demonstrator of Experimental Physics in the University of Glasgow. After being several years with Dr. Andrews in Belfast, as pupil, and as assistant afterwards, he acted as Demonstrator in Chemistry in King's College, London, under Dr. W. A. Miller, and subsequently as Demonstrator and Lecturer in Natural Science, under Prof. W. G. Adams, till 1870, when he came to his present post in the University of Glasgow. Author of "Dynamics," for the Science and Art Department; "Hydrostatics," ditto; "Mathematical Tables for Physical Calculations;" Essay on the Progress of Science since 1833 ("Conversations-Lexicon"); all the articles on Electricity and Magnetism in Moxon's "Dictionary of Science." Also of many scientific articles describing his own experimental researches, including "Thermal Conductivity of Water" (Phil. Trans., 1881); "Permanent Temperature of Conductors, &c." (Proc. Roy. Soc. Edin.), &c.

CHARLES VERNON BOYS,

A.R.S.M. Demonstrator of Physics, Normal School of Science and Royal School of Mines. Author and joint-author of the following:—"Magneto-Electric Induction" (Proc. Phys. Soc., 1879 and 1880); "An Integrating Machine" (Proc. Phys. Soc., 1881); "Integrating and other Apparatus for the Measurement of Mechanical and Electrical Forces" (Proc. Phys. Soc., 1882); "Apparatus for Calculating Efficiency" (Proc. Phys. Soc., 1882); "Measurement of Curvature and Refractive Index" (Proc. Phys. Soc., 1882); "Vibrating Electric Meter" (Proc. Roy. Inst. 1883); "New Driving Gear" (Soc. Art. Lect., 1884); and other papers.

ARTHUR HERBERT CHURCH, M.A. (Oxon.),

F.C.S., F.I.C. Professor of Chemistry in the Royal Academy of Arts. Sometime Professor of Chemistry in the Royal Agricultural College, Cirencester. Researches in Animal, Vegetable, and Mineral Chemistry, *e.g.* Turacin, an animal pigment containing copper (Phil. Trans., 1869); Colein, the pigment of *Coleus Verschaffeltii* (Journ. Chem. Soc., 1877); Aluminium in certain Cryptogams (*Chemical News*, 1874); Vegetable Albinism (Journ. Chem. Soc., 1879, 1880, 1886, Pts. I.-III.); New Mineral Species, Churchite, Tavistockite, Bayldonite (*ibid.*, 1865); Namaqualite (*ibid.*, 1870); Analysis of Mineral Phosphates and Arseniates (*ibid.*, 1868, 1870, 1873, 1875, &c., Proc. Roy. Irish Acad., 1882), &c.

ALFRED GEORGE GREENHILL, M.A.,

Professor of Mathematics for the Advanced Class of Artillery Officers at Woolwich. Was Second Wrangler and bracketed Smith's Prizeman in 1870. Has been Moderator and Examiner for the Mathematical Tripos, University of Cambridge, in 1875, '77, '78, '81, '83, '84. Author of "Differential and Integral Calculus" (1886); Article on Hydromechanics in the "Encyclopædia Britannica." Also of the following papers, in the Proceedings of the Royal Artillery Institute:—"Rotation required for Stability of Elongated Projectiles" (vol. x); "Motion in Resisting Medium" (*ibid.*); "Trajectory for Cubic Law of Resistance" (vol. xiv.); "Reduction of Bashforth's Experiments" (vol. xv.); "Siacci's Method for solving Ballistic Problems" (vol. xiv.). In the *Journal de Physique*:—"Sur le Magnétisme induit d'un Ellipsoide creux" (1881). *American Journal of Mathematics*:—"Wave Motion in Hydrodynamics" (vol. ix.). In the *Engineer*:—"Screw-propeller Efficiency" (1886). In the *Quarterly Journal of Mathematics*:—"Precession and Nutation" (vol. xiv.); "Plane Vortex Motion" (vol. xv.); "Motion of Top" (*ibid.*); "Motion of Water in Rotating Parallelopiped" (*ibid.*); "Fluid Motion between Confocal Ellipsoids" (vol. xvi.); "Solution by Elliptic Functions of Problems in Heat and Electricity" (vol. xvii.); "Functional Images in Cartesians" (vol. xviii.); "Complex Multiplication of Elliptic Functions" (vol. xxii.), and others. In *Messenger of Mathematics*:—"Fluid Motion" (vols. viii.-x.); "Lord Rayleigh's Theory of Tennis Ball" (vol. ix.); "Period Equation of Lateral Vibrations" (vol. xvi.); "Sumner lines on Mercator's Chart" (*ibid.*); "Solution of Cubic and Quartic" (vol. xvii.). In the Proceedings of the Cambridge Philosophical Society:—"Rotation of Liquid Ellipsoid" (vols. iii., iv.); "Green's Function for Rectangular Parallelopiped" (vol. iii); "Integrals expressed by Inverse Elliptic Functions (*ibid.*); "Conjugate Functions of Cartesians" (vol. iv.); "Greatest Height a Tree can grow" (*ibid.*); "Complex Multiplication of Elliptic Functions" (vols. iv., v.). In Proceedings Institution Mechanical Engineers:—"Stability of Shafting" (1883).

LIEUT.-GENERAL SIR WILLIAM FRANCIS DRUMMOND JERVOIS, R.E., G.C.M.G.,

Governor and Commander-in-Chief of New Zealand. Distinguished as a Military Engineer. From 1841 to 1848 employed in South Africa, during which time he erected important military works, and added largely to the topographical knowledge of that part of the world, discovering the true feature of the Quathlamba Mountains, and making a minute topographical survey of Kaffraria; his map, published by E. Stanford, being a wonderful delineation of most difficult and rugged country. For nearly twenty years, from 1856 to 1875, employed in the designing and execution of the fortifications of the Empire at a most critical period, when, owing to the introduction of iron armour, a complete revolution took place in matters relating to ships, forts, and artillery. Was a member of the Scientific Commission (1861-62, &c.) appointed to investigate the subject of the application of iron armour to ships and forts. Governor of Straits Settlements, 1875-77. In 1877 selected to advise the Governments of Australia on the defence of their principal harbours. His recommendations have been adopted and carried out. In 1877 appointed Governor of South Australia, and in that capacity, as also in that of Governor of New Zealand (since 1882), has promoted the progress of Science in various ways.

CHARLES LAPWORTH,

Professor of Geology in the Mason Science College, Birmingham; Hon. LL.D. (St. Andr.). Most important contributions to the right understanding of the stratigraphy of the North-West Highlands and the Southern Uplands of Scotland, and investigations of the Palæozoic and other strata, as published in his papers on "The Moffat Series," "The Girvan Succession," "The Stratigraphy and Metamorphism of the Duness and Eriboll District," the "Secret of the Highlands," the "Close of the Highland Controversy," "Discovery of the Cambrian Rocks in the Neighbourhood of Birmingham," and on "The Classification of the Lower Palæozoic Rocks," &c.,—papers published between 1878 and 1887 in the Quart. Journ. Geol. Soc., and the *Geol. Mag.* Also for his Palæontological work, especially among the Rhabdophora, mainly published in six papers between 1873 and 1887. Recipient of the Murchison and of the Lyell Funds, and of the Bigsby Medal of the Geological Society.

T. JEFFREY PARKER,

Professor of Biology. Author of the Memoirs enumerated below. Distinguished as a Comparative Anatomist and as a Teacher. Has introduced an important new method of preserving the skeletons of cartilaginous fishes for museum purposes, and has rendered service to the cause of Science in the Colonies by his creation of the Otago Museum, and by his popular lectures and addresses. He has published thirty-three original papers on Biological subjects in the Proceedings and Transactions of various Societies—Royal, Zoological, Royal Microscopical, &c. Amongst these may be mentioned the following, viz.:—"On the Stomach of the Fresh-water Cray-fish," "On the Stridulating Organ of *Palinurus vulgaris*," "On the Intestinal Spiral Valve in the Genus *Raia*," "On the Histology of *Hydra fusca*," "On the Venous System of the Skate," "On the Osteology of *Regalecus argenteus*," "On the Blood-vessels of *Mustelus antarcticus*," &c.

JOHN HENRY POYNTING, M.A., B.Sc.

Professor of Physics in the Mason College, Birmingham. Author of the following papers:—"On a Method of Using the Balance with great Delicacy" (Proc. Roy. Soc., vol. xxviii.); "On the Graduation of the Sonometer" (*Phil. Mag.*, 1880); "On a Simple Form of Saccharimeter" (*ibid.*, 1880); "On Change of State: Solid-Liquid" (*ibid.*, 1881); "On the Connection between Electric Current and the Electric and Magnetic Inductions in the surrounding Field" (Proc. Roy. Soc., vol. xxxviii.); "On the Transfer of Energy in the Electro-magnetic Field" (Phil. Trans., 1884, Part II.).

WILLIAM RAMSAY,

Ph.D. (Tüb.). F.C.S., F.I.C. Professor of Chemistry, University College, London. President of the Bristol Society of Naturalists, and of the Bristol Section of the Society of Chemical Industry. Distinguished as a Chemist, and especially for his researches in Chemical Physics. Author of the following papers:—"Orthotoluic Acid and its Derivatives" (*Liebig's Annalen*, 1872); "Picoline and its Derivatives" (*Phil. Mag.*, 1876-78); "The Oxidation Products of Quinine and allied Alkaloids" (Journ. Chem. Soc., 1878-79); "Specific Volumes" (*ibid.*, 1879-81); "The Volatilization of Solids" (Phil. Trans., Pt. I., 1884); "The Vapour Pressures of Solids and Liquids" (Phil. Trans., Pt. II., 1884); "A Study of the Thermal Properties of Alcohol" (Proc. Roy. Soc., vol. xxxviii., p. 329); "On Evaporation and Dissociation" (Preliminary Notice, Rep. Brit. Assoc., 1884).

THOMAS PRIDGIN TEALE, M.A. (Oxon.),

F.R.C.S., 1857. Surgeon to the Leeds General Infirmary. Late Lecturer on Surgery, Leeds School of Medicine. Member of the General Medical Council. Eminent as a Sanitary Reformer, and Surgeon and Ophthalmologist. Author of—(*a*) various Papers and Lectures bearing upon Public Health and Sanitary Reforms, among which are:—(1) "Dangers to Health in our own Houses," a Lecture at the Leeds Lit. and Phil. Soc., 1877; (2) "Dangers to Health: a Pictorial Guide to Domestic Sanitary Defects," 4th ed., 1883 (also in French and German); (3) "Economy of Coal in House Fires," 1882; (4) "Address on Health" (dealing with the effects of Modern Educational Systems upon Health), delivered as President of the Health Section of the Social Science Congress at Huddersfield, 1883. (*b*) Papers of value in Surgery and Ophthalmology, extending from 1850 to 1885—(1) "On the Treatment of Lachrymal Obstructions, with suggestions to use Bulbed Probes" (*Med. Times and Gaz.*, 1860); (2) "On the Relief of Symblepharon by the Transplantation of Conjunctiva" (Ophth. Hosp. Rep., vol. iii., and Report of the International Ophthalmic Congress in London, 1872); (3) "On Extraction of Soft Cataract by Suction" (Ophth. Hosp. Rep., vol. iv.); (4) "The Relative Value of Atropine and Mercury in Acute Iritis" (*ibid.*, vol. v.); (5) "Enucleation of Nævus" (Trans. Med. and Chir. Soc., 1867); (6) "On Atrophy induced by Cicatrix" (*Brit. Med. Journ.*, 1867); (7) "On the Stimulation of Hip Disease by Suppuration of the Bursa over the Trochanter major" (Clin. Essay, No. 2, *Lancet*, 1870); (8) "Ovariotomy during Acute Inflammation of the Cyst" (*Lancet*, 1873); (9) "Ovariotomy in extremis" (Clin. Essay, No. 4, *Lancet*, 1874); (10) "Exploration of the Abdomen in cases of Obstruction of the Bowel" (Clin. Essay, No. 5, *Lancet*, 1875); (11) "On the Treatment of Vesical Irritability and Incontinence in the Female, by Dilatation of the Neck of the Bladder" (Clin. Essay, No. 6, *Lancet*, 1875); (12) "The Surgery of Scrofulous Glands" (*Med. Times and Gazette*, 1885).

WILLIAM TOPLEY,

F.G.S., Assoc.Inst.C.E. Student of the Royal School of Mines, 1858-61. For twenty years engaged in the Geological Survey; and has mapped parts of Kent, Surrey, Durham, Northumberland, &c., with illustrative sections and memoirs. Author of a general Memoir on the Geology of the Weald of Kent and Sussex. Author of various papers in Quart. Journ. Geol. Soc.; of a paper on the Relation of Geology to Agriculture, in Journ. Roy. Agric. Soc.; and on the Channel Tunnel, in *Quart. Journ. Sci.* Assisted Dr. Buchanan in a Report to the Privy Council Medical Officer, on the Distribution of Phthisis as affected by dampness of soil. Secretary (1872-81) of the Geol. Section of Brit. Assoc. Member for England of the Committee for preparing an International Geological Map of Europe. Editor of the *Geological Record*. President, Geologists' Association. Author of Report on "The National Geological Surveys of Europe" (Brit. Assoc., 1884).

HENRY TRIMEN, M.B. (Lond.),

F.L.S. Director of the Royal Botanic Gardens, Ceylon. Devoted to the study of Botany, systematic, descriptive, economic, geographical, and historical. Editor of the *Journal of Botany*, 1872-79. Author (in conjunction with Mr. W. T. Thiselton Dyer, F.R.S.) of "Flora of Middlesex" (1869); of the Botanical portion of Bentley and Trimen's "Medicinal Plants" (1875-80); and of more than sixty papers on botanical subjects, including:—"Descriptions and Critical Observations on the Successive Additions to the British Flora" (*Journ. of Bot.*, 1866-79); "The *Juncaceæ* of Portugal" (*ibid.*, 1872); "*Spenceria*, a new genus of *Rosaceæ*" (*ibid.*, 1879); "*Phyllorachis*, a new genus of *Gramineæ*" (*ibid.*); "Notes on *Oudneya* and *Boea*" (Linn. Soc. Journ., 1877-79); "Systematic Catalogue of the Phanerogams and Ferns of Ceylon" (Journ. Asiat. Soc. Ceylon, 1885); "Notes on the Flora of Ceylon, with Descriptions of many new species" (*Journ. of Bot.*, 1885); "Hermann's Ceylon Herbarium and Linnæus's 'Flora Zeylonica,'" being a critical examination of the plants of Hermann described by Linnæus (Linn. Soc. Journ., 1887); "Report to the Madras Government on the Cinchona Plantations of that Presidency" (1883); "Annual Reports of the Botanic Gardens, Ceylon" (1880–85).

HENRY MARSHALL WARD, M.A.,

F.L.S. Fellow of Christ's College, Cambridge. Professor of Botany, Royal Indian Engineering College, Cooper's Hill (Forestry Branch.) Distinguished for his researches in Histological and Cryptogamic Botany. Appointed by the Secretary of State for the Colonies to visit Ceylon, 1879–81, to investigate the Coffee-Leaf Disease. Has published numerous researches, of which the following are the more important:—"On the Embryo-sac and Development of *Gymnodenia conopsea*" (*Quart. Journ. Micros. Sci.*, 1880, pls. 3); "A Contribution to our knowledge of the Embryo-sac in Angiosperms" (Journ. Linn. Soc., 1880, pls. 9); First, second, and third Reports on the Coffee-Leaf Disease, Ceylon, 1880–81 (*ibid.*); "Researches on the Morphology and Life-history of a tropical Pyrenomycetous Fungus (*Asterina*)" (*Quart. Journ. Micros. Sci.*, 1882, pls. 2); "Observations on the genus *Pythueni*" (*Quart. Journ. Micros. Sci.*, 1884, pls. 3); "On the Structure, Development, and Life-history of a tropical Epiphyllous Lichen (*Strigula complanata*)" (Trans. Linn. Soc., 1883, pls. 4); "On the Morphology and the Development of the Perithecium of *Meliola*, a genus of tropical Epiphyllous Fungi" (Phil. Trans., 1883, Pls. 3); "On the Structure and Life-history of *Entyloma Ranunculi*" (Phil. Trans. 1887, pls. 4); "On some points in the Histology and Physiology of the Fruits and Seeds of the genus *Rhamnus*" (*Annals of Botany*, 1887, pls. 2). Translator of "Lectures on the Physiology of Plants," by Julius von Sachs (Clarendon Press, 1887).

WILLIAM HENRY WHITE,

Assistant Controller and Director of Naval Construction. Charged with principal responsibility for design and construction of all ships of the Royal Navy. Author of a "Manual of Naval Architecture," adopted as a Text-book in the Royal Naval College, issued to the Royal Navy, translated into German and Italian, and officially issued to both fleets. Author of numerous papers on the science and practice of Shipbuilding, most of these being published in the Transactions of the Inst. of Naval Architects, of which he is a Member of Council. In these papers there is a large amount of original scientific work, notably in "Calculations for the Stability of Ships," 1871 (written jointly with Mr. M. John); The Geometry of Metacentric Diagrams," 1878; "The Rolling of Sailing Ships," 1881; "The Course of Study at the Roy. Nav. College," 1877. Engaged in extensive theoretical investigations and experiments on the Structural Strength of Ships, and the Strains to which they are subjected at sea. Many of the results published in the "Manual of Naval Architecture" and Trans. Inst. Nav. Architects. Has had much to do with the extension of systematic observations of rolling, pitching, and general behaviour of H.M. ships at sea, from which much good has resulted to Ship-design, and valuable additions have been made to trustworthy information on Ocean Waves. Has also been able to render good service to the general extension of scientific methods of observing and analyzing the steam trials and turning trials of H.M. ships. Was closely associated for some years with the late Mr. Froude, and with the practical development in the designs of H.M. ships of the principles deduced from model experiments originated and conducted by Mr. Froude, which experiments are now superintended by the late Mr. Froude's son, Mr. R. G. Froude. Is the designer of some of the swiftest ships afloat, both armoured and unarmoured, in which designs wide departures were made from previous practice. Is a member of the Inst. Civ. Eng.; of the Council of the Inst. Naval Architects; Hon. Mem. of the N.E. Coast Inst. of Engineers and Shipbuilders; Member of the Roy. Unit. Serv. Inst. Has diploma as Fellow of the Royal School of Naval Architecture (highest class). Professor of Naval Architecture at South Kensington, 1871–73, and at Royal Naval College, 1873–81.

THE ISLANDS OF VULCANO AND STROMBOLI.

IN the spring of last year, accompanied by my friend Signor Gaetano Platania, I passed a month in a geological ramble through the Æolian Islands. In consequence of such a short stay, no observations were carried out with sufficient detail and accuracy to be worthy of publishing, especially after the many important observations that we already possess from Spallanzani to Judd. Unfortunately, the isolated position of the group, and the absence of any sufficiently qualified local observer, render it impossible to have continuous records of the vulcanological and seismological phenomena of the islands; in fact, what little is known has come from the few scientific travellers who from time to time visit this out-of-the-way locality. It is for that reason, therefore, that the following notes have been written, in the hope of saving a few of the links in the broken chain of the record of the two active volcanoes of Stromboli and Vulcano.

We arrived at Vulcano on May 24, 1887, and left the island on May 28. The eruption that had occurred during February and two following months of 1886 had drilled out the bottom of the crater, so that the lower half of the path (on the west side) leading down to the bottom of the crater had been removed, and its lower end terminated abruptly in a cliff sheer down to the crater bottom. In consequence we were unable to descend, but we could on two days get a good view of the crater bottom. Much hissing and blowing off of steam was going on from the fissures of the floor of the crater, which was covered by a layer of purplish-gray ash washed down from the sloping sides. The edges of the fissures in the bottom and lower part of the crater sides were covered by a yellow crust of what was no doubt sulphur, boric acid, &c.

On the somewhat flattened ridge forming the northern lip of the crater, and not very far from the head of the celebrated obsidian lava stream, was a very large fumarole emitting a strong and large jet of steam under pressure, having about the size and force of that of the *bocca grande* of the Solfatara. With our sticks we removed some of the stones choking the hole, which on their cooler parts were covered with deposits of sulphur and realgar. When this was exposed to the full jet of steam, the minerals were melted, and blown away or over the surface of the blocks, forming a kind of reddish varnish or patina, whilst a rain of drops was thrown into the air, so that our clothes and hats were bespattered with beads of a variable mixture of sulphur and realgar. To the east side, where are distinguishable three crater rings, a considerable number of fumaroles exist, depositing chiefly sulphur, but also boric acid where hottest. Mr. Narlian, a resident in the island, says that not since the 1886 eruption "has the crater entered into its former quiescent condition."

On the upper portion of the northern slopes of the cone, to the east of the obsidian stream, all the ground is fumarolic, and choked with sulphur, where that mineral is extensively quarried.

Vulcanello seems on the verge of extinction, it being possible to find only slightly warm exhalations of watery vapour in a few fissures.

During the days we were at Vulcano we noticed that the apparent quantity of vapour emitted had a very marked relationship to the moisture of the atmosphere, and therefore, indirectly, to the winds. The same we also observed to be the case at Vulcano as we saw it from time to time during our stay on the Island of Lipari.

June 1, 2, and 3 were spent at Stromboli. In ascending the volcano, we, on leaving the town, skirted the northern coast of the island, and after passing the Punta Labronzo commenced the ascent, gradually approaching the north-east limit of the Sciarra. It is a track that passes chiefly over hard rock, and to be strongly recommended in preference to any other paths, which are mostly over loose materials. Skirting the crater, one walks along the ridge of the mountain which overhangs and partly hides the crater; we commenced to descend a little on the south side of the volcanic mouth, until we arrived at a small pinnacle of rock, where a good view of the crater was obtainable. Here, under very great difficulties, from the looseness of the ground of about two square metres upon which we stood, an attempt was made to take two instantaneous photographs of the crater as we looked down into it. Unfortunately, both of these were useless, as we foresaw, from the vapour blowing towards us.

The crater was very quiet, only throwing out a very few fragments of pasty lava cake, with about four or five explosions during the four hours we remained near by. There were other explosions, but too weak to eject anything. I descended to the crater edge, but could not remain long, on account of the heat of the ground and the acid fumes, which seemed to be in great part composed of HCl with a good dash of SO_2.

On returning from the crater edge and descending a little lower on the south-west of the Sciarra, a good view is obtainable of that slope and the crater. Here two successful photographs were taken, which show very well the crater with its relative position to the summit of the mountain and to the Sciarra. On the following day the tour of the island was made in a boat, and, as only a few stones were being ejected, we were able to land on the narrow ledge or beach at the foot of the Sciarra. Two successful photographs were taken from the Scoglio dei Cavassi, from which a fine view is obtainable of the Sciarra and the crater.

During our residence on the island, and our stay at Salina and Panaria, we always noticed that the amount of visible vapour issuing was in direct proportion to the humidity of the atmosphere. On account of the great quietness of the volcano, it was impossible to form any judgment as to whether there was any relation of increased or diminished activity to the barometric pressure, and so, indirectly, to the winds.

Since leaving the island, correspondence has been kept up between Signor Giuseppe Rende, the post and telegraph master, and myself. The following information I have been able to glean from that gentleman's letters. From June to November 1887 the volcano remained in its normal state. On November 18, a moderate eruption (*eruzione mediocre*), and the wind blowing from the west, a shower of scoria (? fragments) (*aride pietre*), fell amongst the vines near the village. This was accompanied by explosions (*botti*), which, it appears, considerably frightened the people. Later, the scoria (*pomice*) fell into the sea, which it covered as far as the eye could see. Unfortunately, Signor Rende did not preserve any of the *ejectamenta*, but, judging from what one sees composing recent deposits of the island, the material was a pumiceous scoria, or a light scoria, as it appears to have floated on the sea.

In answer to further inquiries, Signor G. Rende tells me that the floating scoria extended *eastwards* as far as the eye could reach. No lava appeared, but a small mouth opened at the edge of the crater, but in a very few days disappeared. He then goes on to say :—

"I draw your attention in this letter to a very remarkable fact. On the 25th of last February (*i.e.* 1888), at 4.21 p.m., occurred two little shocks of earthquake of *undulatory* character, followed by a *subsultory* one, so that we thought it would be the end of the world for us. Never had a *subsultory* earthquake been felt. It split various houses, overturned walls, and made earth-banks slip. Those who had their eyes fixed on the mountain seemed to see the summit of it fall over from south to north. People who were working amongst the vines fell on their faces. No victims. Neither Panaria, Lipari, nor the other islands noticed the shock. The volcano (*i.e.* Stromboli) was in no way affected (*non fece mossa alcuna*)."

Prof. Mercalli has collected together what is known of the history of Vulcano and Stromboli. He also published accounts of the state of these volcanoes during the years 1882–86 inclusive ("Natura delle eruzione dello Stromboli," *Atti della Soc. Ital. di Sc. Nat.* vol. xxiv.; "Notizie sullo stato attuale dei vulcani attivi Italiani," *ibid.* vol. xxvii.; "La fossa di Vulcano e lo Stromboli dal 1884 al 1886," *ibid.* vol. xxix.).

The eruption of November 18, 1887, is curiously near the date of November 17, 1882, when one of the strongest modern eruptions of Stromboli occurred, and when five lateral mouths opened on the Sciarra about 100 metres below the crater edge, but without the ejection of a lava stream. As on one or two other occasions, the last eruption extensively covered the sea with scoria, a fact of no small importance when we take into consideration that Stromboli is a very basic volcano, in a unique state of chronic activity, and is yet able to produce scoria or pumiceous scoria, sufficiently vesicular to float on the sea, and so be transported to great distances.

With regard to the position of lateral eruptions of this mountain, the only situation in which dykes are visible is on the north-west side and near the Sciarra, where a considerable number are to be seen. One of these is visible in section near La Serra, showing it continuous with a lava flow that oozed from it only a few metres above sea-level, indicating that not very long since a lateral eruption gave rise to a lava stream; another, close to the crater, stands out as a great wall at right angles to the present eruptive axis of Stromboli, and certainly must have been formed when the crater was at a very much higher level. No less than three dykes at Stromboli are *hollow* ones, with their interspace filled in from above by loose materials, showing that they must also have been drained below present sea-level, as they reach—as hollow dykes—down to the beach. I believe I was the first to draw attention to this peculiar variety of dyke, in describing the eruption of Vesuvius of May 2, 1885, where it was possible to watch the process of formation ("L'Eruzione del Vesuvio nel 2 Maggio, 1885," *Ann. d. Accad. O. Costa d'Asp. Naturalisti*, Era 3, vol. i.; and "Lo Spettatore del Vesuvio," Napoli, 1887). These hollow dykes of Stromboli may be seen at La Serra, the northern limit of La Sciarra, and at Punta Labronzo. I expected them to be rare, as there is no mention of them made in any literature known to me; but as it is also well shown near the Punta del Corno, at Vulcano, it can hardly be the case.

In conclusion, I take this opportunity of thanking Signor Narlian, of Vulcano, and Signor Rende for their past kindness, and for the promise of further notes on these two isolated, neglected, but interesting volcanoes.

H. J. JOHNSTON LAVIS.

HEAD GROWTH IN STUDENTS AT THE UNIVERSITY OF CAMBRIDGE.[1]

IN the memoir read by Dr. Venn, on April 24, at the Anthropological Institute, upon the measurements made, during the last three years, of the students of Cambridge, one column is assigned to what he terms "Head Products," and which may fairly be interpreted as "Relative Brain Volumes." The entries in it are obtained by multiplying together the maximum length and breadth of the head and its height above a specified plane. The product of the three determines the contents of a rectangular box that would just include the portion of the head referred to. The capacity of this box would be only rudely proportionate to that of the skull in individual cases, but ought to be closely proportionate in the average of many cases. The relation they bear to one another affords, as it seems to me, a trustworthy basis for the following discussion, especially as all the measurements were made not only on a uniform plan, but by the same operator.

[1] Read at the Anthropological Institute, on April 24, by Francis Galton, F.R.S.

It will be convenient to reproduce Dr. Venn's figures in a separate table, neglecting the second decimal:—

Head Products.

Ages.	Class A. "High honour" men.	Number of measures.	Class B. The remaining "honour" men.	Number of measures.	Class C. "Poll" men.	Number of measures.
19	241·9	17	237·1	70	229·1	52
20	244·2	54	237·9	149	235·1	102
21	241·0	52	236·4	117	240·2	79
22	248·1	50	241·7	73	240·0	66
23	244·6	27	239·0	33	235·0	23
24	245·8	25	251·2	14	244·4	13
25 and upwards.	248·9	33	239·1	20	243·5	26
		258		476		361

The figures in the table are thrown into diagrams in Figs. I., II., and III., in which curves are also drawn to interpret what seems to be their significance. The great irregularity in Fig. II., corresponding to the age of twenty-four, may be fairly ascribed to the smallness of observations, only thirteen in number, on which it is founded. The three resultant curves are shown by themselves in Fig. IV., where they can be easily compared. It will then be seen that the A and C curves are markedly different, and that the B curve is intermediate. Accepting these curves as a true statement of the case—and they are beyond doubt an approximately true statement—we find that a "high honour" man possesses at the age of nineteen a distinctly larger brain than a "poll" man in the proportion of 241 to 230·5, or one that is almost 5 per cent. larger. By the end of his College career, the brain of the "high honour" man has increased from 241 to 249; that is by 3 per cent. of its size, while the brain of the "poll" man has increased from 230·5 to 244·5, or 6 per cent.

Four conclusions follow from all this:—

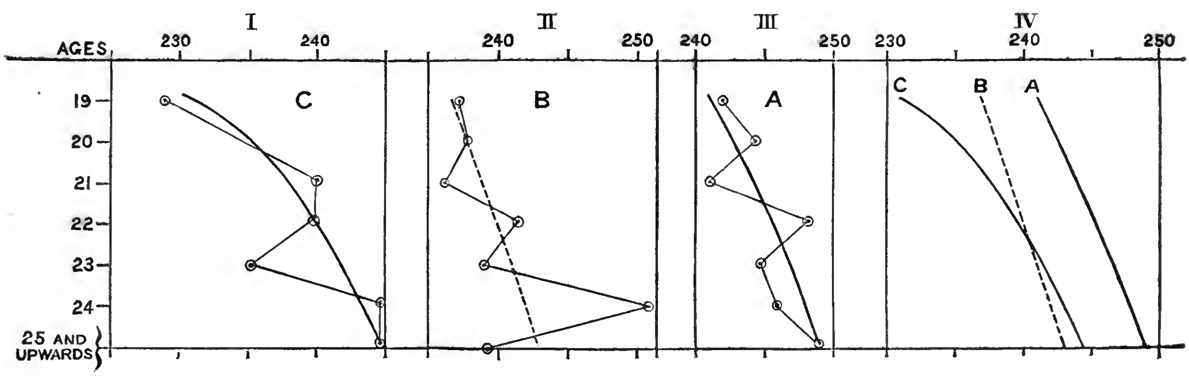
(1) Although it is pretty well ascertained that in the masses of the population the brain ceases to grow after the age of nineteen, or even earlier, it is by no means so with University students.

(2) That men who obtain high honours have had considerably larger brains than others at the age of nineteen.

(3) That they have larger brains than others, but not to the same extent, at the age of twenty-five; in fact their predominance is by that time diminished to one-half of what it was.

(4) Consequently "high honour" men are presumably, as a class, both more precocious and more gifted throughout than others. We must therefore look upon eminent University success as a fortunate combination of these two helpful conditions.

Length × Breadth × Height of Head, in inches, of Cambridge University Men at different Ages (from Dr. Venn's Tables).



A, First Class Men; B, Honour Men, not First Class; C, Poll Men.

PHOTOGRAPH OF THE EYE BY FLASH OF MAGNESIUM.

THE effect of complete obscurity on the normal pupil has hitherto been seen only by the light of electric discharges, which allowed of no measurements.

MM. Miethe and Gaedicke, by their invention of the well-known explosive magnesium mixture, have furnished us with a simpler method. A photograph of the eye can be taken in a perfectly dark room, showing the pupil fully dilated, as its reaction does not begin until after exposure.

Mr. Miethe, astronomer at the Potsdam Observatory, himself at my suggestion undertook to execute the accompanying photograph of a normal eye, life-size, after a quarter of an hour's rest in a carefully darkened room. The pupil was found to measure 10 mm. horizontally (the breadth of the cornea being 13 mm.). A reflection of the flash is seen on the cornea.

This kind of photography may prove a new and valuable method for many other branches of scientific

research, but it is of especial utility to ophthalmology, as the eye, by its mobility and sensitiveness, has hitherto been a most difficult subject for the camera.

CLAUDE DU BOIS-REYMOND.

NOTES.

THE Council of the British Association has nominated Prof. Flower for the Presidency of the meeting to be held next year at Newcastle.

THE annual *conversazione* of the Royal Society will be held on Wednesday, May 9.

THE Council of the Marine Biological Association has appointed Mr. Gilbert C. Bourne, M.A., F.L.S., Fellow of New College, Oxford, to be Director and Secretary of the Plymouth Laboratory. Mr. Bourne began the study of biology under Dr. P. Herbert Carpenter at Eton College, and in 1881 obtained an exhibition in natural science at New College. After studying under Prof. Moseley at Oxford and Prof. Aug. Weismann at Freiburg in Baden, Mr. Bourne was placed in the first class in the honour school of natural science at Oxford in 1885. Immediately after taking his degree he proceeded to Diego Garcia in the Indian Ocean, with the purpose of investigating the fauna and flora of that island. On his return to England he became assistant to Prof. Moseley at Oxford, and has performed the duties of Lecturer and Demonstrator in Animal Morphology for the last two years. In October last Mr. Bourne was elected to an open Fellowship at New College.

ON the evening of April 5, about one hundred and fifty persons interested in science met in the hall of the Columbian University, Washington, to pay a tribute to the memory of Asa Gray. Prof. Langley, Secretary of the Smithsonian Institution, presided, and addresses were delivered by Prof. Chickering, Dr. Vasey, Prof. L. F. Ward, and Dr. C. V. Riley.

THE sixty-first meeting of the German Association of Naturalists will take place at Cologne from the 18th to the 23rd of September next. Prof. Bardenheuer and the chemist Th. Kyll are the secretaries. The subjects to be considered will be divided into thirty sections.

THE following sums for the furtherance of scientific studies have been presented by the Academy of Sciences at Berlin: 1500 marks (£75,) to Dr. Goldstein (Berlin), a physicist; 2000 marks (£100) to Dr. Fabricius (Berlin), the archæologist, and Dr. Suhlmann (Würzburg); and 900 marks (£45) to Prof. Gerhard (Eisleben).

CAPTAIN C. E. DUTTON, of the U.S. Geological Survey, is writing his monograph on the Charleston earthquake. The reports on which it will be based are complete, and in shape for the printer. *Science* is of opinion that no earthquake of ancient or modern times has been observed with such care and fulness of detail. Besides the observations made by Professors in several Colleges, by hundreds of railway officials, and at signal stations, a large number of intelligent private citizens have given an account of their own experiences. The volume which Mr. Dutton is editing will also contain a report on the Sonora earthquake.

ON the night of April 17 a magnificent display of the aurora borealis was observed at Motala, in Sweden, in the northern sky. On the same night at 9.5 p.m. a phenomenon was seen in the north-western sky at Örebro, also in Central Sweden, having the appearance of a bright horizontal flash of lightning, but without any report. It was followed by the appearance of an unsteady and varying aurora. The thermometer stood at 21° C.

ON the night of March 27 a rumbling noise like that of a distant earthquake was heard at Aaseral, in Southern Norway, but no shock was felt. It could not have been thunder, as the weather was clear and intensely cold.

ACCORDING to the official report of the recent great earthquake in Yunnan Province of China, the shocks commenced between 5 and 6 p.m. on January 14, and lasted till 4 o'clock the following morning. During this period about ten serious shocks were counted, all being accompanied by a noise like thunder. In district cities in the south of the province, the town walls were either thrown down or cracked, while public offices and temples shared the same fate. In the city of Shih-ping large numbers of private houses were destroyed, those in the south and east quarters suffering most, while those which remained standing had cracked or slanting walls. Two hundred persons were killed in this town alone, and 3000 were injured. In and around this single city about 5000 persons were killed and injured. Most of the people were left without homes, and were starving, as the provisions were buried in the ruins of the houses. In one town the gaol was thrown down by the shocks, and all the prisoners escaped. The earthquake is said to be the most destructive ever recorded in China. The locality in which it was most violent is mountainous, and produces copper and a particular kind of tea for which Yunnan is famous. The area of disturbance is said to be about 770 miles from east to west, and 60 from north to south, Shih-ping being near the centre. The direction of the shocks appears to have been at right angles to the prevailing direction of the valleys, lakes, and rivers of the region. This, at least, is how the Pekin correspondent of a Shanghai newspaper reads the report; and he adds that from the centre of intensity, a little to the west of the city of Shih-ping, there was a decided extension of the earthquake-wave northward in the direction of the Yunnan lake Tienchih, as well as westward to the city of Weiyuen.

IT is curious to notice that on the day when this earthquake occurred there was one also at Luchon, a town in the Szechuen Province, about 350 English miles north-east of the locality of the Yunnan earthquake. Much loss of life is said to have taken place here also, and there was a great subsidence of land. No official report respecting this second earthquake has yet made its appearance.

THE Manilla Government has intrusted to the Sub-Director of the local Observatory the task of studying the causes of the numerous storms which prevail along the coast of the Philippine Archipelago as well as inland, with a view to drawing a meteorological chart of the islands, and of establishing their magnetic positions.

THE Pilot Chart of the North Atlantic Ocean for the month of March, issued by the United States Hydrographer, contains the following interesting facts. Three pronounced cyclonic storms passed over the North Atlantic during the month. One of these was in some respects one of the most remarkable and destructive storms ever experienced along the Atlantic coast of the United States. After traversing the entire American continent from west to east without any noteworthy energy, it gained terrific force on reaching the coast to the southward of Hatteras on the 11th. Its progress eastward was delayed from the 11th to the 15th by an area of high barometer, and it then resumed its course easterly with renewed energy, crossing the 40th meridian in about 50° N. latitude. Much less fog was experienced off the Grand Banks than usual during March. Field ice was encountered as far south as 43° N., and between 46° and 60° W., but the amount reported was not great. Earthquakes were experienced by the United States store-ship at Coquimbo on January 4, and by the British ship *Diadem* in latitude 26° 2′ N., longitude 63° 19′ W., on March 1. The sensation in the latter case was as though the vessel had grounded upon a reef.

IN the storm to which reference is made in the preceding note, oil seems to have been freely used off the coast of the United States for the calming of the waves. According to *Science*, more than a dozen captains and sailing-masters caught in the tempest when at its worst believe their vessels were saved by this expedient. The sailing-master of the yacht *Iroquois* reports that when furious waves with an immense comb were approaching they were deprived of their power to do harm by "a patch of oil no larger than a dining-room table."

ANOTHER interesting mineral synthesis has just been effected by M. Dufet. Native di-calcium arsenate, pharmacolite, occurs very sparingly upon the known parts of the surface of our globe, and is so rarely found in well-defined crystals that M. Des Cloizeaux has only just completed his investigation of its mineralogical and optical properties. Generally it is found in the form of silky fibres, but is at times met with in perfect monoclinic prisms of pearly lustre and frequently possessing a pink tint. M. Dufet has succeeded in producing these beautiful crystals by a very ingenious method. Two concentric vases, the outer containing nitrate of lime and the inner di-sodium arsenate, were filled with water, and so arranged that very slow diffusion occurred between the two liquids. The conditions of Nature were evidently very closely imitated, for the very gradual precipitation thus brought about resulted in the formation of groups of crystals, exactly resembling those of pharmacolite. Goniometrical measurements showed that they belonged to the monoclinic system; and the close approximation of the fundamental angles to those of the mineral given by Haidinger and Schrauf, and especially the still more remarkable closeness to the values just arrived at by M. Des Cloizeaux, leave no doubt as to the identity of the artificial with the natural. The chemical analysis of M. Dufet's crystals leads to the formula $HCaAsO_4 + 2H_2O$, and it thus becomes chemically as well as physically isomorphous with brushite, the corresponding phosphate of calcium, $HCaPO_4 + 2H_2O$. This result clears up the discrepancy between the acknowledged formula of the latter mineral and that given by older mineralogists for pharmacolite, $2HCaAsO_4 + 5H_2O$. The number of minerals which have now been reproduced in the laboratory must be very considerable, and every day the likelihood is increasing that those noble species which have for ages been prized as gems may discover the secret of their formation to some indefatigable worker. Rubies and sapphires have already yielded, possibly the diamond may not prove refractory much longer.

A VALUABLE paper, describing a new method of extraction of the alkaloids from Cinchona bark by cold oil, as used at the Government Cinchona Factory in Sikkim, was lately drawn up by order of the Lieutenant-Governor of Bengal, and has now been issued. Dr. King, the Superintendent of the Sikkim Plantation, carried on a long series of experiments on an acid and alkali process of manufacture, by which he succeeded in producing an excellent quinine. He never, however, succeeded in recovering much more than half of the amount contained in the bark on which he operated. The acid and alkali process had, therefore, to be abandoned, as wasteful and inefficient. A process depending on the maceration of the bark in spirit was next tried, but, after much experiment, it was in turn abandoned. During a visit which Dr. King paid to Holland in 1884, he obtained some hints as to a process of extraction by means of oil. Benefiting by the advice of some chemical friends, Mr. Gammie, the resident manager in Sikkim, has been able to perfect this process, with the result that the whole of the quinine in yellow bark can be extracted in a form indistinguishable, either chemically or physically, from the best brands of European manufacture. This can be done cheaply, and the Bengal Government has caused an account of the matter to be printed, in order that private growers of Cinchona may be enabled to take full advantage of the process, and that a permanent reduction in the price of quinine may ensue.

THE Trustees of the Indian Museum, Calcutta, have issued a circular announcing that they have had under their consideration the means whereby a useful scientific examination of the insect-pests of India can be best effected. Bearing in view the great economic importance of the investigation, they have directed the first assistant, Mr. E. C. Cotes, to consider it an essential portion of his duties, and have instructed him to communicate with persons interested in the subject, and likely to aid the inquiry, in order to collect materials which may form a sufficient basis for really scientific conclusions. Mr. Cotes will gradually record the entire life-histories and practical methods of dealing with the principal insect-pests, publishing from time to time, as materials accumulate, the information collected, and distributing it to those interested. Those who live in the districts where the insects occur, and have actual experience of the pests, are invited to send to Mr. Cotes accounts of facts they have observed; and the circular includes a full statement of the points upon which information is wanted.

READERS interested in the science and practice of forestry will be interested in the perusal of a Report by the American Consul at Mayence, on Forest-Culture in Hesse, contained in the January issue of the Consular Reports of the United States. The writer discusses the organizations and functions of the department having the care of forests, the duties of the various classes of officials employed in forest-cultivation, the economical results of the system pursued, the course of instruction followed in the schools of forestry, the organization and methods of the institution for experimental forestry, and the degree and amount of control assumed by the State over private forests. The Report is exceedingly detailed, and is practically a handbook of forestry as practised in the Grand Duchy.

PROF. BLANCHARD, the well-known entomologist, has just published in Paris a book on "La Vie chez les Êtres animés," in which he discusses Darwinism at length, but in a very incomplete manner, and of course in a hostile spirit.

THE address delivered by Mr. A. D. Michael, President of the Quekett Microscopical Club, on the 24th of February last, is printed in the Club's Journal, and has also been issued separately. The subject is "Parasitism."

PROF. HENRY DRUMMOND has in the press a new book, "Tropical Africa," which will be published immediately by Messrs. Hodder and Stoughton. It will contain an account of the author's recent travels in Central Africa, with one or two chapters of natural history.

MR. LEWIS, of Gower Street, will publish immediately a volume of "Physiological and Pathological Researches," by the late T. R. Lewis, F.R.S. (elect). The work is edited by Sir William Aitken, F.R.S., G. E. Dobson, F.R.S., and A. E. Brown, and contains five maps, forty-three plates, including chromo-lithographs, and sixty seven wood engravings.

AT the meeting of the Institution of Civil Engineers on Tuesday, April 24, Mr. E. B. Ellington read a paper on the distribution of hydraulic power in London. In the course of his remarks he took occasion to refer to the large extent to which lifts are now used, and he considered it necessary, he said, to urge the importance of securing the greatest possible safety in their construction by the general adoption of the simple ram. Suspended lifts depended on the sound condition of the ropes or chains from which the cages hung. As they became worn and untrustworthy after a short period, it was usual to add safety appliances to stop the fall of the cage in case of breakage of the suspending ropes, but these appliances could not be expected to act under all circumstances.

MISS MARIE BROWN, well known for her researches on the earliest colonization of North America by the Scandinavians, has presented a petition to the United States Congress urging that steps should be taken to secure a thorough search of the

Vatican and other Italian libraries with a view to further light being thrown upon this question.

MR. W. CHANDLER ROBERTS-AUSTEN will give the discourse on Friday evening, May 11, at the Royal Institution in place of Mr. W. H. Barlow, who is unwell.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀), from India, presented by Mr. Lionel H. Hanbury; a Macaque Monkey (*Macacus cynomolgus* ♂), from Burma, presented by Mrs. G. E. Buchanan; a Scarlet Ibis (*Eudocimus ruber*), a Roseate Spoonbill (*Platalea ajaja*), from Brazil, presented by Mr. Charles Booth; a Common Kestrel (*Tinnunculus alaudarius*), British, presented by Mr. H. Weetman, F.Z.S.; a Hoffmann's Sloth (*Cholopus hoffmanni*), from Panama, deposited; three Lined Finches (*Spermophila lineola*), from South America, purchased; two Persian Gazelles (*Gazella subgutterosa* ♂ ♀), two Chinchillas (*Chinchilla lanigera*), born in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *MAY* 6–12.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on May 6

Sun rises, 4h. 24m.; souths, 11h. 56m. 25·9s.; sets, 19h. 29m.: right asc. on meridian, 2h. 55·5m.; decl. 16° 44′ N. Sidereal Time at Sunset, 10h. 29m.

Moon (New on May 11, 1h.) rises, 3h. 15m.; souths, 8h. 49m.; sets, 14h. 34m.: right asc. on meridian, 23h. 47·5m.; decl. 5° 36′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	4 16	11 36	18 56	2 35·1	14 29 N.
Venus ...	3 56	10 49	17 42	1 47·9	9 35 N.
Mars ...	16 9	21 51	3 33*	12 51·5	4 15 S.
Jupiter ...	20 53*	1 9	5 25	16 6·7	19 52 S.
Saturn ...	9 16	17 13	1 10*	8 12·5	20 35 N.
Uranus...	16 13	21 52	3 31*	12 52·3	4 52 S.
Neptune..	5 7	12 50	20 33	3 49·6	18 25 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

May.	h.	
9	9	Venus in conjunction with and 3° 50′ north of the Moon.
10	22	Mercury in conjunction with and 5° 6′ north of the Moon.
11	0	Mercury in superior conjunction with the Sun.

Saturn, May 6.—Outer major axis of outer ring = 40″·2; outer minor axis of outer ring = 14″·3: southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
R Andromedæ	0 18·1	37 57 N.	May 10,		*M*
U Cephei	0 52·4	81 16 N.	,, 7,	2 19	*m*
			,, 12,	1 58	*m*
ζ Geminorum	6 57·5	20 44 N.	,, 10,	0 0	*m*
δ Libræ	14 55·0	8 4 S.	,, 8,	21 12	*m*
U Coronæ	15 13·6	32 3 N.	,, 7,	20 39	*m*
U Ophiuchi	17 10·9	1 20 N.	,, 7,	2 56	*m*
		and at intervals of		20 8	
Z Sagittarii	18 14·8	18 55 S.	,, 8,	0 0	*m*
U Sagittarii	18 25·3	19 12 S.	,, 7,	2 0	*m*
			,, 10,	1 0	*M*
η Aquilæ	19 46·8	0 43 N.	,, 8,	0 0	*M*
T Aquarii	20 44·0	5 34 S.	,, 7,		*m*
δ Cephei	22 25·0	57 51 N.	,, 8,	23 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ε Crateris	170°	10° S.	Very slow.
,, α Coronæ	232	27 N.	Rather faint and slow.
,, ζ Draconis	260	64 N.	Rather slow.

GEOGRAPHICAL NOTES.

THE *Mouvement Géographique* contains details of Lieut. Van Gèle's recent exploration of the River Mobangi, the great tributary of the north bank of the Congo, which discharges a little below the equator. It will be remembered that the Rev. George Grenfell succeeded in making his way up the river as far as 4° N. latitude, where he was stopped by the Zongo rapids. Lieut. Van Gèle started on October 26 last, and reached the rapids on November 21. There are six of them, covering a space of 34 miles. They are situated in what is really a mountain gorge, the mountains, in gentle slopes, coming down to the river banks. The steamer *En Avant* had to be unloaded several times and dragged up the rapids. The spaces between the rapids are mostly covered with islands, with great bars of rock stretching between them. The country on each side is described as being fine, fertile, and covered with villages. The people here are all of the same tribe; head shaved except at the nape, bristling moustaches, and no tattooing. Above the middle falls, the Bakombé inhabit the country. These arrange the hair in *queues*, some of which are over 6 feet long. From the upper end of the falls the river continues in a north-east direction for about 32 miles, when it rounds to the east. It has a breadth of about 2600 feet, and the navigation is easy, the average depth being 14 feet. The easterly direction is maintained as far as the *En Avant* went, about 172 miles further. The mountains disappear from the right bank, and the left is marked by low hills, with grassy plains and woods alternating. The villages are at some distance from the river, but the people came down to the vessel in crowds all the way up, and were perfectly friendly until the last few days. Over the whole course tropical cultures of every kind were abundant, as well as sheep, goats, and fowls. The natives on the right bank belong to the Buraka and Maduru tribes; those on the right to the Bakangi, the Mombate, and the Banzy. They mostly shave the head so as to leave a triangle of hair, with the forehead as base. The ears are enormously elongated with heavy copper rings. The river here is covered with islands, mostly cultivated and inhabited. Among the Banzy the huts have the shape of huge conical extinguishers, resting on a circular wall about 2 feet high. These huts are ranged in circular rows, forming broad streets, well kept, and with a common meeting-house in the centre. Each hut is divided into two apartments, one used for sleeping. Iron is admirably worked into all sorts of implements, weapons, and ornaments. Ivory is abundant, but used only for bracelets, anklets, and *pelelé* or lip-ornaments. About 100 miles above the Zongo rapids a second is met with, at Bemay. The vessel succeeded in passing it, and a third 25 miles further up. Just above Bemay, the only tributary met with from the Zongo rapids upwards—the Bangasso—discharges into the right bank of the Mobangi. Above the river the country is densely peopled by the Mombongo and Yakoma, and these showed themselves distinctly hostile to the expedition. There were unfortunately several conflicts, in which lives were lost on both sides. Rocks and sand-banks obstructed the navigation, and after getting as far as 21° 55′ E., Lieut. Van Gèle turned back, making his way downwards with some difficulty, as the river had lowered about 10 feet. He arrived at Equator Station on February 1. The river was about 8000 feet wide at the furthest point, and covered with islands, mostly inhabited. On the north bank of the river, one village extended along a distance of 3 miles. As Dr. Junker's furthest point on the Wellé was 22° 55′ E., only 1° of longitude separates his point from Van Gèle's furthest, or about 68 miles. As they are both on the same line of latitude, there can be no doubt that the Mobangi and the Wellé are the same river.

FROM an official Report by Mr. Percy Smith, Assistant Surveyor-General of New Zealand, on a visit to the Kermadec Islands, in August last, we glean some information as to this recent annexation to the British dominions. The group is situated between the parallels of 29° 10′ and 31° 10′ S. lat., and between the meridian of 177° 45′ and 179° W. long. There are four islands, with some outlying isiets and rocks, the most northerly, Raoul or Sunday Island, being 674 miles north-east of Auckland. The islands are all volcanic; in two of them, indeed, signs of volcanic activity are to be seen at the present day, though on a limited scale. They appear to be situated on an oceanic plateau which extends from New Zealand to the Tonga Group, on which soundings are obtained at depths much less than in the adjacent areas, but still so great as to show that the islands form, as it were, the tops of volcanic cones rising to a great height above their bases. The group is situated on the north-easterly projection of the axis of the volcanic zone of the Bay of Plenty, which, continued still further north-eastward, strikes the Tonga and Samoan Groups, places where volcanic action is still going on. Two, if not three, volcanic disturbances have taken place at the Kermadec Islands within recent years, and earthquakes were very frequent there at one time; but since the eruption of Tarawera, June 10, 1886, they have ceased entirely. On Sunday Island the most prominent feature is the large crater near the centre of the island. It is $1\frac{3}{4}$ mile long by $1\frac{1}{4}$ mile wide; its walls are generally over 1000 feet high. Steam escapes occasionally from the Green Lake on the south side, and from the crevices in the precipitous cliffs of Denham Bay, while warm water oozes out of the sand on the north coast.

DR. HANS MEYER, who recently ascended Kilimanjaro, and Dr. O. Baumann, who accompanied Dr. Lenz up the Congo, are preparing to start on a new expedition to East Africa. Their object will be to make a thorough exploration and survey of the whole of the Kilimanjaro region.

RECENT issues of the journals published in French Indo-China, contain an interesting letter from M. Gauthier, describing a journey down the Meikong River, from Luang Prabang into Cambodia. The traveller spent forty days on the journey, and passed twenty cataracts, in one of which his boat was almost dashed to pieces. He visited the Laos States, and describes its inhabitants as doing nothing except laughing, smoking, and singing throughout the day, such business as there is being wholly in the hands of the Chinese.

OUR ELECTRICAL COLUMN.

GOUY has found that the attraction between two electrified surfaces maintained at a constant potential-difference is one hundred times greater in distilled water than in air.

ADMIRABLY well-equipped public electrical laboratories have been established in Paris and Vienna. When are we to see one in London?

VAN AUBEL (*Arch. de Genève*, xix. p. 105, 1888) has been studying the effect of magnetism and heat on the electric resistance of bismuth and of its alloys with lead and tin. Contrary to all other metals, the resistance of bismuth sometimes increases with reduction of temperature. He also verified the fact that the resistance of bismuth at low temperatures increases in the magnetic field. The effect is very feeble with alloys.

FOEPPL (*Ann. Wiedemann*, xxxiii. p. 492) has been endeavouring to prove Edlund's hypothesis that a perfect vacuum is a conductor, but has completely failed to do so. He makes the resistance of a vacuum to be three million times greater than that of copper.

MR. C. VERNON BOYS has communicated to the Royal Society some further details of his beautiful radio-micrometer. It is a thermo-electric circuit, consisting of a bar of antimony and bismuth, of small sectional area, the ends being formed by a loop of copper wire, suspended by a torsion fibre in a strong magnetic field. It is possible to observe by its means a difference of temperature of one ten-millionth of a degree Centigrade.

C. L. WEBER (*Centralblatt für Elektrotechnik*, 1887, vol. ix.), experimenting on various amalgams and alloys of tin, bismuth, lead, and cadmium, has found that many of them have a higher conductivity than that of each of their constituents.

SIRKS, of Deventer (Holland), has found a peculiar dynamical action of the current on the electrodes. An electrical current passing through a solution of $CuSO_4$ between two electrodes of copper, which are varnished at the back, pulls *both against* the direction of the positive stream. Independently of the concentration, if only high enough to prevent the formation of gases, the pressure at the anode and the traction at the kathode amount to nearly 1 gramme per ampere and per square metre.

ON THE COMPARISON OF THE CRANIAL WITH THE SPINAL NERVES.

THE origin of vertebrate animals is to be found according to many morphologists in those invertebrates which are composed of a series of segments, and one of the chief arguments in favour of this view has always been the fact that the spinal nerves are arranged segmentally. It has, however, long been felt that the cranial nerves ought to give evidence of a segmental arrangement as clearly as the spinal before it is possible to speak of a segmentation based upon the arrangement of the nervous system; and indeed many ingenious tables have been manufactured by morphologists in order to bring the cranial nerves into the same system as the spinal. The failure of these attempts is to my mind due largely to the following reasons:—

1. Confusion has arisen because anatomists have been in the habit of looking upon the nervous system of the vertebrate as composed of two separate nervous systems, viz. the cerebro-spinal and sympathetic.

2. In the comparison of cranial and spinal nerves the morphologists have directed their attention too exclusively to the exits of the nerves from the central nervous system without taking into account the place of origin of the nerves in the central nervous system itself.

3. It has been assumed on insufficient grounds that the presence of ganglia in connection with motor cranial nerves indicates that the cranial nerves do not follow Bell's law, and are therefore not strictly comparable with spinal nerves.

These difficulties are all found to vanish as soon as a clear conception is obtained of what is meant by the nerves of a spinal segment.

Since the time of Charles Bell it has been recognized that a spinal nerve is formed by two roots: the one, posterior, which contains only afferent fibres, *i.e.* fibres which convey impulses from the periphery to the central nervous system; and the other, anterior, containing exclusively efferent fibres which convey impulses from the central nervous system to the periphery. In correspondence with these two sets of fibres the grey matter of the spinal cord is divided into two portions, named respectively ths posterior and anterior horns. Another division, however, exists of almost equal importance, which is not so generally recognized, viz. a division both of the nerve fibres and their centres of origin in the grey matter for the purpose of supplying the internal and external portions of the body—a division of nerves and nerve centres into splanchnic and somatic as well as into afferent and efferent. The centres of origin of the splanchnic nerves are situated in the internal part of the grey matter of the spinal cord, being arranged in groups in the neighbourhood of the central canal, and the nerves themselves supply the viscera and internal surfaces of the body, together with certain muscles of respiration and deglutition which are derived from special embryonic structures known as the lateral plates of mesoblast. On the other hand, the centres of origin of the somatic nerves are situated in the outlying horns of grey matter, and the nerves themselves supply the integument and the ordinary muscles of locomotion, &c.,—muscles which are derived from the muscle-plates or myotomes.

Further, these two sets of nerves are arranged in the posterior and anterior roots in a special manner, the significance of which is the key to the whole question of the segmental nature of the cranial nerves. In the posterior roots the afferent fibres of both splanchnic and somatic systems pass into the spinal ganglion, which is always situated on the nerve root soon after its exit from the central nervous system; so that we may speak of the afferent fibres of both systems as being in connection with a ganglion which is stationary in position. In the anterior roots, on the other hand, we find that some of the fibres are in connection with no ganglia, while others are in connection with ganglia which are not fixed in position, but are found at various distances from the central nervous system (it is this system of ganglia which has hitherto been looked upon as forming a separate nervous system, viz. the sympathetic system), so that the fibres of the anterior root, all of which are efferent, are divisible into a ganglionated and a non-ganglionated group, of which the ganglionated group belongs to the splanchnic system, and is characterized by the smallness in the size of its fibres, while the non-ganglionated group is composed both of somatic and splanchnic nerves, and forms the ordinary large-sized motor nerve fibres of the voluntary striped muscles both of respiration and deglutition as well as of locomotion.

Again, it has been shown that these efferent ganglia are in reality offshoots from a primitive ganglion mass situated on the spinal nerves into which both afferent and efferent fibres ran.

We see, then, that both roots of a fully formed spinal nerve are ganglionated, so that the presence of a ganglion is no longer the sign of a posterior root, and we must define a spinal nerve as being formed by—

1. A posterior root, the ganglion of which is stationary in position and is connected with both splanchnic and somatic afferent nerves.

2. An anterior root, the ganglion of which is vagrant, and is connected with the efferent small-fibred splanchnic nerves.

Also it is not a fundamental characteristic of a spinal nerve that the anterior root should necessarily pass free from the spinal ganglion, for it is clear that both anterior and posterior roots may pass into the same stationary ganglionic mass if the whole or part of the efferent ganglion has not travelled away from the parent mass. This passage of the fibres of the anterior as well as of the posterior roots into the spinal ganglion is common enough in the lower animals, and is a peculiarity of the first two cervical nerves in such an animal as the dog. If, then, the cranial nerves are formed on the same plan as the spinal, their efferent roots ought to be divisible into a large-fibred non-ganglionated portion and a small-fibred ganglionated portion, the ganglia of which may be vagrant in character, while their afferent roots should possess stationary ganglia near their exits from the brain; also the centres of origin for the different sets of nerve fibres, *i.e.* for the splanchnic and somatic nerves, ought to be the direct continuation of the corresponding centres of origin in the spinal cord. Such I find to be the case; if we leave out of consideration the nerves of special sense, viz. the optic, olfactory, and auditory nerves, the remaining cranial nerves are found to divide themselves into two groups—

(1) A foremost group of nerves, which in man are entirely efferent, viz. third, fourth, motor part of fifth, sixth, and seventh nerves.

(2) A hindmost group of nerves of mixed character, viz. ninth, tenth, eleventh, and twelfth nerves, and the sensory part of fifth.

The nerves of the first group resemble the spinal nerves as far as their anterior roots are concerned, for they are composed of large-fibred non-ganglionated motor nerves and small-fibred splanchnic efferent nerves, which possess vagrant ganglia, such as the ganglion oculomotorii, the ganglion geniculatum, &c.

They resemble spinal nerves also as far as their posterior roots are concerned, for they have formed upon them a ganglion at their exit from the brain corresponding strictly to the stationary posterior root ganglion of a spinal nerve. One great difference, however, exists between their posterior roots and those of a spinal nerve, for neither the nerve fibres nor the ganglion cells of these roots are any longer functional; they exist simply in the roots of this group of cranial nerves in man, and other warm-blooded animals, as the phylogenetically degenerated remnants of what were in ages long since past doubtless functional ganglia and functional nerve fibres.

This foremost group of cranial nerves, then, is built up on precisely the same plan as the spinal nerves; the apparent difference being due to the fact that the afferent roots with their ganglia have degenerated.

The hindmost group of cranial nerves is also composed of the same constituents as the spinal nerves, and their different components arise from centres of origin in the medulla oblongata and in the cervical region of the spinal cord which are directly continuous with the corresponding groups of nerve cells in other parts of the spinal cord. Here, however, the deviation from the spinal nerve type which has taken place consists not in the suppression of any particular component, but in the scattering of the various components, so that none of the nerves of this group form in themselves complete segmental nerves, but rather the whole of them taken together form a broken up group of segmental nerves which are capable of being rearranged not only into afferent and efferent but also into splanchnic and somatic divisions of precisely the same character as in a group of spinal nerves.

I conclude therefore that both these two great groups of cranial nerves are built up on the same plan as the spinal nerves, not only with respect to the structure, function, and distribution of their nerve fibres, but also as far as the arrangement of the centres of origin of those nerve fibres in the central nervous system is concerned; and I think it probable that the reason for the deviation of the cranial nerves from the spinal nerve type is bound up with the changes which occurred at the time when a large portion of the fibres of the foremost group of cranial nerves lost their functional activity. I imagine that in the long past history of the vertebrate animal some extensive tract in connection with the foremost part of the nervous system has become useless and disappeared, and in consequence the nerves supplying those parts have degenerated. In this phylogenetic degeneration the whole of the splanchnic and somatic afferent nerves of that region were involved, and probably also some of the efferent nerve fibres, with the result that certain only of the motor elements have remained functional. In the further history of the vertebrate, the parts which have replaced those which became useless have received their nerve supply from tracts of the central nervous system situated behind this foremost group of nerves; in consequence of which the component parts of that hindermost group have become more or less separated from each other. The extent of the area involved is especially well seen when the sensory nerves of this area, both somatic and splanchnic, are considered; for we see not only that the sensory part of the trigeminal, representing the somatic sensory elements, and the sensory part of the vagus, representing the splanchnic sensory elements, are derived from their respective ascending roots, *i.e.* arise in connection with a series of nerve segments extending well into the cervical region, but also that the peripheral distributions of these two nerves are very extensive. Without speculating further at present upon the nature of the change which has disturbed the orderly arrangement of the cranial nerves, enough has been said to prove that the cranial nerves considered in this article are built up on the same plan as the spinal nerves. Further it is worthy of notice that just as the division into somatic and splanchnic has thrown great light upon the conception of the manner in which a segmental nerve is formed, so also it lends aid to the consideration of the segmentation of structures other than the nervous, for we find that two distinct segmentations exist in the body which do not necessarily run parallel to each other: the one, a segmentation which may be fitly called splanchnic, and is represented by the orderly arrangement of visceral and branchial clefts; and the other, a somatic segmentation, characterized by the formation of somites, *i.e.* of vertebræ and somatic muscles arranged also in orderly sequence.

The splanchnic segmentation is most conspicuous in the cranial region, the somatic segmentation in the spinal region, and it is most advisable to remember that a valid comparison between cranial and spinal segments can only be made when like is compared with like, for it by no means follows that the somatic and splanchnic segmentations have proceeded on identical lines; consequently, in comparing cranial with spinal nerves, we must compare structures of the same kind, and seeing that the spinal nerves are arranged according to somatic segments so also must the cranial nerves be arranged in accordance with their relation to the somatic muscles of the head, and not in relation to the branchial and visceral clefts.

It is not advisable in this article to enter upon any discussion as to the number of segments supplied by the cranial nerves, or to speculate upon the nature of the changes which have taken place in the past history of the vertebrate animal, whereby the present distribution of the cranial nerves has been brought about. I desire only to put as shortly as possible before the readers of NATURE the general results of my recent investigations into the structure of the cranial and spinal nerves.

W. H. GASKELL.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. T. C. Fitzpatrick, of Christ's College, has been appointed an Assistant Demonstrator of Physics.

Prof. H. M. Ward, M.A., of Christ's College, has been appointed Examiner in Botany in the place of Prof. Bayley Balfour.

Dr. R. D. Roberts has been appointed an Elector to the Harkness Scholarship.

The name of Mr. Adami, the new Demonstrator of Pathology, was misprinted Adams in our last issue.

SCIENTIFIC SERIALS.

Bulletin de l'Académie Royale de Belgique, February.—Researches on the colloidal state, by C. Winssinger. This is the first part of a memoir describing a series of experiments undertaken to determine the various conditions of the colloidal state—that is, of the state assumed under certain circumstances by bodies generally insoluble in water. For the present the author confines himself to describing the mode of preparation and the chief properties of the colloidal substances. All the fifteen sulphides studied by him (those of mercury, zinc, tungsten, molybdene, indium, platinum, gold, palladium, silver, thallium, lead, bismuth, iron, nickel, and cobalt) have been obtained in the colloidal state. They bring up to thirty-one the number of colloids now known to science. Some have been prepared by Graham's method, others directly by treating the oxides suspended in the water with hydrosulphuric acid.—On the pretended pro-atlas of mammals and *Hatteria punctata*, by Jules Cornet. The bony process between the occipital and the atlas known as the pro-atlas or proto-vertebra, and found in crocodiles and some other reptiles, is here shown not to exist in the mammals as supposed by some naturalists. The view of Smets regarding its absence from Hatteria is also confirmed.—On the process employed by the fresh-water Gasteropods for crawling over the liquid surface, by Victor Willem. This process is shown to be somewhat analogous to that of snails moving on dry land, being effected by secreting a mucus which enables the mollusk to adhere to the surface.—Researches on the volatility of the carbon compounds; chloro-oxygenated compounds, by Louis Henry. The object of these researches is to examine, in reference to their volatility, the compounds in which chlorine and oxygen are simultaneously combined with carbon. The subject is discussed under three heads: (1) the compounds comprising the system $>C-O$; (2) the system $\rightarrow C-OX$; (3) the mixed derivatives simultaneously including both these systems.

Rendiconti del Reale Istituto Lombardo, March 22.—Observations made in the Brera Observatory, Milan, during the total lunar eclipse of January 28, 1888, by G. V. Schiaparelli. These observations were made under favourable conditions in accordance with the instructions issued by the Pulkova astronomers, with the ultimate view of determining more accurately than has yet been possible the exact length of the diameter of the moon. In the accompanying tables are given the results of the observations, comprising the comparison-stars with their magnitudes and numbers as in the catalogue distributed by the Pulkova astronomers.

SOCIETIES AND ACADEMIES.

London.

Royal Society, March 22.—"The Chemical Composition of Pearls." By George Harley, M.D., F.R.S., and Harald S. Harley.

(1) As regards oyster pearls. Of these, three varieties were examined—British, Australian, and Ceylonese.

The qualitative analyses showed that they all had an identical composition, and that they consisted solely of water, organic matter, and calcium carbonate. There was a total absence of magnesia and of all the other mineral ingredients of sea-water—from which the inorganic part of pearls must of course be obtained. Seeing that ordinary sea-water contains close upon ten and a half times more calcium sulphate than calcium carbonate, one might have expected that at least some sulphates would have been found along with the carbonates, more especially if they are the mere fortuitous concretions some persons imagine them to be; a view the authors cannot indorse, from the fact that by steeping pearls in a weak aqueous solution of nitric acid, they are able to completely remove from them all their mineral constituents without in any way altering their shape, and but very slightly changing their naked eye appearances, so long as they are permitted to remain in the solution. When taken out they rapidly dry and shrivel up. Dr. George Harley will take occasion to point out in his next communication, which will be on the microscopic structure of pearls, that a decalcified crystalline pearl bears an intimate resemblance to a decalcified bone, in so far as it possesses a perfectly organized matrix of animal matter. No phosphates whatever were found in any of the three before-named varieties of pearls.

The next point being to ascertain the exact proportions of the substances composing the pearls, and pure white pearls being expensive, from having ascertained that all the three kinds they were operating upon had exactly the same chemical composition, instead of making separate quantitative analyses of them, they simply selected two pearls from each variety, of as nearly the same size and weight—giving a total of 16 grains—and analyzed them collectively, the result obtained being: carbonate of lime 91·72 per cent; organic matter (animal), 5·94 per cent; water 2·23 per cent.

(2) Composition of cocoa-nut pearls.

A portion of a garden pea sized cocoa-nut pearl, weighing 14 grains, was subjected to analysis, and found that, like shell-fish pearls it consisted of carbonate of lime, organic matter (animal), and water.

It had all the external appearances of the pearls found in the large clams (*Tridacna gigas*) of the Southern Ocean, being perfectly globular, with a smooth, glistening, dull white surface, and resembling them exactly in microscopic structure. Besides which in chemical composition it bore no similarity to cocoa-nut milk, to which it is supposed to be related; for cocoa-nut milk is said to contain both the phosphate and the malate, but not the carbonate of lime. That there are pearls found in cocoa-nuts the authors do not presume to deny; all they mean to say is that they are doubtful if the specimen examined had such an origin.

(3) As regards mammalian pearls.

These so-called pearls have been met with in human beings and in oxen.

In so far as naked-eye appearances are concerned, a good specimen of the variety of pearl now spoken of is quite undistinguishable from a fine specimen of Oriental oyster pearl, from its not only being globular in shape, and of a pure white colour, but from its also possessing the iridescent sheen so characteristic of Oriental oyster pearls of fine quality.

In chemical composition, however, mammalian pearls bear no similarity whatever to pearls found in shell-fish, for they are composed of an organic instead of an inorganic material—namely, cholesterin. In microscopic structure again, they bear a marked resemblance to the crystalline variety of shell-fish pearls.

April 19.—"On Hamilton's Numbers. Part II." By J. J. Sylvester, D.C.L., F.R.S., Savilian Professor of Geometry in the University of Oxford, and James Hammond, M.A. Cantab.

§ 4. *Continuation, to an infinite Number of Terms, of the Asymptotic Development for Hypothenusal Numbers.*

In the third section of this paper (Phil. Trans. A., vol. clxxviii. p. 311) it was stated, on what is now seen to be insufficient evidence, that the asymptotic development of $p-q$, the half of any hypothenusal number, could be expressed as a series of powers of $q-r$, the half of its antecedent, in which the indices followed the sequence $2, \frac{3}{2}, 1, \frac{3}{4}, \frac{5}{8}, \frac{1}{2}, \ldots$

It was there shown that, when quantities of an order of magnitude inferior to that of $(q-r)^{\frac{3}{4}}$ are neglected,

$$p-q=(q-r)^2+\tfrac{4}{3}(q-r)^{\frac{3}{2}}+\tfrac{11}{18}(q-r)+\tfrac{10}{81}(q-r)^{\frac{3}{4}};$$

but, on attempting to carry this development further, it was found that, though the next term came out $\frac{88}{1215}(q-r)^{\frac{5}{8}}$, there was an infinite series of terms interposed between this one and $(q-r)^{\frac{1}{2}}$.

In the present section it will be proved that between $(q-r)^{\frac{3}{4}}$ and $(q-r)^{\frac{1}{2}}$ there lies an infinite series of terms whose indices are—

$$\tfrac{5}{8}, \tfrac{9}{16}, \tfrac{17}{32}, \tfrac{33}{64}, \tfrac{65}{128}, \ldots$$

and whose coefficients form a geometrical series of which the first term is $\frac{88}{1215}$ and the common ratio $\frac{2}{3}$.

We shall assume the law of the indices (which, it may be remarked, is identical with that given in the introduction to this paper as originally printed in the *Proceedings* but subsequently altered in the Transactions), and write—

$$\begin{aligned} p-q &= (q-r)^2+\tfrac{4}{3}(q-r)^{\frac{3}{2}}+\tfrac{11}{18}(q-r)+\tfrac{10}{81}(q-r)^{\frac{3}{4}} \\ &+\frac{2^3}{3^3}A(q-r)^{\frac{5}{8}}+\frac{2^4}{3^4}B(q-r)^{\frac{9}{16}}+\frac{2^5}{3^5}C(q-r)^{\frac{17}{32}} \\ &+\frac{2^6}{3^6}D(q-r)^{\frac{33}{64}}+\frac{2^7}{3^7}E(q-r)^{\frac{65}{128}}+\text{\&c., } ad\ inf. \quad \ldots (1) \\ &+\Theta\,^* \end{aligned}$$

The law of the coefficients will then be established by proving that—

$$A=B=C=D=E=\ldots\ldots=\tfrac{11}{45}.$$

If there were any terms of an order superior to that of $(q-r)^{\frac{1}{2}}$, whose indices did not obey the assumed law, any such term would make its presence felt in the course of the work; for, in the process we shall employ, the coefficient of each term has to be determined before that of any subsequent term can be found. It was in this way that the existence of terms between

* In the text above, Θ represents some unknown function, the asymptotic value of whose ratio to $(q-r)^{\frac{1}{2}}$ is not infinite.

$(q - r)^{\frac{5}{8}}$ and $(q - r)^{\frac{1}{2}}$ was made manifest in the unsuccessful attempt to calculate the coefficient of $(q - r)^{\frac{1}{2}}$.

It thus appears that the assumed law of the indices is the true one.

It will be remembered that $p, q, r,$ . . . . are the halves of the sharpened Hamiltonian Numbers $E_n + 1$, E_n, $E_n - 1$, ... and that consequently the relation—

$$E_{n+1} = 1 + \frac{E_n(E_n - 1)}{1 \,.\, 2} - \frac{E_{n+1}(E_{n-1} - 1)(E_{n-1} - 2)}{1 \,..\, 2 \,...\, 3} + \;.\;.\;.\;.$$

may be written in the form—

$$p = \tfrac{1}{2} + \frac{q(2q-1)}{2} - \frac{r(2r-1)(2r-2)}{2 \,.\, 3} + \frac{s(2s-1)(2s-2)(2s-3)}{2 \,.\, 3 \,..\, 4}$$
$$- \frac{t(2t-1)(2t-2)(2t-3)(2t-4)}{2 \,.\, 3 \,...\, 4 \,..\, 5}$$
$$+ \frac{u(2u-1)(2u-2)(2u-3)(2u-4)(2u-5)}{2 \,...\, 3 \,.\, 4 \,.\, 5 \,...\, 6}$$
$$- \;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;. \qquad (2)$$

The comparison of this value of p with that given by (1) furnishes an equation which, after several reductions have been made in which special attention must be paid to the order of the quantities under consideration, ultimately leads to the determination of the values of A, B, C, ... in succession.

Physical Society, April 14.—Shelford Bidwell, F.R.S., Vice-President, in the chair.—Mr. W. E. Sumpner read a paper on the variation of the coefficients of induction. The author pointed out that there are three ways of defining the coefficient of self-induction of a circuit, expressed by the following equations—

$$(1)\quad e = L_1 \frac{dC}{dt}\;; \qquad (2)\quad N = L_2 C\;;$$
$$(3)\quad T = \tfrac{1}{2} L_3 C^2\;;$$

where e = back E.M.F. due to change of current, C = current, N = total induction through the circuit, and T the kinetic energy of the circuit. If the medium be air, L_1, L_2, and L_3 are identical, but in the case of iron this is no longer the case. When the curve of magnetization is given, their values, corresponding with any value of C, can be easily determined by the above equations. Maxwell's absolute method of measuring self-induction gives L_2, and by a modification due to Prof. Ayrton, where the current is altered from C_1 to C_2 instead of from 0 to $C = \frac{C_1 + C_2}{2}$, the value of L obtained is approximately L_1, if $C_1 - C_2$ is small compared with C. From the known character of the curves of magnetization of iron, it is easily seen that the value of L_2 increases with the current when the current is small, then becomes nearly constant, and afterwards decreases. For an electro-magnet having a horse-shoe core of best Swedish iron ½" diameter and 14" long, wound with 800 convolutions, the value of L_2 for currents between ·047 and ·107 amp. was found to satisfy the equation $L_2 = \frac{A}{5} + {\cdot}0425$, where A = current in amperes. A method of comparing self-induction with capacity is described, in which the arm of a Wheatstone's bridge opposite the one containing self-induction is shunted by a condenser of capacity K. The bridge is balanced for steady currents, and the deflection, θ_1, of the galvanometer observed on breaking the battery circuit. θ_1 is : : $L_2 - Kps$, where p and s are the resistances of the two remaining arms of the bridge. The condenser is then disconnected, and another swing, θ_2, obtained, on again breaking the battery circuit. θ_2 is : : L_2,

$$\therefore\; \frac{\theta_2}{\theta_1} = \frac{L_2}{L_2 - Kps}\;, \text{ or } L_2 = \frac{\theta_2}{\theta_2 - \theta_1} Kps.$$

Further experiments were made on the electro-magnet when its poles were joined by a piece of soft iron, the currents being reversed. The resulting values of L_2, $\mathfrak{B}$, $\mathfrak{H}$, and μ are given in absolute measure, and from them the author deduces—

$$L_2 = {\cdot}05 + 3{\cdot}9\,A, \qquad \mu = 210 + 720\,\mathfrak{H},$$
$$\mathfrak{B} = 210\,\mathfrak{H} + 720\,\mathfrak{H}^2, \quad \text{for values of A between ·06 and ·9.}$$

The difficulties experienced in determining the induction coefficients for strong magnetizing forces produced by the testing current are described. They arise chiefly from the fact that in order to obtain strong currents, the resistances must be small. This makes the "*time constant*" large, and in order to obtain the values of L in absolute measure, a ballistic galvanometer of very long period would be required. A method of calibrating a galvanometer of comparatively short period to give approximate results is described. Where the magnetizing force is produced by an independent coil, no such difficulties present themselves. Results obtained for the coefficients of self-induction of a gramme armature (A type) for different currents round the field magnets vary from ·0218 for current 0 to ·0117 for a current of 29 amperes. The value of L for a given point on the curve of magnetization is not a definite quantity, but has always two or more distinct values, depending on whether the magnetization is increased or decreased by the test currents, and on the previous history of the iron. That this must be the case is easily seen from the curves obtained by Prof. Ewing in his "Experimental Researches on Magnetism." The values of L corresponding to the three sides of a small Ewing's cycle are denoted by L_p (*progressive coefficient*), L_r (*return coefficient*) and L_c (*cyclic coefficient*). L_p is always the largest, whether the magnetization be increased or decreased by the testing current. Numerical values of L_p and L_c obtained from a Kapp and Snell transformer are given. L_c can be very accurately determined by Profs. Ayrton and Perry's secohmmeter, and some of the results given in the paper were thus obtained. Having given the curve of magnetization and that connecting impressed E.M.F. and time, a simple graphical method is described for drawing the current curve. Applying this to an alternating current where the E.M.F. is a pure sine function of the time, it is shown that the resulting current curve differs considerably from a sine curve. The case of the rise of current in the magnet coils of a dynamo excited by accumulators is also discussed, the derived curves being in accordance with observation. In conclusion the author pointed out that the time taken to discharge a condenser through a given resistance may be decreased by adding self-induction to the circuit, provided L is less than $\frac{1}{2}KR^2$. When $L = \frac{1}{4}KR^2$, the discharge is completed in one-half the time required when L = 0. This may account for the remarkable results observed by Dr. Lodge in his experiments on iron and copper as lightning-conductors.—Mr. C. V. Boys described and performed some experiments on soap-bubbles, and by their aid demonstrated in a remarkable manner the phenomena of surface tension, diffusion, and the magnetic properties of gases. By blowing one bubble inside another, he showed that there is no electrical force inside a closed conductor. A peculiar property of soap-bubbles is their refusal to come into contact when knocked against each other; they may receive violent shocks and still remain separate. If, however, an electrified body be brought in the vicinity, they immediately coalesce. So sensitive are they to electrical attraction that a potential difference due to one Leclanché cell between the two bubbles causes them to unite. They may thus serve as very delicate electroscopes. Many other beautiful and extremely interesting experiments on liquid films of different shapes were performed in a masterly manner.

Geological Society, April 11.—W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—On the lower beds of the Upper Cretaceous series in Lincolnshire and Yorkshire, by W. Hill.—On the Cae Gwyn Cave, North Wales, by Dr. Henry Hicks, F.R.S.; with an appendix by C. E. De Rance. The author gave an account of the exploration of the cavern during the latter part of 1885, and during 1886–87. He considered that the results obtained during that time proved conclusively that there was no foundation for the views of those who contended that the drift which covered over the entrance and extended into the cavern was *remanié*, but they proved that the deposits which lay over the bone-earth were *in situ*, and were identical with the normal glacial deposits of the area. These deposits had once extended continuously across the valley, and the cavern (400 feet above Ordnance datum) had consequently been completely buried beneath them. The cave must have been occupied by animals during the formation of the bone-earth, before any of the glacial deposits now found there had accumulated, and a thick floor of stalagmite had covered this "earth" before the cavern had been subjected to water-action. This action had broken up the floor, and completely re-sorted the materials, and added sandy and gravelly material to the deposits; this sand and gravel had been examined by Prof. Boyd Dawkins, who found that it agreed in every particular with the glacial sand

and gravel occurring in the valley a little way above. The large limestone blocks in the cavern had also been evidently disturbed by water-action; they were invariably found in the lowest deposits, and were covered over by laminated clay, sand, and gravels. The author considered it certain that the caverns had been completely filled with these materials, and in the case of the Cae Gwyn Cave they appeared to have been conveyed mainly through the entrance recently discovered under the drift. The stratification at this entrance was so marked, and could be traced so continuously inwards over the bone-earth, that there could be no doubt that this was the main entrance. There was not the slightest evidence that any portion of the material had been conveyed in through a swallow-hole, and the conditions witnessed throughout were such as to preclude any such idea. The author quoted a Report by Dr. Geikie, who considered that the wall of the cavern had given way, but before the deposition of the glacial deposits, which were subsequently laid down against the limestone bank so as to conceal this entrance to the cavern. In conclusion, he referred to the presence of reindeer remains in these caves, in conjunction with those of the so called older Pleistocene Mammalia, proving that these had reached the area long before the period of submergence, and evidently at an early stage in the Glacial period. It was important to remember that reindeer remains had been found in the oldest river-gravels in which implements had been discovered. Man, as proved by the implements discovered, was also present at the same time with the reindeer, and it was therefore natural to suppose that he migrated into this area in company with that animal from some northern source, though this did not preclude the idea that he might also have reached this country from some eastern or southern source, perhaps even at an earlier period. In the course of the discussion which followed the reading of this paper, Dr. Evans said the archæological evidence was against Dr. Hicks's views.

Chemical Society, April 19.—Mr. W. Crookes, F R.S., in the chair.—The following papers were read:—The influence of temperature on the composition and solubility of hydrated calcium sulphate and of calcium hydroxide, by Messrs. W. A. Shenstone and J. T. Cundall. The authors find, contrary to the usual statements on the subject, that hydrated calcium sulphate, whether of natural or of artificial origin, parts with a portion of its water at moderate temperatures, *e.g.* 40° C., and that it may be almost completely dehydrated in dry air at temperatures below 100° C. The effect of heat in diminishing the solubility of calcium sulphate in water at temperatures between 40° and 150° may therefore be possibly due to the unequal solubility of the hydrated and anhydrous salts. Calcium hydroxide is likewise less soluble in hot than in cold water, but the authors have failed to obtain evidence in favour of the view that the diminished solubility in this case may depend upon the dissociation of the hydroxide or of some hydrate of the hydroxide.—Thermo-chemical constants, by Mr. S. U. Pickering. In a criticism of several deductions drawn by Thomsen from thermo-chemical data, the author refers to the supposed "common constant of affinity"—a quantity whose multiples by numbers up to 10 are supposed to represent various reactions, some of which are similar, and others totally dissimilar (*Ber. Deutsch. Chem. Ges.*, v. 170, vi. 239); and points out that any number taken at random, *e.g.* 15,000 cal., would have given results similar to those obtained by employing Thomsen's value of the constant, viz. 18,361 cal.—Action of hot copper on the mixed vapours of phenol and carbon bisulphide, by Prof. T. Carnelly and Mr. J. Dunn. A small yield of a new diphenylene ketone (m.p. = 83°) is obtained in this reaction.—Oxidation of oxalic acid by potassium bichromate, by Mr. E. A. Werner.—The action of phenylhydrazine on urea and on some of its derivatives, by Mr. S. Skinner and Dr. S. Ruhemann.—Derivatives of phenylisobutyric acid, by Dr. L. Edeleanu.—The logarithmic law of atomic weights, by Mr. G. J. Stoney, F.R.S.

Zoological Society, April 17.—Dr. St. George Mivart, F.R.S., Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of March 1888.—Mr. C. Stewart exhibited a preparation showing the structure and development of the brood-pouch of a Marsupial Tree-Frog (*Nototrema marsupiatum*).—Mr. Boulenger exhibited and made remarks on the type specimen of a new species of Marsupial Tree-Frog (*Nototrema fissipes*) recently discovered by Mr. G. A. Ramage near Pernambuco, in Brazil.—Mr. Herbert Druce read the descriptions of some new species of Heterocera collected by Mr. C. M. Woodford at Suva, Viti Levu, Fiji Islands. The collection had been made during the months of February, March, and April, 1886, and was especially interesting on account of the exact localities being noted, as well as for the new species it contained. Ninety-four species were represented, eight of which were described by the author as new to science.—A communication was read from Mr. T. D. A. Cockerell, containing some remarks on atavism, with reference to a paper on the same subject read by Mr. J. Bland Sutton at a previous meeting of the Society.—Prof. G. B. Howes gave an account of the vocal pouch of *Rhinoderma darwini*, and described in detail the mode of its attachment and the position of the embryos in it. The author controverted the idea of Espada that the alimentary functions were arrested during the development of the embryos in this Batrachian.—Mr. Oldfield Thomas read a paper describing a new genus and species of Muridæ obtained by Mr. H. O. Forbes during his recent expedition to New Guinea. The author proposed to call this form, which was characterized by the possession of a prehensile tail, *Chiruromys forbesi*, after its discoverer.—Lieut.-Colonel Godwin-Austen, F.R.S., read the first of a proposed series of papers on the Land-Mollusca of Burma. The present communication gave an account of the shells collected by Capt. Spratt, R.A., in Upper Burma, among which were specimens of several new and very interesting species. A communication was read from Mr. R. Bowdler Sharpe, containing the sixth of his series of notes on the specimens of the Hume collection of birds. The present paper treated of some of the species of the genus *Digenea*.

Anthropological Institute, April 24.—Francis Galton, F.R.S., President, in the chair.—A paper by Dr. Venn on recent anthropometry at Cambridge was read, and was followed by a communication by the President on the head-growth of Cambridge students. The President's paper we print elsewhere. Mr. Galton also read a paper on the answers he had received from teachers in reply to questions respecting mental fatigue.

Paris.

Academy of Sciences, April 23.—M. Janssen, President, in the chair.—Influence of gravity on the co-ordinates measured by means of equatorials, by MM. Lœwy and P. Puiseux. The paper deals mainly with the equatorials *coudés*, such as the large instrument intended for the Paris Observatory, and gives the general formulas of reduction.—On the aperiodic regulation of the amortisement and of the phase in a system of synchronized oscillations, by M. A. Cornu. The principle is explained of this aperiodic method of control, which is shown to possess several advantages over the systems at present in use. It reduces to a minimum, if not to zero, the influence of the more ordinary disturbing causes, and supplies a continuous check for the regulating apparatus as well as a simple means of readjustment should it get out of order.—Remarks on M. Stoletow's recent communication on a class of electric currents set up by the ultraviolet rays, by M. Edm. Becquerel. The note referred to the passage of an electric current between two disks, or metallic conductors, placed parallel to, and at a little distance from, each other, by means of the layer of intervening air, which requires to be more or less heated by the radiation of a voltaic arc. M. Becquerel points out that these effects appear to be analogous to those which he observed and analyzed in a different way in the year 1853. He then showed that heated gases may conduct electric currents, these effects being functions of the nature and density of the gases, as well as of the relative dimensions of the electrodes.—On the fixation of nitrogen by vegetable soil, by M. Berthelot. This is a reply to M. Schlœsing's recent remarks, the main object of the note being to more clearly establish the history of these researches and their present character.—On the optical properties of natural pharmacolite, by M. Des Cloizeaux. The author, having recently resumed his interrupted studies of this crystal, finds that its optical crystallographic properties are absolutely identical with those of the artificial crystals lately obtained by M. Dufet. The only difference is an excess of about 4 per cent. of water as determined by previous analyses of the natural crystals. But these crystals are hygrometric, and lose some of their water at 100° C. The specimens analyzed were also probably mixed with a little wapplerite, which has yielded as much as 29 per cent. of water, and which in the state of an efflorescent powder is usually associated with pharmacolite.—Note on the optical characters of haidingerite, by M. Des Cloizeaux. An examination of some small specimens of this extremely rare crystal found in association with a few fragments of pharmacolite shows that it must be grouped with the family of the positive acute bisector crystals. One of its indices of refraction, formerly measured by Haidinger on a natural prism of 40°, formed by two opposite facets, *h'* and *m*, must be the maximum index, $\alpha = 1·67$.—Observations of Palisa's new planets 275 and 276, made at the Observatory of Algiers, by MM. Trépied, Rambaud, and Sy. These observations, which were made with the 0·50 m. telescope, cover the period April 17-18, when the two planets were of the respective estimated magnitudes 11 and 11·5.—On the employment of gas thermometers, by M. Crafts. These remarks are made in connection with the hydrogen instrument recently described by M. Cailletet, who mentions an analogous type of thermometer devised ten years ago by M. Crafts.—On a new system of telephonic communication between trains in motion and the neighbouring stations, by M. P. Germain. A series of electric measurements effected on rails from the stand-point of their resistance, insulation, and diffusive electric power, has satisfied the author that the two metallic parts of the same line connected together constitute an excellent conductor, provided the circuit and pile be insulated from earth. He has established curves of resistance for the rails according to the variations caused by the temperature and by the humid condition of the ballast. A new line shows less resistance than an old, owing to the oxidation of the points and the slow transformation brought about in the molecules of steel under the influence of vibration. By setting up the necessary apparatus in the stations and in the guard's van, telephonic correspondence may be carried on in both directions; but the details of the process are for the present withheld.—On a new fossil fish of the Commentry (Allier) Coal-measures, by M. Charles Brongniart. This fish, of which several good specimens have been found, presents peculiarities distinguishing it from all other fishes extinct or living. It is here consequently constituted a separate order of Pleuracanthides, as the prototype of the star-fish, Ceratodus, and allied forms. The present specimen is named *P. gaudryi*, in honour of M. Albert Gaudry.

BERLIN.

Physiological Society, April 13.—Prof. Munk, President, in the chair.—Prof. Gad made a complementary communication to his previous one dealing with the proof of the Wallerian law. His experiments were carried out, in conjunction with Dr. Joseph, on the vagus nerve and its jugular ganglion. The nerve was cut through either on the central or peripheral side of the ganglion, and after six or eight weeks degeneration was looked for in the ganglion and nerve. These experiments yielded only a general confirmation of Waller's law; at the same time they brought to light so many peculiarities and divergencies, that, even with the help of physiological experiment, it was found impossible to deduce any universal laws from the details communicated to the Society.—Dr. Baginski spoke on the Bacteria normally present in the fæces of children which are being fed on the milk of the mother. As is well known, Eschricht has distinguished two kinds among the above, viz. *Bacterium lactis* and *Bacterium coli*; of these the first is said to be capable of inducing the lactic fermentation of milk-sugar. The speaker had investigated the truth of this statement by cultivating the *Bacterium lactis*, with all needful precautions, in a solution of milk-sugar to which neither peptone nor any other nutrient fluid had been added. When the fermentation was at an end, the fluid was strongly acid, but no lactic acid, or at most the minutest trace of this acid, could be discovered in it: all the reactions which it did yield pointed to the presence in it of acetic acid. This *Bacterium lactis* (which should now rather perhaps be called *Bacterium aceti*) produced no effect on casein or any other proteid, and no putrefactive change was induced. Similarly it had no action on starch paste. Bearing in mind the practical medical interest which attaches to fermentative processes which may occur in the alimentary canal of children at the breast, Dr. Baginski had next investigated the behaviour of the Bacterium and the nature of the fermentation it produces when deprived of air and oxygen, and found that the fermentation was in all respects the same as that which takes place with access of air. The gaseous products of the fermentation were carbonic acid gas, hydrogen and marsh-gas. From among the various substances whose action on the Bacterium was tried, it is sufficient to mention that acetic acid very speedily killed it, so that no growth of the organism was observed in gelatine made acid with the product of its own activity. This product therefore plays the part of an active poison as regards the further life of the organism.—Dr. Mertsching spoke on the histology of the skin and hairs, and in some detail on the mode of origin of horny growths. The speaker exhibited a large number of preparations in support of his views.

AMSTERDAM.

Royal Academy of Sciences, March 31.—Mr. Martin stated that he had been charged by Mr. van Lansberge, late Governor-General of Dutch India, to present to the Leyden Museum a portion of a jaw of a gigantic Ichthyosaurus from the south coast of Ceram. From this fossil the existence of Mesozoic strata in that island may be inferred; and the fact that in British India and in Australia remains of the same animal have been found in the Chalk suggests that in Ceram also there may be a Cretaceous formation. The statement made in Berghaus's Physikalischer Atlas, to the effect that a Palæozoic formation is to be found on the south coast of Ceram, is without foundation.

BOOKS, PAMPHLETS, and SERIALS RECEIVED FOR REVIEW.

The Australian Race, 4 vols.: E. M. Curr (Trübner).—Abhandlungen und Berichte des K. Zoologischen und Anthropologisch-Ethnographischen Museums zu Dresden: Dr. A. B. Meyer (Friedländer).—Diamagnetism and Magne-Crystallic Action; New Edition: John Tyndall (Longmans).—Silk-worms: E. A. Butler (Sonnenschein).—A Treatise on Hydrodynamics, vol. i.: A. B. Basset (Deighton, Bell, and Co.).—Publications of the Lick Observatory of the University of California, vol. i., 1887 (Sacramento).—Methodik der Gesamten Naturwissenschaft: K. Kollbach (Leipzig).—Turbans and Tails: A. J. Bamford (Low).—Antipodean Notes: Wanderer (Low).—Lights and Shadows of Melbourne Life: J. Freeman (Low).—The Land of the Pink Pearl: L. D. Powles (Low).—The Birds of Dorsetshire: J. C. Mansel Pleydell (Porter).—Argentine Ornithology; A Descriptive Catalogue of the Birds of the Argentine Republic, vol. i.: P. L. Sclater and W. H. Hudson (Porter).—Dr. H. G. Bronn's Klassen und Ordnungen des Thier-Reichs; Erster Band, Protozoa: Dr. O. Bütschli (Williams and Norgate).—Mémoire sur la Théorie de la Figure des Planètes: M. O. Callandreau.—Bulletin de l'Académie Royale des Sciences de Belgique, No. 3, 1888 (Bruxelles).—Transactions of the New York Academy of Sciences, vol. vi. (New York).

CONTENTS.

THURSDAY, MAY 10, 1888.

FORMS OF ANIMAL LIFE.

Forms of Animal Life. A Manual of Comparative Anatomy, with Descriptions of Selected Types. By the late George Rolleston, D.M., F.R.S., Linacre Professor of Anatomy and Physiology in the University of Oxford. Second Edition, Revised and Enlarged by W. Hatchett Jackson, M.A., Natural Science Lecturer, St. John's College, Oxford. (Oxford: Clarendon Press, 1888.)

THE first edition of Prof. Rolleston's "Forms of Animal Life" was published in 1870. Avowedly an educational work, and written expressly for students, it came at a time when the teaching of zoology was in a very different position from that which it now holds, and opportunities for systematic laboratory instruction were rare.

At Oxford there already existed an admirably equipped Museum, in the arrangement of which the wants of students received special attention; facilities for laboratory work were also offered, and among the Linacre Professor's pupils were men destined to become the leaders of the younger school of English zoologists. Elsewhere, however, the conditions were less favourable. The Cambridge school of biology, which has made for itself so great and honourable a reputation, as yet had no existence. Indeed, it was not till the year of publication of Prof. Rolleston's volume that the Trinity Prælector in Physiology entered on the duties of his new office; and it was in October of the same year that the late Prof. Balfour commenced his brilliant University career.

In other centres the state of things was very similar. Zoology was taught almost exclusively by lectures, often indeed of great value, but supplemented at most by demonstrations. Individual students worked hard at dissections or in museums, but organized laboratory instruction, in direct connection with systematic lectures, existed on a very small scale, if at all.

There was, however, a firm conviction on the part of those most directly and intimately concerned, that a great change was necessary; and a determination to carry out this reform at the earliest possible opportunity. In 1872, Prof. Huxley entered into possession of the new Biological Laboratories at South Kensington, and at once inaugurated a system of combined lecture and laboratory instruction which has formed the model on which all subsequent courses have been framed. Three years later he published, in conjunction with Prof. Martin, the "Course of Elementary Instruction in Practical Biology," and from that time the teaching of biology by lectures only became impossible.

This same year, 1875, witnessed the commencement of Prof. Balfour's systematic courses of practical morphology at Cambridge, and the introduction, by its newly elected Professor of Zoology, of the reformed system into one of the most eminent of the London medical schools. The change spread rapidly throughout the country, and the adoption of the new methods of teaching, pushed to its logical conclusion, led to the establishment of numerous appointments, and to the building and equipment of the splendid Laboratories at Cambridge, Manchester, and elsewhere.

It would not be wise to attempt to estimate too accurately to what extent Prof. Rolleston's book was instrumental in bringing about this reform, by which the whole scope and method of biological teaching were altered. It must be noted, however, that the time of its appearance was most opportune, and that the two leading principles of the book, in which it differed most markedly and most deliberately from all other works of the time, were precisely the characteristic features of the new school. These are, in the first place, the insistence on accurate and practical examination of selected types before a student is allowed to proceed to the systematic study of the groups to which the types belong; and, secondly, the importance of direct reference to the original sources of information. On the first of these points, Prof. Rolleston says, in his preface:—

"The distinctive character of the book consists in its attempting so to combine the concrete facts of zootomy with the outlines of systematic classification as to enable the student to put them for himself into their natural relations of foundation and superstructure. The foundation may be made wider, and the superstructure may have its outlines not only filled up, but even considerably altered, by subsequent and more extensive labours; but the mutual relations of the one as foundation and of the other as superstructure, which this book particularly aims at illustrating, must always remain the same."

On the importance of direct reference to the original authorities he speaks very positively:—

"In some cases, even the beginner will find it necessary to consult some of the many works referred to in the descriptions of the preparations and in the descriptions of the plates; but the bibliographical references have been added with a view rather to the wants indicated in the words 'Für akademische Vorlesungen und zum Selbststudium,' so often prefixed to German works on science, than to those of the commencing student."

"Forms of Animal Life" was the first student's text-book in which these principles were distinctly formulated and deliberately adopted; and there can be no doubt that it played a most important part in stimulating and enforcing a direct study and accurate acquaintance with type forms as a necessary prelude to systematic zoological work: just as the admirable series of preparations by Mr. Robertson, the description of which forms so characteristic and important a feature of the book, have furnished a model from which other museums have copied freely and to their great advantage.

Prof. Rolleston took great interest in his book: during the later years of his life he was actively engaged in preparing the second edition; and very early in this work he asked Mr. Jackson to act with him as joint author. Some progress was made in this joint work, but it was soon interrupted by the illness which, in the winter of 1880, compelled Prof. Rolleston to go abroad, and which proved fatal only a few months later.

"When Prof. Rolleston went abroad," says Mr. Jackson, "he put me in possession of his plans for the rest of the work, handed his papers to me, and expressed a hope that, if he were disabled from completing the new edition, I might be the person to do it in his stead. It is almost needless for me to add that in fulfilling this sacred trust

I have endeavoured to carry out his wishes, which were mainly three: (1) to enlarge the descriptions of the preparations and accounts of the various classes of animals, and to bring them to the standard of contemporary knowledge; (2) to add to each class or group a brief classification; and (3) to give as full a bibliography as space would admit."

The new edition which is now before us has been most carefully revised throughout; very considerable additions have been made, especially in the systematic portion, which has been entirely re-written by Mr. Jackson; and the volume is more than double the size of its predecessor—extending to upwards of 900 pages.

The book, as before, consists of three main sections: the descriptions of the selected preparations; the descriptions of plates illustrating the salient features in the anatomy of certain of these types; and, thirdly, the systematic accounts of the several groups into which the animal kingdom is divided. The arrangement of these sections has been altered; for while in the former edition the descriptions of the preparations and plates were placed after the systematic part, the relative positions have in the new edition been reversed. The present arrangement is a more natural one, and the change, which was contemplated by Prof. Rolleston, is certainly an improvement.

The selected preparations, the description of which forms the first section of the book, are for the most part the same as those of the former edition. The skeleton and certain parts of the muscular system of the rabbit, and the alimentary canal, urinary, and generative organs of the same animal, have been added; the privet hawk moth has been substituted for the death's head; and the skeleton of the pigeon and a few invertebrate preparations have been omitted. Though the number of the preparations remains practically the same as before, this portion of the book has been increased by nearly a hundred pages; the expansion being due mainly to the insertion of much fuller accounts of allied forms, and partly to a large addition in the bibliography.

It would be an easy matter to take exception to the plan of this part of the book, and to urge that the space devoted to the description of particular specimens, which the majority of readers can never have a chance of seeing, might have been allotted, with far greater advantage to students, to thorough descriptions of the anatomy of typical animals selected as representatives of the several groups. Accounts such as these are much wanted, and the opportunity for providing them was an exceptionally favourable one. The criticism, however, loses all point as directed against this second edition, for Mr. Jackson, regarding his task as a trust, has rightly refrained from interfering with the scheme of arrangement of this, perhaps the most characteristic section of the book.

He has, however, subjected the whole to very careful revision. The descriptions are admirably clear and concise, and the additional paragraphs have given Mr. Jackson opportunity for introducing references to allied forms which are always important, and in many cases of very high value indeed.

The second part of the book, containing the plates with their descriptions, is less satisfactory. Of the twelve plates of the first edition ten have been retained without change, one has been slightly altered, and one cancelled. Three new plates, which had been prepared and completed under Prof. Rolleston's own direction, have been added, illustrating points in the anatomy of the skate, of the oyster, and of certain Arthropoda respectively. We sincerely wish these plates had been omitted. They form no essential part of the book; the subjects are not well chosen; and the drawings themselves are not always correct. The figure of the reproductive organs of the earthworm, for instance, is very misleading; and the nephridia, as shown in the same figure, are entirely wrong. The new plates show no improvement on the old ones: the figure of the oyster is not of sufficient importance to justify its insertion, while the plate supposed to illustrate the anatomy of the skate is one of the very worst we have ever seen. We cannot but feel the highest respect for the conscientious and self-effacing spirit in which Mr. Jackson has carried out a most laborious and delicate task; but we believe most sincerely that he would have done more honour to the memory of his chief by suppressing most if not all of these plates, which are in every way unworthy of the book and of its authors. From the fact that this part of the book has alone undergone compression, we suspect that Mr. Jackson, who has no responsibility in connection with the plates save that of retaining them, agrees with us as to their merits.

About a dozen woodcuts have been inserted in the descriptions of the preparations: these are well chosen and will prove useful, though the absence of descriptions in two or three cases is somewhat exasperating. At the present time accurate and original figures illustrating the anatomy of typical animals are so urgently needed that we cannot but regret that the resources of the Clarendon Press were not drawn on more largely in this respect.

The third and concluding portion of the book contains the systematic descriptions of the groups; and here the changes are very great indeed. Occupying less than two hundred pages in the former edition, it has now increased to six hundred. This part of the book is by far the most important, and is exceedingly well done. Short descriptions of the larger groups are followed by most accurate and comprehensive accounts of the several classes. The further subdivision of the classes into orders and other minor groups is given in all cases; and the most recent researches are referred to, without being given undue prominence.

For this part of the work Mr. Jackson is entirely responsible, and we congratulate him very heartily on the admirable manner in which he has effected it. We have indeed but one complaint to make—namely, that, as in the former edition, the groups are described in descending order, Vertebrates being taken first, and Protozoa last. This is a serious fault, giving the effect of an uncomfortable drop as we pass from group to group, and, furthermore, rendering discussion of the mutual relations of the several groups very difficult, and in many cases futile or impossible.

Apart from this, we have nothing but praise to offer. Limits of space will not allow that we should deal at length with the several classes, but a few points may be noted.

The Enteropneusta are left among the "Worms"; their vertebrate affinities are mentioned, though Mr.

Jackson does not appear to favour their claims to rank among the higher group. The vexed question of the homologies of the Arthropod appendages is treated fully The antennules of Crustacea are doubtfully classed as true appendages, while the Crustacean antennæ, with the cheliceræ of Arachnida are regarded as post-oral appendages which have become pre-oral by shifting forwards. The antennæ of Myriapods and Insects are ruled out, "as being apparently processes of the procephalic lobes;" while the suggestion that the telson represents a region rather than a somite will meet with very general approval.

Brauer's classification of Insects is adopted, with some slight modifications, and is given in considerable detail. The leeches are treated with caution as an isolated group, and no suggestion is made of their possible affinities with Turbellaria.

Among the lower groups the Cœlenterata are dealt with very thoroughly. The possibility of near kinship between the Acraspedote Medusæ and the Anthozoa that has found favour of late with Götte and others is mentioned, but rejected. The Protozoa also receive very liberal and thorough treatment, more than a hundred pages being devoted to them. As regards classification three main divisions are adopted: the Acinetaria, Ciliata and Mastigophora are classed together as Plegepoda, a group equivalent to the Stomatophorous Corticata of Lankester, and for which the old term Infusoria might conveniently be used. The remaining divisions are the Endoparasita or Sporozoa, and the Rhizopoda, the latter group being equivalent to Lankester's Gymnomyxa.

Mr. Jackson is a singularly modest writer, and seldom allows his own hand to be seen; a note on the blood-vessels of the earthworm, in which he questions the existence of the so-called subintestinal vessel, is of considerable interest; and throughout the volume there is abundant evidence of intimate practical acquaintance with the groups he describes so well.

The importance, even for the junior student, of direct reference to original papers was, as we have noticed above, one of the points on which Prof. Rolleston insisted most strongly. In this respect Mr. Jackson has afforded assistance of a singularly efficient character. Possessed of a most unusually accurate and extensive acquaintance with the zoological literature of all countries, Mr. Jackson has given the full benefit of his knowledge to readers of his book. Every page teems with evidence of the most diligent research amongst authorities, and none but a specialist in each group can estimate rightly the enormous amount of labour that its preparation must have cost him. Only less admirable is the restraint which has enabled him to refrain from burdening the book with an undue number of references, while those that are given have been selected with the utmost care, and arranged in such way as to afford the student aid of a kind hitherto denied him. "The method I have adopted," says Mr. Jackson, "is to cite the most important and recent authorities, which, when consulted, will in most cases give the names of all other accounts worth reading, so as to form a really very complete index to the state of present knowledge." It is this "index" which constitutes the characteristic feature of the new edition; and in the care and thoroughness with which he has compiled it, Mr. Jackson has conferred an inestimable boon on zoologists, and has rendered his work indispensable to teachers and students alike.

The earlier edition of "Forms of Animal Life" was marked by a certain singularity, at times almost grotesqueness, of diction, which interfered to some extent with the popularity of the book; we are glad to observe that care has been taken to remove this blemish, though an occasional tendency to reversion may be noticed in such statements as that "the anterior prostate is divaricable into two lobes," or that a given figure is "one-half less than natural size."

It would be better, too, if zoologists could completely emancipate themselves from the traditions of human anatomy, and cease to speak of the anterior part of a rabbit as the "upper half," or to use such terms as "vena cava descendens." "Uro-genital," too, which threatens to establish itself permanently, should not be used for urino-genital; and the term "pseud-hæmal" is objectionable, and, as applied to the vascular system of an earthworm, meaningless.

However, these are but small points; and in concluding we acknowledge in the fullest degree the singularly painstaking and conscientious manner in which Mr. Jackson has fulfilled his task, and the signal service he has thereby rendered to zoologists. "Forms of Animal Life" is a unique book; none but Prof. Rolleston could have written it; and probably there is no one who could have retained and developed more successfully than Mr. Jackson has done the exactness and thoroughness to which Prof. Huxley long ago alluded as its special charm.

A M. M.

THE CARDINAL NUMBERS.

The Cardinal Numbers, with an Introductory Chapter on Numbers Generally. By Manley Hopkins. London: Sampson Low, 1887.)

UNLIKE Hudibras, who could, as we are told, "extract numbers out of matter," Mr. Hopkins proposes in the essay before us to extract matter from numbers, or, as he says in the preface, "to show that every-day things—numbers being one of them—possess in themselves materials worth investigation, and connections with other subjects of thought and study." Our author does not attempt any systematic investigation of the properties of numbers: to do so would far transcend the modest limits to which he confines himself. He prefers to consider numbers in their relation to such subjects as religion, music, poetry, mythology, and superstition. Some purely numerical facts are, however, given, which either are, or else ought to be, found in every text-book of arithmetic—for instance, the rules (given on p. 75, at the beginning of the appendix) for determining when a number is divisible by any of the first twelve numbers, 7 only excepted. The cardinal numbers from 1 to 10 inclusive are treated separately in ten distinct chapters. These, with the introductory chapter and an appendix, the principal portion of which is taken up with magic squares, form the whole of the work.

The nature of our author's remarks will be best seen by making a few quotations. Thus in the chapter on Number One, after speaking of the unity of the Godhead and the oneness of self, he goes on to say:—

"Geography and natural history abound in words which express the separateness of an object, its isolation, its *one-liness*. Similar to the number 1, and to the pronoun I, there are found in different languages and dialects referring to local separation, the words *i*, *hi*, *ey*, *eye*, *egg* (and here think of the Latin *ego* and the Greek ἐγὼ), *eyot*, *ait*, *inch*, *innis*, *île*, *isle*, *inver*, *insula*, *isola*, *isla*; and connote some of these with the animal *eye* and *egg*, having a similar separation as an island in geography. All the latter have the same meaning, and express a portion of land segregated, cut off from other land and surrounded by water—oneness."

With the above we may compare Shakespeare's use of the word *eye* in the passage—"The ground, indeed, is tawny. With an eye of green in 't" ("Tempest," Act II., sc. 1).

Respecting the celebrated *twos* in profane myth and history, we read in the next chapter:—

"Prominent among these are Romulus and Remus; Brutus and Cassius; and in Irish legend Eber and Airem; concerning whom we are informed that Eber was slain by his brother Airem. He was the hero of the Ivernians, the ancient non-Celtic inhabitants of Ireland. Airem was the ancestor of the Celts who conquered the country."

The Hibbert Lecture, May 1886, is referred to in a footnote as the source from which this Irish version of the story of Romulus and Remus was taken. It is new to us, and will probably be so to most of our readers.

In the chapter on Number Three we are told the origin of the heraldic *fleur de lys*:—

"It was the device of *three* fishes tied together with a ribbon, which formed the *fleur de luce—luce* being the name of the fish; but which was afterwards transfigured into the more elegant emblem of the *fleur de lys*, the flower of the iris, taking the place of the fish, its *three* petals still presenting a trine."

It will be remembered that Justice Shallow, in the opening scene of the "Merry Wives of Windsor," speaking of "the dozen white luces" in his coat, remarks that "the luce is the fresh fish."

To the noble army of circle-squarers we leave the task of refuting the following argument; merely remarking that it may with equal facility be used to disprove the quadrature of the parabola, which has been believed in by all orthodox mathematicians since the time of Archimedes:—

"In a quadrangle, the space may be divided into the minutest squares, leaving no space undivided; but in a circle, every square applied to its periphery will always leave an angular space; and however far the process of smaller angles may be carried, an ultimate undivided space will remain."

Apparently our author is not quite satisfied with this; for in the next paragraph (on p. 47) he proves, in another manner, that the circle cannot be squared. In both proofs, for the words "a circle" we may substitute "any curve, including the parabola," without thereby affecting the argument.

We have never heard of Montrecla, to whom we are referred for an account of attempts to square the circle; but possibly Montucla is meant, who in 1754 published a "History of Researches relating to the Quadrature of the Circle," a second edition of which (by Lacroix) appeared in 1831. This conjecture is strengthened by the fact that our author's list of the principal calculators of π ends with Vega (born in 1754 and murdered in 1802), who obtained its value to 140 decimal places, making no mention of Rutherford and Shanks, who in more recent times pushed on the calculation to 500 and 707 places respectively.

From Chapter VII., which treats of a variety of subjects, including among them "the number of the beast" and the fine distinction between *six* and *half-a-dozen*, we select for comment the following sentence:—

"Six, also, is the least number of the *points of fixature*; so that a body cannot under all circumstances be immovable unless secured (or resisted) at six points."

Having only common-sense to guide us, and being unable to divine what train of reasoning could have led the author to the above conclusion, we should imagine that whenever any two points, A, B, of a body are fixed every other point in the straight line AB is also fixed, so that the body can only rotate round the line AB. Consequently if any third point (not in the straight line AB) is also fixed the body is immovable. Do the words "under all circumstances" imply that the body is immovable even when all *six* of our author's "points of fixature" are in the same straight line? If not, we are at a loss to know what they mean.

The appendix contains among other things a method of filling up magic squares which is said to have been communicated by a Russian mathematician to Prof. Sylvester and by him to a friend of the author. As some portions of the Russian Empire are not very far distant geographically from the land of the Chaldæans, this tradition may have had its origin among the magicians, astrologers, Chaldæans, and soothsayers of the Court of Nebuchadnezzar, to whom magic squares were doubtless well known. We hope Mr. Hopkins will be able to trace it to its source; even though it would take some time to do so, and the appearance of a second edition of "The Cardinal Numbers" might thereby be delayed. The public need not be impatient, for they can in the meantime allay their curiosity concerning the properties of magic squares by a perusal of the "Mathematical Recreations" of Ozanam and a host of more modern writers.

OUR BOOK SHELF.

The Romance of Mathematics. Being the Original Researches of a Lady Professor of Girtham College in Polemical Science, with some Account of the Social Properties of a Conic; Equations to Brain-Waves; Social Forces; and the Laws of Political Motion. By P. Hampson, M.A., Oriel College, Oxford. (London: Elliot Stock, 1886.)

OUR first acquaintance with the title, which we have copied in full, was limited to its four opening words. These suggested various ways in which the subject might be treated; we had no idea that the task before us was to examine and report upon a somewhat mild *jeu d'esprit*.

The editor, who poses as a Cambridge student and quondam pupil of the Girtham Professor, and subsequently as her husband, discovers, in a well-worn desk, certain lectures, essays, and other matter. In his introduction he says it is not his intention to disclose how he came into possession of the papers; in the closing pages he is caught in his work of reading and transcribing, and "at length we gained our point, and obtained the full sanction of the late Lady Professor of Girtham College to publish her papers." "Thus her obedient pupil is enabled to repay his late instructress for all her kindness to him," and also to remove from the mind of the reader the unpleasant feeling he has all along had whilst perusing the papers, that he was a party to a mean action in so doing.

The earliest essay, in an unfinished form, written whilst *in statu pupillari*, is entitled "Some Remarks of a Girtham girl on Female Education," and combats those "male sycophants" who "would prevent us from competing with you; you would separate yourselves on your island of knowledge, and sink the punt which would bear us over to your privileged shore. Of all the twaddle—forgive me, male sycophants!—that the world has ever heard, I think the greatest is that which you have talked about female education."

The second paper is a "Lecture on the Theory of Brain-Waves, and the Transmigration and Potentiality of Mental Forces." She takes the usual equation

$$y = \frac{a}{r} \sin \frac{2\pi}{\lambda} (vt - r),$$

and determines λ by the method of mesmerism. "We find the ratio of brain to brain—the relative strength which one bears to another; and then, by an application of our formula, we can actually determine the wave of thought, and read the minds of our fellow-creatures. An unbounded field for reflection and speculation is here suggested. Like all great discoveries, the elements of the problem have unconsciously been utilized by many who are unable to account for their method of procedure. . . . The development of this theory of brain-waves may be of great practical utility to the world. It shows that great care ought to be exercised in the domain of thought, as well as that of speech." Some verses follow, and then we have Papers iii. and iv., which are, in our opinion, the best part of the book, viz. a "Lecture on the Social Properties of a Conic Section," and the "Theory of Polemical Mathematics." Paper v. contains a "Lecture upon Social Forces, with some Account of Polemical Kinematics," and Paper vi. carries on the preceding into "Polemical Statics and Dynamics"; Paper vii. expounds the "Laws of Political Motion," and Paper viii. closes the book with a lecture "On the Principle of Polemical Cohesion." We ought to apologize for going into such detail, but our account will show our readers that the present work does not deal with mathematical discoveries. It is a "skit," with the perusal of which a reader acquainted with mathematics may while away, not unpleasantly, an odd half-hour or two.

Antipodean Notes. By "Wanderer." (London: Sampson Low, 1888.)

Lights and Shadows of Melbourne Life. By John Freeman. (Same publishers.)

THE "notes" in the first of these two books do not embody the results of a very wide experience. They simply record some observations made by the author in the course of a nine months' tour round the world. "Wanderer" does not, however, pretend to offer an exhaustive account of any of the subjects on which he touches. He has an easy, pleasant style, and gives with some vividness his first impressions of the scenes he describes. The greater part of the book relates to New Zealand, the practical, commercial, and social aspects of which he had, he thinks, more and better opportunities of studying than are obtainable by the majority of "globe-trotters." There is a short but interesting chapter on the Maoris, of whose qualities, as they have been affected by contact with civilization, "Wanderer" has no very exalted opinion. He admits, however, that there are exceptions to what he calls "the average of uselessness." One of the native members of the House of Representatives is, he says, "highly educated, intelligent, and even eloquent." The question whether women should be admitted to the House was lately discussed, and the speech of this deputy on the subject was "by far the most brilliant and entertaining of a debate in which many colonial legislators soared above the ordinary level of dull mediocrity."

The second book consists of a series of papers, some of which were originally contributed to Melbourne newspapers. They are written in rather too "smart" a style, but contain much information which it would be hard for Englishmen who may be interested in Melbourne to find elsewhere. The book will no doubt be welcomed by many visitors who will go this year to Melbourne to see the Centennial International Exhibition.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Salt Industry in the United States.

I CAN sympathize to a great extent with your correspondent George P. Merrill on the question of salt statistics. For a number of years I have been accumulating information on the whole subject of salt, and have found the greatest difficulty in obtaining much of a trustworthy character. The most extensive salt literature is in Germany: even there the statistical part of the subject is not dealt with so extensively as the geological, geographical, chemical, and manufacturing. Perhaps the most complete salt literature is that of India, which is issued yearly by the Government; but it deals almost exclusively with Indian salt.

I am not much surprised that the information in our Encyclopædias respecting the salt industry of the United States should be so scanty. Most of the information derivable respecting it has to be obtained from public newspapers, trade pamphlets, or papers in scientific journals. It is true that, so far as the deposit of Petit Anse, in Louisiana, is concerned, the United States Government published an account of it shortly after the termination of the great war. Dr. Sterry Hunt, whom I had the pleasure of meeting at Manchester at the British Association, has written probably more than anyone else on American salts; but it must be borne in mind that it is only within the last twenty years that the great salt discoveries in Western New York and Michigan have been made. I have a complete or nearly complete list of all the Michigan works, which was issued in the *Chicago Tribune* of January 24, 1888. I have also before me a copy of the *Saginaw Courier* of December 18, 1887. This gives some valuable tables respecting the Michigan salt. In Michigan, in 1887, 3,944,309 barrels of salt were inspected by the salt inspector. In 1869, only 561,288 barrels were made; and in 1880, 2,676,588. There was more salt made in Michigan in 1887 than had been made previously to 1869 in that State. The growth of the salt manufacture has been exceedingly rapid in the States; hence the reason why so little is known of it outside the persons interested in the trade.

Within the last five or six years there has grown up a most extensive salt manufacture in the Wyoming Valley in Western New York. Already this new district bids fair to cut out entirely the old Syracuse or Onondaga district. The make of American salt has much more than doubled itself in the last ten years. I am sure that personally I shall be much pleased if Mr. Merrill will, either through your columns or direct to me, give any information more accurate than is obtainable from our Encyclopædias. I am striving to establish at Northwich, the centre of the Cheshire salt trade, a Salt Museum, and although I have been for a long time accumulating specimens of salt from all parts, and have, thanks to the East Indian Government, and through the kind exertions of Mr. J. T. Brunner, M.P., our Parliamentary representative, who is most handsomely furnishing the Museum, a complete set of specimens of Indian salt, yet I find great difficulty in obtaining works treating on salt, also maps, plans, and diagrams. I trust, by degrees, to have a Museum perfectly unique, I believe.

When I say that until the last two or three years our English salt statistics have not been trustworthy, and that it is only by the indefatigable exertions of Mr. Joseph Dickinson, H.M. Inspector of Mines, assisted by myself and one or two other gentlemen connected with our salt trade, that they are now very nearly complete, Mr. Merrill must not be surprised at the difficulty of getting trustworthy information. For some seventeen years I have kept a complete list of all salt exports from the Mersey ports, and this list, I think, is the only complete one published, though the Salt Chamber of Commerce here professes to have a list, which it does not issue for public use. Indeed, I regret to say that it is almost impossible to get any assistance or information from this body.

The French Government issues at times a list of salt manufactured or raised from mines. The last I received, viz. 1879, gave, as the production for that year, 283,000 tons of sea salt and 293,000 tons of rock salt.

I shall be glad to give any information I possess to Mr. Merrill, and should be glad if any of your readers could give any information or assistance that would enable me to make as complete as possible the Salt Museum we are here forming.

Northwich, Cheshire. THOMAS WARD.

Prof. Rosenbusch's Work on Petrology.

PROF. BONNEY'S letter (NATURE, vol. xxxvii. p. 556) makes me venture once more to ask permission for space for a few remarks. One of the objects I had in view in writing to you at first is partly attained by the appearance of Prof. Bonney's "friendly protest"; and his remark that but for my letter he should have refrained "for a season" leads me to hope that in due course this object may be still further realized.

Prof. Bonney sees great objections to Rosenbusch's system of classification, and demurs to some of his groups altogether, both as to those admitted and those omitted. Naturally, then, he desires that this system shall not, by students of petrology, be too readily accepted nor too blindly followed. I do not think there is much danger of this, nor do I think that the "viaduct" was too much complimented either by Dr. Hatch or myself, the defective foundation of the piers in question being quite sufficiently alluded to for the time being.

The position, however, seems to be this. The number of earnest students of petrology is larger now than formerly, and is on the increase. They feel that no satisfactory system of classification had yet been offered to them, and indeed are rather bewildered by the fact that opinions as to what is the best system have been almost as many in number as the teachers who could by any means claim to be authorities entitled to instruct in this matter. Also, it is now a long time since any detailed system of classification, covering the whole ground, has been attempted.

Now we have such an attempt offered to us by Rosenbusch, and there is no doubt that to many it will be very welcome and will be largely used, in spite of the defects undoubtedly seen in it.

Prof. Bonney objects to the viaduct because of the weakness of some of its piers, and still more strongly objects to it, I think, because he considers that when a student has crossed it he will arrive at a point from which he will obtain a view of the surrounding country which will not be a good or correct view, and which will in some respects confuse the knowledge of that country already obtained and still to be sought for.

Would not this be just exactly the best time for some authority of great experience to come forward and point out to us younger workers wherein the viaduct is defective, and wherein we shall see wrongly from the ground on the further side of it; and to tell us his opinions as to a better viaduct, so placed as to lead us to a better point of outlook?

May we hope that Prof. Bonney will himself give us such a detailed criticism of the subject? It would be received with great attention and gratitude by many who, like myself, are looking for "light and leading" in this branch of study.

A. B.

History of the Contraction Theory of Mountain Formation.

IN his "Physical Geology," second edition, p. 674, Prof. Green says: "The notion that the earth's contraction has been the cause of the displacement of the rocks and the elevations of the surface seems to have occurred first to Descartes (éd. française, 1668, p. 322)."

It does not seem to be generally known that, a few years later, the same idea occurred to Newton. In a letter to Dr. Thomas Burnet he refers to that writer's "Sacred Theory of the Earth," the Latin edition of which was published in 1681, and considers the creation of the earth in connection with the Mosaic account. After suggesting illustrations of the "generation of hills," Newton concludes thus: "I forbear to describe other causes of mountains, as the breaking out of vapours from below before the earth was well hardened,—*the settling and shrinking of the whole globe after the upper regions or surface began to be hard;*" though he adds, "I have not set down anything I have well considered, or will undertake to defend."

The letter, which is written in reply to one of Burnet's, dated January 13, 1680-81, is given in full in Brewster's "Memoirs of Sir Isaac Newton," vol. ii. Appendix 4. The manuscript from which it is printed is a copy of the letter, without date or signature; but, according to Brewster, "the whole is distinctly written in Sir Isaac's hand." CHARLES DAVISON.

Birmingham, April 23.

Lightning and Milk.

EMIN PASHA (NATURE, vol. xxxvii. p. 583) mentions the African superstition "that fire kindled by a flash of lightning cannot be extinguished until a small quantity of milk has been poured over it." This idea is embodied in a Russian proverb, and has also existed in parts of Germany (Boyes, *Lacon*, p. 157). Emin Pasha adds that, in tempering swords made from meteoric iron (*vulgo*, thunderbolts), the blacksmith uses not water, but milk. Are other instances of this custom known? Has any explanation been offered? Indian folk-lore furnishes two ideas which may illustrate it: one, that the fall of a meteor is a bad omen (*Indian Notes and Queries*, July 1887, 674); the other, that evil spirits are very fond of fresh milk (*ib.*, December 1886, 198). Meteorites and lightning are connected in the minds of ignorant people, particularly, as Emin Pasha tells us, in the present instance. The milk, therefore, whether applied by smith or fire-man, may be rather intended as a propitiation than used for its intrinsic power of tempering steel or extinguishing flame. F. A. BATHER.

20 Campden Hill Road, Kensington, W., April 29.

The Duplex Pendulum Seismograph.

As the accuracy of the duplex pendulum seismograph has been impugned by a writer in NATURE, vol. xxxvii. p. 571, who at the same time adopts the instrument (with modifications which are, in my opinion, the reverse of improvements) I forward to you comparison diagrams. They show side by side the record given by the seismograph itself, and the real motion of the base of the instrument when that was artificially shaken in a manner that closely imitated an earthquake. The real motion was recorded by means of a multiplying lever hinged by a universal joint in a bracket fixed to a separate support. In both records the motion is magnified about six times. The agreement of the two demonstrates the accuracy of the instrument as an earthquake recorder, alike for large and for small motions. These are examples of tests which I have been in the habit of applying to seismographs since 1880 (see Proc. R. S., vol. xxxi. p. 440). In the present case the test was made with one of the duplex pendulum seismographs made and sold by the Cambridge Scientific Instrument Company, and described by me in NATURE, vol. xxxiv. p. 343. J. A. EWING.

University College, Dundee, April 20.

Self-Induction.

I HAVE to apologize for erroneously attributing to Dr. Lodge a suggestion with reference to the self-induction of wires for high-tension electric discharges. I do not, however, consider, as Prof. Lodge appears to do, that for such discharges it is "on the face of it absurd" to suppose that the self-induction of iron wires is less than that of copper wires of the same dimensions. Prof. Ewing has suggested that for very small values of the magnetizing force, H, iron may possibly behave as a diamagnetic body, and the corresponding values of the magnetic susceptibility, k, may be negative. The values of the magnetic induction, B, which are given by the equation—

$$B = (1 + 4\pi k)H,$$

will be less than H, because k is negative. The rate of increase of B with H will be less than unity for iron if this supposition is true, and will be equal to unity for copper, for which we may suppose that the value of k is negligible. The coefficient of self-induction, which will be proportional to the rate of increase of B with regard to H for wires of the same dimensions will accordingly be less for the iron than for the copper.

City and Guilds Institute, May 2. W. E. SUMPNER.

SUGGESTIONS ON THE CLASSIFICATION OF THE VARIOUS SPECIES OF HEAVENLY BODIES.[1]

IV.

IV.—ON THE SPECTRA OF STARS OF GROUP II.

IN the previous part of this memoir I have attempted to give a general idea of that grouping of celestial bodies which in my opinion best accords with our present knowledge, and which has been based upon the assumed meteoric origin of all of them.

I now proceed to test the hypothesis further by showing how it bears the strain put upon it when, in addition to general grouping, it is used to show us how specific differences are arrived at.

I. GENERAL DISCUSSION OF DUNÉR'S OBSERVATIONS.

In the paper communicated to the Royal Society on November 17 I pointed out that the so-called "stars"

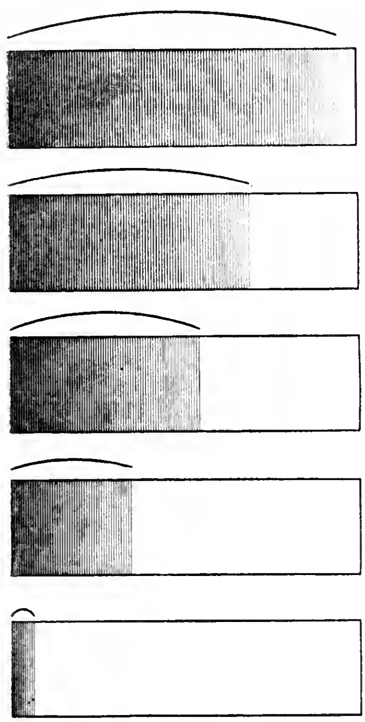


FIG. 6.—Diagram showing how an absorption fluting varies in width according to the quantity of absorbing substance present.

of Class III.*a* were not masses of vapour like our sun, but swarms of meteorites; the spectrum being a compound one, due to the radiation of vapour in the interspaces and to the absorption of the light of the red- or white-hot meteorites by vapours volatilized out of them by the heat produced by collisions.

I also showed that the radiation was that of carbon vapour, and that some of the absorption was produced by the chief flutings of Mn and Zn.

Dunér in his map gives eleven absorption bands, chiefly flutings, in Class III *a*, but in the case of the tenth and eleventh bands there is some discrepancy between his map and the text, to which reference will be made subsequently. His measurements are of the darker portions of the flutings, speaking generally.

[1] The Bakerian Lecture, delivered at the Royal Society on April 12, by J. Norman Lockyer, F.R.S. Continued from p. 11.

It will be clear at once that in the case of the *dark* flutings the dark bands should agree with the true *absorption* of the vapours, and that when the amount of absorption varies, only that wave-length away from the maximum of the flutings will vary. Thus, the same fluting may be represented as in Fig. 6, according to the quantity of the absorbing substance present.

In the case of the *bright* flutings, however, the dark bands on either side may *in some cases* be produced partly by contrast only, and the brighter and wider the bright flutings are the more they will appear to vary, and in two ways: first, they will dim by contrast when the bright fluting is dimmer than ordinary; and secondly, the one on the side towards which the bright fluting expands from its most decided edge will diminish as the bright fluting expands (see Fig. 7).

There is also another important matter to be borne in mind. As these spectra are in the main produced by the integration of the continuous spectra of the meteorites, the bright flutings of carbon, and the dark flutings produced by the absorption of the continuous spectra by the

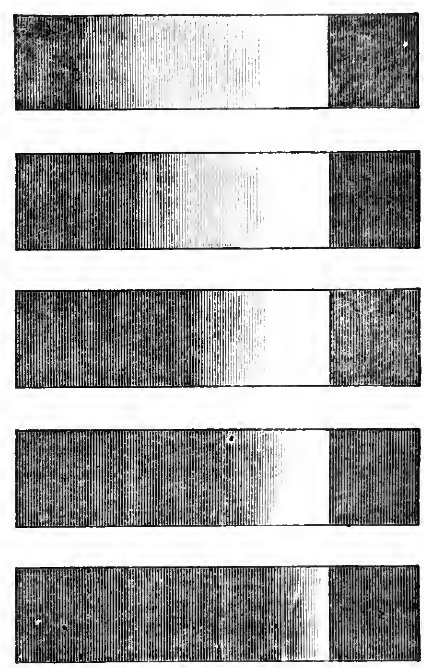


FIG. 7.—Diagram showing the variation in width of a bright fluting, and the consequent variation in width of the contrast band at the fainter edge.

vapour surrounding each meteorite; the proportion of bright fluting area to dark fluting area will vary with the reduction of the spacing between the meteorites.

If any bright or dark flutings occur in the same region of the spectra when the spaces are greatest, the radiation effect will be stronger, and the absorption fluting will be "masked;" where they are least the radiation itself will be masked. This reasoning not only applies to flutings but to lines also.

The Radiation Flutings.

We will first deal with the radiation flutings—those of carbon. The brightest less refrangible edge of the chief one is at wave-length 517, where it sharply cuts off the tail end of the absorption of the magnesium fluting the darkest edge of which begins at 520, as the carbon light from the interspace pales the absorption. The same thing happens at the more refrangible edge of the other absorption of Mg at 500, as Dunér's figures show.

	Less refrangible edge.	More refrangible sharp edge.
Band 8 (absorption of Mg)	502	496 in α Herculis.
	501	496 in ρ Persei.
	503	496 in R Leonis Min.
	505	496 in β Pegasi.

If this explanation of the rigidity of the less refrangible edge may be accepted, it is suggested that the rigidity of the end of band 8 at 496, near the nebula line 495, seems to indicate that we may have that line as the bright, less refrangible, boundary of another radiation fluting.

The fluting at 517 is the chief radiation fluting of carbon. The next more refrangible one, which would be most easily seen, as the continuous spectrum would be less bright in the blue, has its less refrangible and brightest edge at 474.

This in all probability has been seen by Dunér, though, as before stated, there is here a discrepancy between his maps and his text. It lies between his dark bands 9 and 10, the measurements of which are as follow:—

	Less refrangible edge.	More refrangible edge.
Band 9	482	476 in α Orionis.
	484	477 in β Pegasi.
Band 10	472	460 in α Orionis.
	474	462 in α Herculis.

It is not necessary for me to point out the extreme and special difficulty of observations and determinations of wave-lengths in this part of the spectrum. Taking this into consideration, and bearing in mind that my observations of the chemical elements have shown me no other bands or flutings in this region, I feel justified in looking upon the narrow bright space between bands 9 and 10 as an indication of another carbon fluting—the one we should expect to find associated with the one at 517, with its bright edge at 473 instead of 476, where Dunér's measurements place it. There is a bright fluting in this position in Nova Orionis.

I shall refer to both these points later on.

The third fluting, the carbon one with its brightest edge at 564, is certainly also present; though here the proof depends upon its masking effect, and upon the manner in which this effect ceases when the other flutings narrow and become faint.

In addition to these three flutings of carbon, which we shall distinguish in what follows as carbon A, there is sometimes a fourth more refrangible one beginning at wave-length 461, which is due to some other molecular form of carbon. It extends from wave-length 461 to 451, and, as we shall presently see, it is this which gives rise to the apparent absorption band No. 10 in the blue; this we shall distinguish as carbon B.

It is very probable also that in some cases there is, in addition to carbon A and carbon B, the hydrocarbon fluting which begins at wave-length 431, the evidence of this being Dunér's apparent absorption band 11. It may be remarked here, that although most of the luminosity of this fluting is on the more refrangible side of 431, there is also a considerable amount on the less refrangible side.

With regard to bands 9, 10, and 11, then, there is little doubt that they are merely dark spaces between the bright blue flutings of carbon, and that whether they are seen or not depends upon the relative brightnesses of the carbon flutings and the continuous spectrum from the incandescent meteorites. When the continuous spectrum is faint, it will not extend far into the blue, and the resulting dark space between the bright carbon A fluting at 474 and the end of the continuous spectrum is the origin of the apparent absorption band 9. When the continuous spectrum gets very bright, band 9 should, and does, disappear. On reference to the maps of the spectra of the "stars" with bright lines, it will be seen that the broad apparent absorption band in the blue agrees exactly in position with band 9, and it undoubtedly has the same origin in both cases. This band may therefore be regarded as the connecting link between the bodies belonging to Group I. and those belonging to the group under consideration.

Band 10 is the dark space between the bright carbon A fluting at 474 and the carbon B at 461, and can only exist as long as the carbon flutings are brighter than the continuous spectrum. Dunér's mean values for the band are 461–473, and on comparing these with the wave-lengths of the carbon flutings (see Fig. 10, which will be given in the next instalment) it will be seen that the coincidence is almost perfect.

There is a little uncertainty about band 11, which Dunér was only able to measure in one star, but it very probably has its origin in the dark space between the bright carbon B fluting and the hydrocarbon fluting at 431 (see Fig. 10). This would give a band somewhat broader and more refrangible than that shown in Dunér's map; but, as already pointed out, great accuracy in this part of the spectrum cannot be expected.

Chemical Substances indicated by the Absorption Flutings and Bands.

I may state that I have now obtained evidence to show that the origin of the following *absorption* flutings is probably as under:—

No. of Fluting.	Origin.	Wave-length of darkest most refrangible edge.	Wave-length of less refrangible end, given by Dunér as measured in α Orionis.
2	Fe	616	628
3	Mn (2) ...	585	595
4	Mn (1)[1] ...	558	564
5	Pb (1)[2] ...	544	550
6	Ba[3]	524	526
7	Mg	521	517
8	Mg	500	495

These flutings are characteristic of the whole class, and Dunér's catalogue consists chiefly of a statement of their presence or absence, or their varying intensities, in the different stars.

He gives other bands and wide lines which he has measured specially in α Orionis. I have also discovered the origin of the majority of these. They are as follows:—

			Wave-length.
	I.	Fluting of Cr (1)	581
	II.	?	570–577
	III.	Fluting of Pb (2)	567
	IV.	?	543
	V.	Line of Mn seen in bunsen ...	538–540
	VI.	Band of Ba	532–534
Lines	1.	Fluting of Cr (2)...	559[4]
	2.	,, ,, (3)...	536
	3.	Line of Cr seen in bunsen	520
	4.	Ba band	514[5]
	5.	1st, 2nd, and 3rd Ba flutings ...	601
	6.		634
	7.		649

Band 1, which extends from wave-length 649·5 to 663·8, has not yet been allocated.

Tests at our Disposal.

In order to prove that my explanation of the nature of these celestial bodies is sufficient, a discussion of the individual observations of them, seeing that differences in

[1] Means strongest fluting.

[2] The second Pb band has been seen in α Scorpii and α Orionis. Owing to an error in the map in the former paper, this fluting was ascribed to zinc.

[3] This is the second brightest band, wave-length 525. The first, at wave-length 515, is masked by the radiation fluting at 516.

[4] This is not given by Dunér. It would be masked by the Mn fluting in the star. I have inserted it to show that we could not be dealing with the 3rd fluting of Cr at 536 if we could not explain the apparent absence of the 2nd.

[5] In the early stages this band is masked by the vivid light coming from the carbon in the interspaces.

the spectra are known to exist, should show that all the differences can be accounted for in the main by differences in the amount of interspace; that is to say, by a difference between the relative areas of space and meteorite in a section of the swarm at right angles to the line of sight. I say in the main, because subsequent inquiry may indicate that we should expect to find minor differences brought about by the beginnings of condensation in large as opposed to small swarms, and also by the actual or apparent magnitudes of the swarms varying their brilliancy, thus enabling a more minute study to be made of the same stage of heat in one swarm than in another.

How minor differences may arise will be at once seen when we consider the conditions of observation.

The apparent point of light generally seen is on my view produced not by a mass of vapour of more or less regular outline and structure, but by a swarm of meteorites perhaps with more than one point of condensation.

An equal amount of light received from the body may be produced by any stage, or number of nuclei, of condensation; and with any differences of area between the more luminous centre and the outliers of the swarm.

All these conditions producing light of very different qualities are integrated in the image on the slit of the spectroscope.

I have said "generally seen," because it has been long known that many of the objects I am now discussing are variable, as well as red, and that at the minimum they are not always seen as sharp points of light[1] but have been described as hazy.

The severe nature of the tests at our disposal will be recognized when we inquire what must follow from the variation of the spacing. Thus, as the spacing is reduced—

I. The temperature must increase.

- *a.* Vapours produced at the lowest temperatures will be the first to appear.
- *β.* The spectrum of each substance must vary with the quantity of vapour produced as the temperature increases, and the new absorptions produced must be the same *and must follow in the same order* as those observed in laboratory experiments.

II. The carbon spectrum must first get more intense and then diminish afterwards as the spaces, now smaller, are occupied by vapours of other substances.

- *a.* The longest spectrum will be that produced by mean spacing.
- *β.* The masking of the dark bands by the bright ones must vary, and must be reduced as the mean spacing is reduced.

III. The continuous spectrum of the meteorites must increase.

- *a.* There will be a gradually-increasing dimming of the absorption-bands from this cause.
- *β.* This dimming will be entirely independent of the width of the band.

IV. The spectrum must gradually get richer in absorption-bands.

- *a.* Those produced at the lowest temperatures will be relatively widest first.
- *β.* Those produced at the highest temperatures will be relatively widest last.
- *γ.* They must all finally thin.

These necessary conditions, then, having to be fulfilled, I now proceed to discuss M. Dunér's individual observations. I shall show subsequently that there are, in all probability, other bodies besides those he has observed which really belong to this group.

[1] Hind first noticed this in 1851. Quoted by Arago, "Astronomie Populaire."

II. Discussion of Dunér's Individual Observations.

Consideration of the Extreme Conditions of Spacing.

Cæteris paribus, when the interspaces are largest we should have a *preponderance* of the radiation of carbon, so far as quantity goes. The bands will be wide and pale, the complete radiation will not yet be developed; a minimum of metallic absorption phenomena—that is, only the flutings of magnesium (8 and 7), the first fluting of manganese (3), and the first fluting of iron (2); but the great width of the bright band at 517 will mask band 8.

When the interspaces are least, the radiation of carbon should give place to the absorption phenomena due to the presence of those metallic vapours produced at the highest temperature at which a swarm can exist as such; the bright flutings of carbon should be diminished, and the true absorption flutings of Mg, Fe, Mn, Pb, and the band of Ba, should be enhanced in intensity.

There will be an *inversion* between the radiation and absorption.

The highest intensity of the absorption phenomena will be indicated by the strengthening of the bands 2, 3, 4, 5, and 6; and the appearance of the other flutings and bands specially recorded in *a* Orionis. The bands 7 and 8 will disappear as they are special to a low temperature, and will give way to the absorption of manganese, iron, *b*, &c.

This inversion, to deal with it in its broadest aspect should give us at the beginning 7 strong, and 2, 3 weak, and at the end 7 and 8 weak, and 2, 3 strong.

The first stage, representing almost a cometic condition of the swarm before condensation has begun, has been observed in Nos. 3,[1] 23, 24, 25, 36, 68, 72, 81, 118, 247, 249. There is a very large number of similar instances to be found in the observations. The above are only given as examples.

The *last* stage, before all the bands fade away entirely, has been observed in Nos. 1, 2, 26, 32, 33, 38, 40, 61, 64, 69, 71, 75, 77, 82, 96, 101, 116. As before, these are only given as instances.

It is natural that these extreme points along the line of evolution represented in the bodies under consideration should form, as I think they do, the two most contrasted distinctions recorded by Dunér—that is, recorded in the greatest number of cases.

Origin of the Discontinuous Spectrum.

I have already shown that when the meteorites are wide apart, though not at their widest, and there is no very marked condensation, the spectrum will extend farther into the blue, and therefore the flutings in the blue will be quite bright; in fact, under this condition the chief light in this part of the spectrum, almost indeed the only light, will come from the bright carbon. Under this same condition the temperature of the meteorites will not be very high, there will therefore be little continuous spectrum to be absorbed in the red and yellow. Hence we shall have discontinuity from one end of the spectrum to the other. This has also been recorded, and in fact it is the condition which gives us almost the most beautiful examples of the class (196, *a* Herculis, 141, 172, 229).

The *defect* of continuous light *in the blue* in this class, after condensation has commenced and the carbon flutings are beginning to disappear, arises from defect of radiation of the meteorites, and hence in all fully-developed swarms the spectrum is not seen far into the blue for the reason that the vapours round each meteorite are at a tempera-

[1] The references are to the numbers of the stars in Dunér's catalogue.

ture such that fluting absorption mainly takes place, although of course there must be some continuous absorption in the blue. This is perhaps the most highly-developed normal spectrum-giving condition; 44, 45, 55, 60, 65, 86, 92, 278 are examples.

The Paling of the Flutings.

Subsequently, the spectra are in all cases far from being discontinuous, and the flutings, instead of being black, are pale. Thus, while the bands are dark in the stars we have named, they are not so dark in α Orionis. Here, in short, we have a great distinction between this star and α Herculis, *o* Ceti, R Lyræ, and ρ Persei.

Obviously this arises from the fact that the average distances between the meteorites have been reduced; their temperature being thereby increased as more collisions are possible, the vapours are nearly as brilliant as the meteorites, and radiation from the interspaces cloaks the evidences of absorption. Nor is this all: as the meteorites are nearer together, the area producing the bright flutings of the carbon is relatively reduced, and the bands 10 and 9 will fade for lack of contrast, while 8 and 7 will fade owing to the increased temperature of the system generally carrying the magnesium absorption into the line stage; *b* is now predominant (see 102, 157, 163, 114, 125, 135).

Under these conditions the *outer* absorbing metallic atmosphere round each meteorite will in all probability consist of Mn and Fe vapours, and in this position the masking effect will least apply to them. This is so (114, 116); they remain dark, while the others are pale.

Here we have the indication of one of the penultimate stages already referred to.

Phenomena of Condensation.

Dealing specially with the question of condensation,—I have already referred to possibly the first condition of all, recorded by Dunér in the observations now discussed—I may say that the first real and obvious approach to it perhaps is observed when all, or nearly all, except 9 and 10 of the flutings are *wide* and *dark*. The reasons will be obvious from what has been previously stated. Still more condensation will give all, or nearly all, the bands wide and pale, while the final stage of condensation of the swarm will be reached when all the bands fade and give place to lines. We have then reached Class II. (107, 139, 168, 264); 2 and 3 should be and are perhaps the last to go (203).

The Bands 9 and 10.

With regard specially to the bands 9 and 10, which include between them a bright space which I contend is the second fluting of carbon, I may add that if this view is sound, the absence of 10 should mean a broad carbon band, and this is the condition of non-condensation, though not the initial condition. The red flutings should therefore be well marked—whether broad or not does not matter; but they should be dark and not *pale*. Similarly the absence of band 9 means non-condensation.

Therefore 9 and 10 should vary together, and as a matter of fact we find that their complete absence from the spectrum, while the metallic absorption is strong, is a very common condition (1, 2, 6, 16, 26, 32, 39, 40, 46, 54, 60).

That this explanation is probably the true one is shown by further consideration of what should happen to the red flutings when 9 and 10 are present. As the strong red flutings indicate condensation, according to my view this condensation (see *ante*) should pale the other flutings. This happens (3, 8, 13, 28, 35, 45, 30; and last, not least, among the examples, I give 50, α Orionis).

III. Results of the Discussion.

The Line of Evolution.

I have gone over all the individual observations recorded by Dunér, and, dealing with them all to the best of my ability in the light afforded by the allocation of the bands to the various chemical substances, the history of the swarms he has observed seems to be as follows:—

(1) The swarm has arrived at the stage at which, owing to the gradual nearing of the meteorites, the hydrogen lines, which appeared at first in consequence of the great tenuity of the gases in the interspaces, give way to carbon. At first the fluting at 473 appears (as in many bright-line stars), and afterwards the one at 517. This is very nearly, but, as I shall show subsequently, not quite, the real beginning of Class III.*a*, and the radiation is now accompanied by the fluting absorption of Mg, Fe, and Mn—bands 7, 2, 3. This is the absorption produced at the temperature of the oxy-coal gas flame, while the stars above referred to give us the bright line of Mn seen at the temperature of the bunsen.

(2) The bright band of carbon at 517 narrows and unveils the Mg absorption at band 8. We have 8 now as well as 7 (both representing Mg), added to the bands 2 and 3, representing Fe and Mn, and these latter now intensify.

(3) The spacing gets smaller; the carbon, though reduced in relative quantity, gets more intense. The second band at 473 in the blue gets brighter as well as the one at 517. We have now bands 9 and 10 added. This reduced spacing increases the number of collisions, so that Pb and Ba are added to Mg, Fe, and Mn. We have the bands 2, 3, 4, 5, 6, 7, 8, 9, and 10. This is the condition which gives, so to speak, the normal spectrum

(4) This increased action will give us a bright atmosphere round each meteorite, only the light of the meteorite in the line of sight will be absorbed: we shall now have much continuous spectrum from the interspaces as well as the vapour of carbon. *The absorption flutings will pale*, and the Mg flutings will disappear on account of the higher temperature, while new ones will make their appearance.

(5) Greater nearness still will be followed by the further dimming of the bright carbon flutings including the one at 517. The blue end of the spectrum will shorten as the bands fade, narrow, and increase in number. If the star be bright, it will now put on the appearance of α Orionis; if dim, only the flutings of Fe and Mn(1), bands 2 and 3, will remain prominent.

(6) All the flutings and bands gradually thin, fade, and disappear. A star of the third group is the result.

In the latter higher-temperature stages we must expect hydrogen to be present, but it need not necessarily be visible, as the bright lines from the interspaces may cancel or mask the absorption in the line of sight of the light of the meteorites; but in case of any violent action, such as that produced by another swarm moving with great velocity, we must expect to see them bright, and they are shown bright in a magnificent photograph of *o* Ceti, taken for the Draper Memorial, which I owe to the kindness of Prof. Pickering. I shall return to this question.

Stages antecedent to those recorded by Dunér.

So far I have referred to the swarms observed by Dunér. The result of the discussion has been to show that all the phenomena are included in the hypothesis that the final stages we have considered are antecedent to the formation of stars of Group III., bodies which give an almost exclusively line absorption, though these bodies are probably not yet stars, if we use the term star to express complete volatilization, similar to that observed in the case of our sun.

The question then arises, Are all the mixed fluting stages really included among the objects already considered?

It will be remembered that in my former communication I adduced evidence to the effect that the mixed fluting stage was preceded by others in which the swarms were still more dispersed, and at a lower temperature. The first condition gives us bright hydrogen; the last little continuous spectrum to be absorbed, so that the spectrum is one with more bright lines than indications of absorption; and, in fact, the chief difference between the spectra of these swarms and of those still sparser ones which we call nebulæ lies in the fact that there are a few more bright metallic lines or remnants of flutings; those of magnesium, in the one case, being replaced by others of manganese and iron.

If my view be correct—if there are stages preceding those recorded by Dunér in which we get both dark and bright flutings—it is among bodies with spectra very similar to these that they should be found.

The first stage exhibited in the objects observed by Dunér is marked by flutings 7, 3, and 2 (omitting the less refrangible one not yet allocated), representing the flutings Mg, Mn, and Fe visible at the lowest temperatures.

The stars which I look upon as representing a prior stage should have recorded in their spectra the flutings 7 and 3 (without 2), representing Mg and Mn.

(*To be continued*)

THREE DAYS ON THE SUMMIT OF MONT BLANC.

ALPINE men are already beginning to think of the work of the coming season. We commend to their attention the following notes relating to the experiences of M. Richard, who spent three days during the past summer on the summit of Mont Blanc, with a view to making a series of continuous meteorological and other observations. There are many Alpine men who might, if they pleased, follow his example without much inconvenience to themselves and with considerable advantage to science. The following is a summary of the record which M. Richard has contributed to *La Nature*:—

The summit of Mont Blanc is a station of the utmost importance to meteorology, since it rises to a great height (4810 metres), and overtops the whole Alpine group. But it had not hitherto been considered possible to remain there for any length of time. De Saussure, whose statue is erected at Chamounix, passed some days in 1788, on the Géant hill, at the height of 3510 metres. In 1844 Martins, Bravais, and Le Pileur, pitched their tent at the Grand-Plateau, 4000 metres high, and here they passed several days, and made numerous and important observations. Hitherto no explorer had remained on the summit of the mountain itself for any length of time; tourists making but a very short stay—usually only a few minutes From these facts we can see the importance of the scientific expedition carried out in the summer of 1887, with great success, by M. Joseph Vallot, one of the most daring and able members of the Alpine Club. Having made, in 1886, a series of physiological observations, during the ascent of some of the highest peaks of the Alps, he determined to establish on Mont Blanc three temporary meteorological observatories, the first at Chamounix, 1050 metres high, the second on the rocks of the Grands-Mulets, 3059 metres high, and the third on the summit of Mont Blanc. He constructed meteorological sheds, and furnished each of them with registering instruments constructed by MM. Richard Brothers—a barometer, a thermometer, and a hygrometer. The instruments placed at Chamounix and the Grands-Mulets were inspected every week, but those at the summit could not be reached for fifteen days, on account of bad weather. To superintend the lower stations he procured the assistance of M. Henri Vallot, a distinguished engineer, on whose competence and carefulness he could rely. At Chamounix, M. Joseph Vallot's plan was considered impracticable. He executed it, however, in company with M. F. M. Richard, one of the makers of the registers. No less than twenty-four guides were necessary, on account of

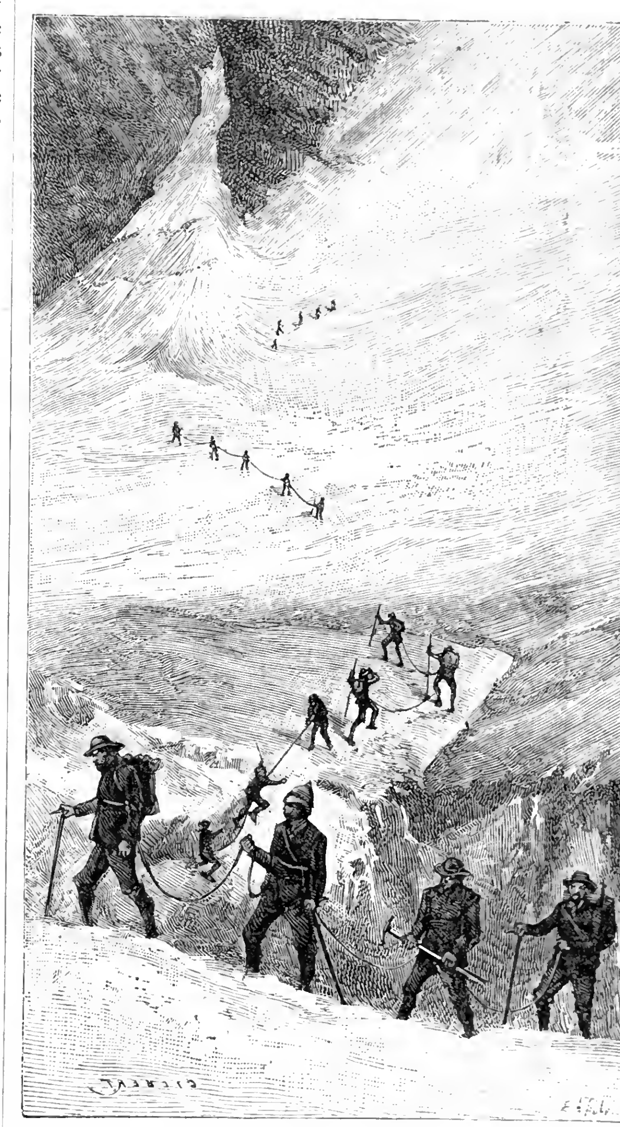


Fig. 1.

the great weight of the baggage (250 kilogrammes). At midday, July 27, 1887, they began the ascent to the Grands-Mulets. On account of the late start, the party, overtaken by night, arrived at the Grands-Mulets at 10 o'clock. Getting to bed at 11 o'clock, the travellers set out again the next morning at 3, after a light meal.

M. Richard then proceeds to tell the story of the journey and of the time spent on the top of Mont Blanc. The ascent from the Grands-Mulets is difficult, but not very dangerous when the snow is good. Crevasses have to be crossed by ladders, and very steep banks of snow must be struggled through. They arrived at the Grand-Plateau at 7 o'clock, and stopped there for refreshment and repose. At the Tournette rock, one of the bearers was forced to stop from fatigue, and to give his load to one of the more robust, and about 3 o'clock in the afternoon they arrived at the summit. All the guides but two deposited their burdens on the snow, and immediately took their departure. When ascending the last hill, MM. Vallot and Richard were attacked by mountain-sickness, and for some hours did not recover. M. Richard compares the shape of the mountain-top to a pear cut in two and resting on a plate, the stalk of the fruit well representing the narrow ridge by which one ascends. Between this ridge and the dome, which measures scarcely more than 20 metres in diameter, is a small indentation, in which they fixed their tent. Having driven the stakes into the snow, they secured the tent by a long rope. None of them had at that time the strength or courage to arrange the baggage. They were compelled to take shelter from the wind, and having refreshed themselves with a little soup, made with melted snow and preserved bouillon, they stretched themselves on the ground, with their heads on the boxes of instruments and the cooking-utensils.

Overcome by his efforts in erecting the tent, M. Richard fell asleep; but during part of the night M. Vallot made gallant efforts to fix his instruments, but he was at length compelled by the snow to return. After some hours of sleep, the cold woke M. Richard, and, fearing the effects of the carbonic acid gas engendered by the breathing of four persons, with the consent of the others he allowed some air to enter, and, lighting a lantern, placed it on the ground, believing it would be extinguished before there would be any danger of suffocation. However, the wind which raged outside kept the tent well ventilated, and froze them to the marrow. About 4 o'clock they all went out of the tent and watched the sun rise—a sight which, M. Richard says, was worth all the pains and fatigues they had endured. The thermometer, when placed on the snow, stood at 19° C. below zero. The sun rose, and it was a most marvellous sight. As the day-star shone out, rosy clouds enveloped the snow-clad tops of the surrounding mountains; little by little, the shadows in which the rocky peaks emerging from the snow were clothed disappeared, leaving the peaks covered with the richest tints. The clouds below sometimes appeared like a rough sea, with its waves dashing against a rocky shore, and sometimes like a thick veil thrown over valleys by the night. Then these clouds dissolved into air under the influence of the sun's rays, seeming to disappear as if by magic, leaving no other trace of their existence than a light mist clinging to the sides of the mountains.

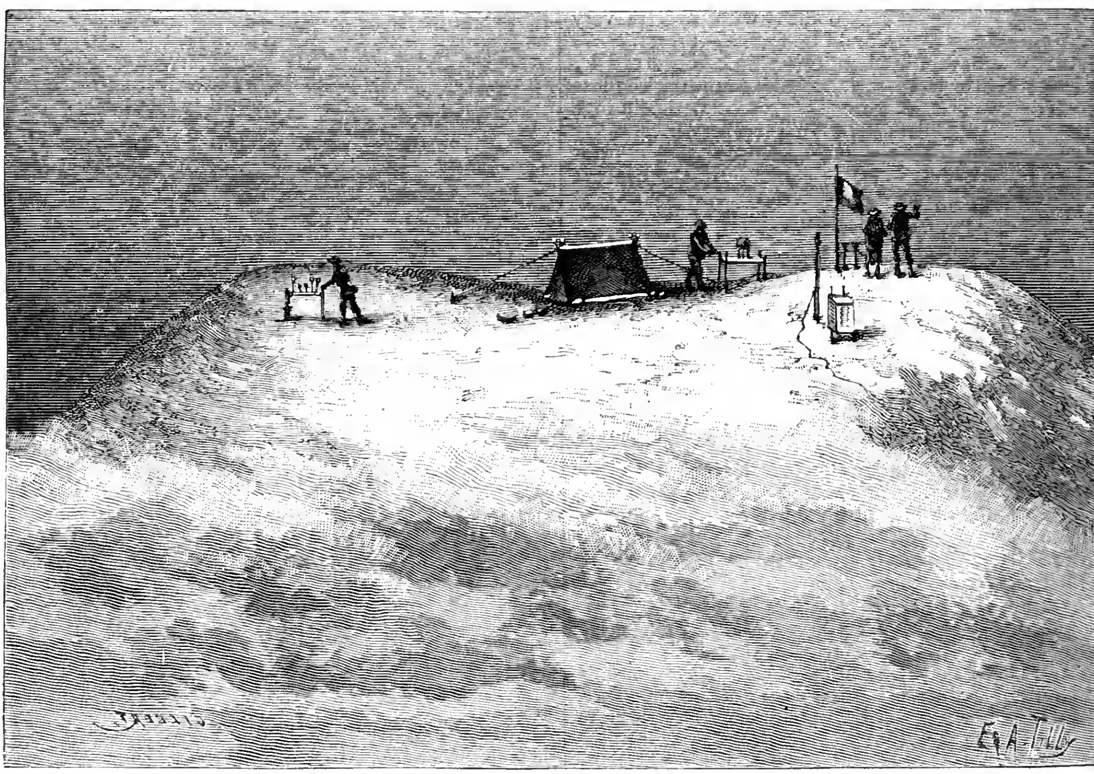


Fig. 2.

They now began to put their instruments into position. The large actinometer, made by M. Violle, was placed on a small table; and the others—the actinometers of Arago and M. Violle, the thermometers, and the Fontin barometer—being fixed (Fig. 2), M. Vallot at once commenced his observations. Then they made their tent more comfortable with a floor of double-tarred cloth, and, above this, a mattress, hard, no doubt, but to them a very welcome addition. The tent was 4 metres square, and 1·50 metre high. The health of the party was not very good: M. Richard and one of the guides suffered from severe headaches, with feverish symptoms. The least effort, even ordinary movement, caused such fatigue that they were compelled to lie down during a great part of the day. They had a visitor the first day, in the person of Baron Munch, coming from Courmayeur, in Italy, into Chamounix, who was amazed to find sojourners on the top of Mont Blanc. The second night was not so trying as the first: they had pillows, which were softer than the pots and pans, and they thus had a most refreshing sleep. The tent was very picturesque. M. Vallot had brought for the party gutta-percha snow-boots, which they put on over fur-lined boots. Thanks to this precaution their feet were kept free from frost-bite. Their leather shoes were of no use; they had been dried in the sun and hung on a string stretched aloft across the tent. On this string at night were also hung the glasses which are always necessary to protect the eyes from ophthalmia in those regions. M. Vallot had also brought coverings for the ears and neck, and linen masks to preserve the skin of the face. Equipped in this manner the aspect of the travellers was curious and even terrifying (Fig. 4). The tent with the various articles hung up, with the boxes of provisions, the blazing stove, and the boiling soup, had a most picturesque appearance (Fig. 3).

The second day was spent in making observations. The provisions were almost neglected; they never had an appetite during their stay. The different preserved meats, though very tempting, did not entice their benumbed stomachs, and twice each day they took nothing but a little preserved bouillon, in which a small piece of cheese had been broken. Their drink was warm coffee; on the first day tea had made them ill, and they never could take it again during their three days' sojourn: the guides, however, drank a little of it.

On July 30, the observations began at sunrise. Towards 10 o'clock the little colony received a second visitor, an Englishman, who, on his departure, wished to take away with him some letters dated from the top of Mont Blanc. A yellow-beaked crow settled herself time after time near the observers. The guides declared that her presence was a sign of good weather; but it did not prove so. Towards 2 o'clock enormous clouds began to form on the side of Mont Pelvoux; then their colour changed; the gloom turned to darkness; and while the weather remained fine over Chamounix, the valley of Aosta and the Savoy Alps were soon hidden by a terrible thunderstorm. A furious wind drove the observers into the tent. It was 4 o'clock, and they had almost made up their

FIG. 4. FIG. 3.

minds to descend, but as there was not time to put all their instruments in safety, they decided to remain and weather the storm. They held the ropes of the tent, and piled snow all around it to keep it steady. Towards 9 o'clock, M. Vallot having gone out, found himself surrounded by electrical clouds, which played around his clothes and his head, but he escaped any actual shock. During the hours that they thus anxiously passed in the tent they were compelled to close the last opening to prevent the snow from getting in. But the time was not spent without profit. M. Vallot made some physiological diagrams. The beatings of the pulse, of the carotid, &c., were to have so much the more interest because they would differ from those which would be obtained when but a short stay is made, the travellers now having been two days at the summit. These observations made them forget their troubles. At last, about 2 o'clock in the morning, the tempest passed away, and, although the wind continued to blow violently, they got a refreshing sleep.

They decided on the following day, July 31, to continue their observations till 9 o'clock, then to bring everything into the tent, and to redescend to Chamounix. The guide Payot was suffering from a violent head-ache, with a high fever, and was compelled to keep his bed, but about 11 o'clock he bravely offered to descend at once, and even desired to carry his knapsack. M. Vallot had not given orders for help to be sent to take their baggage away; they therefore left the greater portion behind them in the tent; still there were many things that could not be left. These were divided into bundles, and, with a last glance at the magnificent view, they began the descent. The guide Michel had warned them that this would be very difficult, since last night's storm would have obliterated all traces of the usual paths. And so it was found to be. After the Grand-Plateau, the journey was most dangerous. At this height it had rained, and the snow had become so soft that they often sank to the waist in it. In the rapid slopes, where they were forced to descend zigzag, the snow slipped from under their feet, but, after much care and fatigue, they arrived at the Grands-Mulets. A good meal, a denser air, and a milder temperature, soon restored them to their usual health. Towards 7 o'clock they came to Chamounix, where they received an enthusiastic welcome.

It had thus been proved that it was quite possible to live and make observations at those high altitudes. The greatest danger is in the violent storms that burst almost without notice, and which may become terrible tempests against which any temporary observatory would not stand. M. Richard says that the results of the observations will be published when the papers have been inspected and classified.

THE PHOTOGRAPHIC CHART OF THE HEAVENS.

WE reprint from the *Observatory* for May the following article by the editors:—

The "Bureau du Comité international permanent pour l'exécution photographique de la Carte du Ciel" has published, amongst other more technical papers relating to this subject, one by Dr. Gill, of a very remarkable character, to which we wish to draw attention. Most of those who attended the Conference understood that the work in contemplation was to make a photographic chart of the heavens, to take pictures of the stars by photography, showing, with the greatest care, the appearance of the heavens as they are at the present time, in order that at a future time these pictures might be used, by comparison with other pictures taken under similar conditions or directly with the sky, to determine the many questions that could be dealt with in this way—to enable, in fact, the astronomer of the future to have the sky of his past and his present to deal with. That this was so will be seen from a consideration of the three following resolutions which were agreed to unanimously by the Conference:—

"1. The progress made in astronomical photography demands that the astronomers of the present day should unite in undertaking a description of the heavens by photographic means.

"2. This work should be carried out at selected stations, and with instruments which should be identical in their essential parts.

"3. The principal objects are (α) to prepare a general photographic chart of the heavens for the present epoch, and to obtain data which will enable us to determine with the greatest possible accuracy the positions and the brightness of all the stars down to a given magnitude (the magnitude being understood in a photographic sense to be defined); (β) to provide for the best means of utilizing both at the present day and in the future the results of the data obtained by photographic means."

These were the fundamental resolutions; others, recommended by the two sections into which the Conference divided, were adopted as explanatory of the first. Amongst these was one in which it was decided to take "a second series of plates down to the 11th magnitude, in order to insure greater precision in the micrometric measurement of the reference-stars, and render possible the construction of a catalogue." We have stated these fundamental resolutions at length as bearing on the question of a catalogue of stars, for the paper by Dr. Gill contains the astounding proposition of cataloguing no less than 2,000,000 stars; that is to say, Dr. Gill gravely and seriously proposes the establishment of a Central Bureau, consisting of chief, assistants, secretaries, and a staff of measurers and computers, to take the photographs and *measure* them, and make a catalogue, the work to go on for twenty-five years at a cost of 250,000 francs, or £10,000, per annnm, or for fifty years at 150,000 francs.

It is quite true that this is only a proposition that Dr. Gill makes; but if such a proposition is possible in face of these direct resolutions of the Conference, it is quite time that everyone interested in the success of the work the Conference met to consider (that is, the photographic chart of the heavens) should bestir himself and see that the proposed work is not endangered by such astounding proposals.

To tack on to a work such as that sanctioned by the Conference—a work eminently practical, that has the support of all astronomers, and that has already been taken up by many of the Governments who were expected to join—a gigantic work such as Dr. Gill proposes, a work beside which that proposed by the Conference sinks into insignificance, would neither be fair to the Conference nor just to those Governments who have joined in the undertaking. The feature of the international scheme that makes it possible to obtain the assent of Government is that the work is proved to be practicable by experiment, and that it can be done at a moderate cost in something like five years, while the results are good for as long as the plates will last. To increase this work by extending it to, at the lowest computation of time, twenty-five and possibly fifty years, and to add enormously to the cost, would be to jeopardize the whole scheme.

Dr. Gill states that the actual state of astronomical science demands a catalogue of stars to the 11th magnitude. He thus raises the question on its merits; and we would here state that it is more than possible that not only is there no need of such a catalogue, but that the use of such catalogues as he proposes has for ever ceased. The minds of some astronomers move in grooves, and it will, no doubt, never be conceded by them that catalogues can be superseded; they will die as they have lived, in the strong belief that the only way to use the stars is to catalogue them.

Till recently the knowledge we had of the stars was only to be gained from a written description of their brightness and position with regard to each other; hence the catalogue was an absolute necessity if we needed to know the number or brightness of certain stars in any part of the sky at any previous time; and we could only find this out if we had a catalogue of that time. Our catalogues of stars are all we have to show what has been observed up to the present time; but when we have a photographic chart of the heavens, we have for our record not a catalogue, but a representation. That catalogues of stars such as are used for fundamental places will be always used goes without saying; the photographic plates themselves, and the four or five stars on each required as the fiducial points and for identification, will of course be catalogued; but, beyond this, to catalogue the stars on each plate, to measure them for the purpose only of getting their places written down, would be the most utter waste of time, labour, and money that it could enter the mind of man to conceive.

The proposition brought forward by Dr. Gill should be settled decisively so far as the proposition concerns the work proposed by the Conference. There can be no question that such a thing was never intended; had such a thing been thought of, we should have had a "Conference for discussing the best way of making a Catalogue of Stars by photography."

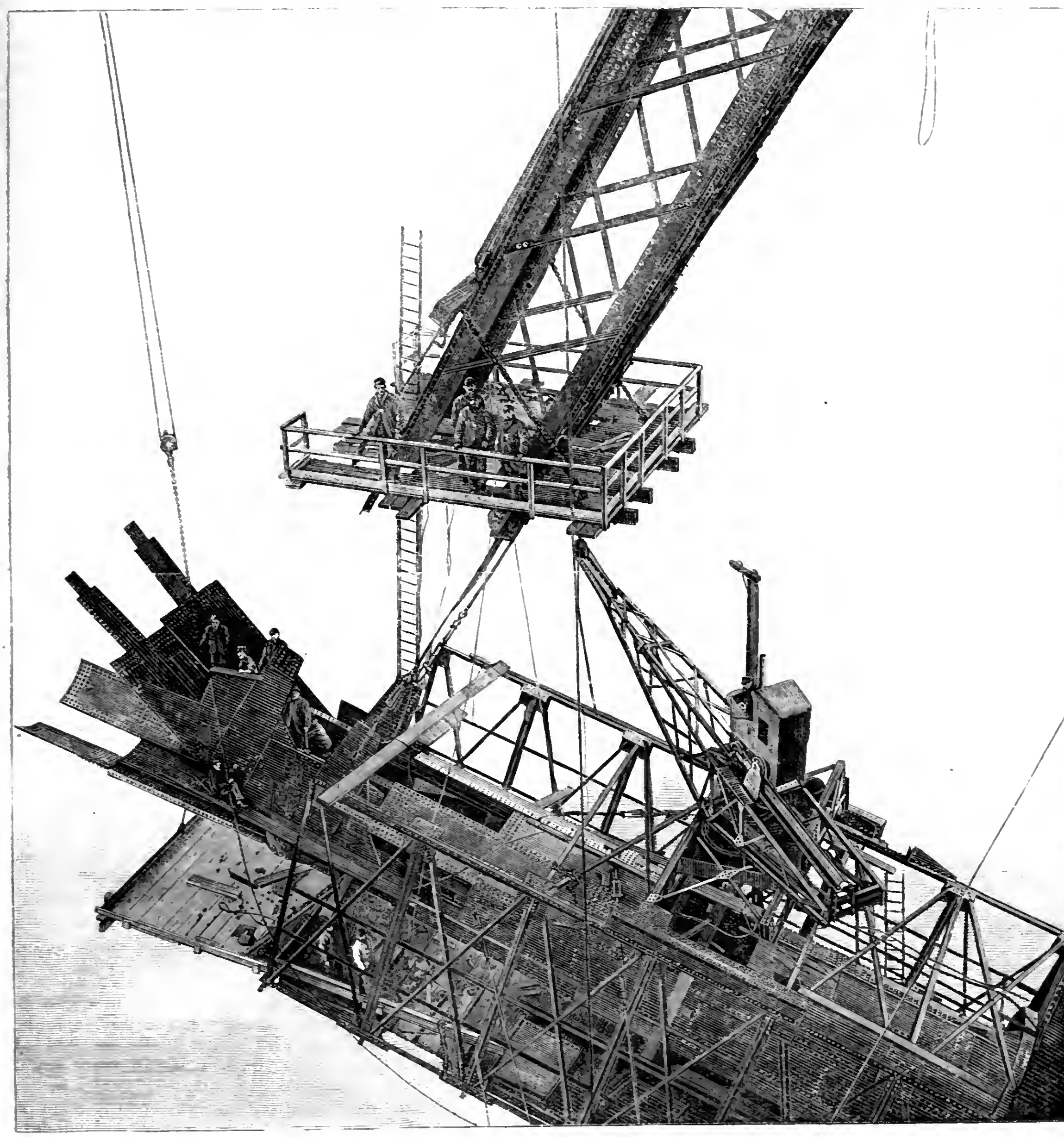
As this was not done, it can be done now; and if there is the great need of a catalogue of stars to the 11th magnitude felt by so many astronomers, as stated by Dr. Gill, it is a thing of so much greater importance as far as cost and time are concerned, that it should be considered and dealt with entirely apart from the other work. A new Congress might discuss it; the one which met in 1887 is not in any way committed to such a scheme.

THE FORTH BRIDGE.

WE have been enabled, through the kindness of Mr. Baker, to reproduce one of the photographs of the Forth Bridge, showing what is known as the "junction" at the end of bay 1, between tie 1, strut 2, and the bottom member.

A general account of the Forth Bridge has been so recently placed before the readers of NATURE (vol. xxxvi. p. 79), in the lecture by Mr. Baker, on May 20, 1887, before the Royal Institution, that it is unnecessary to cover the same ground again. The progress made in erection since that date is indicated by our engraving, showing the successful completion of the lower portion of the first bay.

The junction we have illustrated is nothing more nor



less than the connection of the web of a lattice girder with one of its booms, but here the junction alone weighs as much as an ordinary iron railway bridge of 100 feet span. This mass of steel work is suspended 80 feet above high water, and projects 180 feet beyond the masonry piers. Considerable forces are sometimes needed to bring the tubes into their correct position; and as in the case of the Britannia Bridge, which on a hot day moves 3 inches horizontally and $2\frac{1}{4}$ inches vertically between sunrise and sunset, so here considerable movement takes place during the day, and by careful watching the great tubes can sometimes be caught and retained in proper position, without the intervention of hydraulic or other power.

The weight of steel employed in the junction now under consideration is about 90 tons. The attachments to the strut and tie are made by means of strong gusset plates, the bottom member itself being strengthened internally at the junction by suitable diaphragms.

The importance of this junction will be readily understood, when it is stated that a load of some 6000 tons—the weight of an American liner—will be transmitted through it, in the finished structure, on its way to the masonry pier. Some 16,000 rivets are required for the junction; and large as this number may appear, it bears but a small ratio to the eight million rivets used in the whole structure. The method of construction of the junction was that uniformly adopted in dealing with these members. The junction was erected on the drill roads attached to the workshops at South Queensferry, all holes drilled by specially designed plant; and, having been marked for re-erection, it was taken down and transported plate by plate, and finally hoisted into position in the finished structure from a steam barge, by a crane working from the internal viaduct.

The tie was built downwards from the top of the vertical column; the timber cage—shown in our illustration—in which the men worked being attached to and following it as length by length was added. To design and build a structure of steel to bear a load of some 6000 tons is no mean task in itself, but what shall we say of the whole undertaking, when this junction alone contains but one five-hundredth of the material required for the completed Forth Bridge?

FLORA OF THE ANTARCTIC ISLANDS.

MR. W. B. HEMSLEY, who elaborated at Kew the collections made during the *Challenger* expedition illustrative of the floras of oceanic islands, has handed to me the following interesting letter from Dr. Guppy. The materials and notes accumulated by this skilful observer during his travels in the Western Pacific threw a good deal of light on the mode in which oceanic islands were stocked with plants, and Mr. Hemsley was able to make an advantageous use of them in discussing the collections made in the same region by Prof. Moseley.

I myself am very much impressed with the probable truth of the views expressed by Dr. Guppy. It would be very desirable to obtain additional observations which would serve to test their accuracy. It is with this object that I have obtained Dr. Guppy's permission to communicate his letter to NATURE.

W. T. THISELTON DYER.

Royal Gardens, Kew, April 28.

17 *Woodlane, Falmouth, April* 8, 1888.

As I am likely to be proceeding soon to the South Seas, I have been re-perusing your volume of the "Botany of the *Challenger*," more especially the remarks concerning the dispersal of plants, which I hope to take the opportunity of following up in a more systematic way than before.

I was thinking that if you thought it worth while you might direct the attention of masters of ships going round the Horn and the Cape of Good Hope to the chance of finding seeds in the crops of the oceanic birds that follow the ships in the regions of the westerly winds. I am inclined to believe that important results would be obtained. Judging from my experience, about one bird in twenty-five would contain a seed in its crop.

I am still inclined, if you will pardon my saying so, to the belief that the agency of birds like the Cape pigeons may explain some of the difficulties in the floras of the islands in the Southern Ocean. To return to the instance of my seed, I have since found an account where a Cape pigeon, around the neck of which a ribbon had been tied, followed a ship on its way home from Australia for no less than 5000 miles (Coppinger's "Cruise of the *Alert*," 1885, p. 18); and on consulting other voyages I find that the Cape pigeon appears to perform the circuit of the globe in the region of the Westerlies, so that my seed might readily have been transferred from Tristan d'Acunha to Amsterdam.

A remarkable point has occurred to me whilst reading your remarks (doubtless you have already thought of it). In a botanical sense, and also in a geographical sense, the Antarctic Islands seem to be arranged in two parallel zones. Tristan d'Acunha, Amsterdam, and St. Paul's, lying between the parallels of 37° to 40° S. lat., have similar floras. Further south is the second zone, between 47° and 55° (*circa*), in which the land and islands (Fuegia, Crozets, Kerguelen, Macquarie, &c.) are characterized by their common floras. Now, how are these two parallel botanical zones to be explained? It seems to me that if you grant that the northern zone may largely derive its common characters by the agency of birds following the westerly winds, such as I believe to have been the case, you are almost forced to the conclusion that the floras of Fuegia, Kerguelen, Macquarie Island, &c., in the southern zone have obtained their common characters in the same way. Of course the distinctiveness between the floras of the two parallel zones would then be explained by the difference in the climatic conditions arising from difference in latitude. For my own part I do not think the hypothesis of a sunken southern tract (or tracts) of land to be supported by geological evidence. Is not the geological character of the remote oceanic islands strongly negative of the idea that they are portions of ancient submerged tracts? Can Kerguelen, Amsterdam, &c., be in any sense *continental* islands as regards their rocks? With reference to New Zealand, if geologists are right in regarding it as lying along the same volcanic line that extends southward through the Western Pacific from New Guinea, then it is probable that the vast post-Tertiary upheaval of the island groups (Solomon Islands, New Hebrides, &c.) which I have shown to have taken place along this line of volcanic activity in the Western Pacific, has been represented in New Zealand by elevation rather than depression. I believe that subsequent investigation will confirm my belief that the great island groups of the Western Pacific, with New Caledonia and New Zealand, have been always insular. This is, I think, the great lesson I learned in the Solomon Islands.

H. B. GUPPY.

LORD HARTINGTON ON TECHNICAL EDUCATION.

THE Marquis of Hartington was the chief guest at the anniversary banquet of the Institution of Mechanical Engineers held on Friday, May 4, at the Criterion Restaurant. Mr. Edward H. Carbutt, President of the Institution, occupied the chair. In responding to the toast of "Our Guest," proposed by the Chairman, Lord Hartington, after speaking of the part which the mechanical engineering profession of this country takes in the maintenance and the extension of our material and industrial supremacy in the world, referred to the vast importance of technical education. He had never professed to be an authority on the subject of technical education—he was no authority on that subject; all he could do in the position he held was to endeavour to arouse such interest as he could in that subject, to enlist in the minds of the ordinary public—the unscientific public of whom he formed a part—an interest in this question, and to listen to the advice and attend to the counsel which were given to the public by those who were authorities on the subject, and to whose advice he held it was most important that attention should be paid. He had been greatly struck by the fact that in every country in Europe which competed with us in industrial or commercial pursuits greater attention had recently been paid to giving a practical direction to the national education than had hitherto been considered necessary in England. We had, like other countries—perhaps somewhat in arrear of them—established a national and tolerably complete instruction; but they, earlier than we, had embraced the idea of making that national instruction not only a literary instruction, but a technical and commercial education. But he could not help thinking that in that respect they had gained some considerable advantages over ourselves. He did not think there was any occasion for us to take a desponding or a pessimistic view of the situation. He had great confidence in the energy, the skill, and the intelligence of our people. But he believed there were facts which it would be madness on our part to ignore. If a new process, a new invention, were discovered in any other country—if a new process of manufacture were discovered greatly superior to that which was in existence among ourselves—we should at once admit that it was necessary for us either to improve that invention or else to resign ourselves to being defeated in the competition for the production of that article. But if it were true, as he believed it was, that the system of national education in other countries was being devoted to purposes which made the manual labour of the working population more intelligent, more skilled, and therefore more valuable, that was a fact which was just as important and which had consequences of exactly the same character as if foreign nations were to discover an invention which was not available for our own use. These facts had been investigated by a Royal Commission, and by a great number of private individuals for their own purposes; and there was no sort of doubt that foreign countries had not only attempted to give, but had to a very considerable extent succeeded in giving, a more practical turn to the education of their people in all branches of industry and commerce where science and art could be usefully and successfully applied. If it were the fact that we had fallen behind in this branch of the instruction of our people, it appeared to him that it would be worse than idle, it would be criminal, on our part if we were for a moment to ignore the consequences of those facts, and the consequences which might result not only to our temporary commercial and manufacturing position in the world, but to the future industrial position of England. He was sure there were none to whose advice great employers and leaders of industry in this country would more cheerfully and more willing listen, none who exercised a greater influence over the public mind of this country, than those whom he had the honour of addressing; and it was a great satisfaction to him to be assured by the words that had fallen from their Chairman that they were giving their earnest and anxious attention to the subject of technical education.

NOTES.

A Royal Commission has been appointed to inquire "whether any and what kind of new University or powers is or are required for the advancement of higher education in London." The Commissioners are Lord Selborne, Chairman; Sir James Hannen, Sir William Thomson, Dr. J. T. Ball, Mr. G. C. Brodrick, the Rev. J. E. C. Welldon, and Prof. Stokes, P.R.S. Mr. J. L. Goddard is appointed Secretary to the Commission.

Much trouble was taken to secure the success of the annual *conversazione* of the Royal Society held last night. We shall give some account of it next week.

The Emperor Frederick has marked the opening of his reign by conferring personal honours on some eminent Germans. Dr. Werner Siemens, the electrician, is one of those who have been ennobled or dignified with the prefix "Von."

The Donders Memorial Fund, to which we called attention some time ago, now amounts to about £2000, of which £250 has been subscribed in England. Prof. Donders' seventieth birthday falls on Sunday, the 27th inst.; but it has been decided that the celebration in his honour shall take place on the following day. The subscription list, so far as this country is concerned, will be closed on the 14th inst.

At the general monthly meeting of the Royal Institution, on Monday last, Dr. Tyndall was elected Honorary Professor, and Lord Rayleigh Professor, of Natural Philosophy.

A preliminary meeting, called by invitation of the Council of the Yorkshire Philosophical Society, to consider the desirability of forming a Museum Association, was held in York on May 3. Among the Museums represented at the meeting were those of Liverpool, Manchester, York, Sheffield, Nottingham, Bolton, Bradford, Sunderland, and Warrington. It was unanimously decided that a Museum Association should be formed, and that it should consist of curators or those engaged in the active work of Museums, and also of representatives of the Committees or Councils of Management of Museums. The Association will consider (1) whether it may not be possible to secure a compendious index of the contents of all provincial museums and collections; (2) the most effectual methods of facilitating the interchange of specimens and books between various museums; (3) the best plans for arranging museums and classifying their contents; (4) the organization of some concerted action for the obtaining of such Government publications as are interesting or important from a scientific point of view.

Prof. Arthur Schuster, F.R.S., has been appointed to the Langworthy Professorship of Physics and Directorship of the Physical Laboratory at the Owens College, in succession to the late Prof. Balfour Stewart.

The Gaekwar of Baroda is reported to have decided to send a number of young men, carefully selected for the purpose, to study scientific and technical subjects in England, under the supervision of Mr. Gajjar, Professor of Biology in the Baroda College.

The Government of Ceylon have sanctioned the opening of a Forest School at Kandy.

We regret to have to record the death of Sir Charles Bright, the eminent electrician. He died last Thursday, at the age of fifty-six.

Dr. Sigismond Wróblewski, Professor of Experimental Physics at the Polish University of Cracow, died on April 16 last, in consequence of injuries received through the explosion of some petroleum lamps. Prof. Wróblewski lived for some time in London, and was afterwards a Professor at the University of Strasburg. He also worked in the laboratory of Prof. Debray in the École Normale, Paris. He accepted the appointment at Cracow in 1882. His researches on the liquefaction of gases are well known.

The sodium salt of a new sulphur acid, of the composition $H_2S_4O_8$, has been prepared by M. Villiers (*Bull. de Soc. Chim.*, 1888, 671). It was obtained by the action of sulphur dioxide upon a strong solution of sodium thiosulphate, and is tolerably stable, crystallizing in well-developed prisms. A quantity of crystalline sodium thiosulphate contained in a flask was treated with an amount of water insufficient for complete solution; the flask was immersed in iced water, and a current of sulphur dioxide passed, with constant agitation, until the solution was saturated and all or nearly all the thiosulphate had dissolved. If any of the latter crystals remained undissolved, a little more water was added, and the solution again saturated with the gas, repeating this treatment until all had passed into solution. After leaving the liquid thus obtained at the ordinary temperature for two or three days, it was found to be capable of taking up a further considerable volume of sulphur dioxide, the former quantity having evidently entered into chemical combination in some way or other. It was therefore again saturated, and left for another day or two, after which the solution was evaporated *in vacuo* over sulphuric acid. It was then found that a precipitate of sulphur was gradually deposited upon the base of the containing dish, while fine white prisms of brilliant lustre were formed at the surface. On analysis they were found to be anhydrous, and yielded numbers corresponding to the formula $Na_2S_4O_8$ or NaS_2O_4. They dissolved in water with formation of a neutral solution. On again evaporating this solution under the receiver of the air-pump, crystals of a hydrate, $Na_2S_4O_8 + 2H_2O$, separated out. From the remarkable similarity in properties between oxygen and sulphur, it is probable that this new acid by no means exhausts all the possible combinations, for it appears as if one is capable of replacing the other to any extent, forming compounds which may perhaps be considered as oxygen substitution derivatives of polysulphides. M. Villiers has not yet completed his investigation of the properties and constitution of the new acid, further details of which will be awaited with considerable interest.

ON April 2 a severe shock of earthquake was felt at Kalleli, in the Lysefjord. It occurred simultaneously with one at Gjæsdal, also on the west coast of Norway. In the former place three distinct shocks were felt, causing the windows to rattle, clocks to stop, &c. A loud subterranean rumbling was heard. On the other side of the narrow fjord no shock was felt, but a deep rumbling detonation was heard.

ON the morning of April 18 a severe shock of earthquake was felt at Vexiö, in the south of Sweden. It lasted fully two minutes, and was followed by subterranean detonations. This is the third earthquake observed in this district during the last six months.

THE Calcutta Correspondent of the *Times* telegraphs that India has been visited by a series of what he calls "phenomenal" storms, partaking very much of the character of the Dacca tornado. At Moradabad, 150 deaths are reported, caused chiefly by hailstones. Many of the houses were unroofed, trees were uprooted, and masses of frozen hail remained lying about long after the cessation of the storm. At Delhi there was an extraordinary hailstorm lasting about two minutes, which was virtually a shower of lumps of ice. One of the hailstones picked up in the hospital garden weighed 1½ lb.; another, secured near the Telegraph Office, was of the size of a melon, and turned the scale at 2 lbs. At another place the Government House suffered severely, 200 panes of glass being broken by hail. In Lower Bengal, at Rayebati, 2000 huts were destroyed, while twenty persons are reported to have been killed and 200 severely injured. Chudressur, close to Serampore, was almost completely wrecked. The storm lasted only three minutes, its course extending for a mile and a half, and its path being 300 yards wide. Its advent was preceded by a loud booming noise. Large boats were lifted out of the river, and in one case a small boat was blown up into a tree.

ACCORDING to an official report, the substance of which has been given by the Calcutta Correspondent of the *Times*, an immense amount of injury was done by the Dacca tornado. No fewer than 118 persons were killed, excluding those drowned, and 1200 wounded had to be treated. The amount of the damage to property is estimated at Rs.6,78,428. Three hundred and fifty-eight houses were completely destroyed, 121 boats were wrecked, and 148 brick-built houses were partially, and 9 were completely, destroyed. Shortly after the Dacca tornado, another visited part of the Murchagunje subdivision, and 66 deaths and 128 cases of injury are reported. All the houses struck were completely destroyed. The Dacca tornado travelled altogether 3¼ miles. Its rate of speed varied from 12 to 20 miles, and its greatest width was 20 yards. It was accompanied by a rumbling hissing sound, the clouds over it were illuminated, and liquid mud was deposited along its track, and was ingrained in the wounds of the injured.

WE are glad to be able to report, on the authority of Captain de Brito Capello, Director of the Lisbon Observatory, that the Government of Brazil has established a Meteorological Service there, by decree dated April 4 last. The Director is Senhor A. Pinheiro, who has visited this country on several occasions.

AT the meeting of the French Meteorological Society, on the 3rd of April, M. Vaussenat presented and analyzed a long series of photographs of clouds taken at the Observatory of the Pic-du-Midi, from 1880 to 1887, under all conditions of the atmosphere. He drew special attention to the importance of the systematic observation of clouds, at that mountain observatory, and stated that by the aid of such observations he had been able to issue local predictions of weather which had acquired great accuracy. M. Grad gave particulars respecting the present meteorological organization in Alsace and Lorraine. In 1870, the Meteorological Commission presided over by M. Hirn established a complete network of stations, but this service was interrupted by the war which broke out soon after. At present there are twenty stations in the two provinces. One of these, viz. Strasburg, possesses an unbroken series of observations since 1801. It has been decided to establish a service there for the issue of weather forecasts for the benefit of agriculture.

MR. T. WILSON, of the Smithsonian Institution, gives in the *American Naturalist* an interesting account of some recent discoveries made by Mr. Frank Cushing, who has not only been adopted by the tribe of Zuñis, but initiated into the order of their priesthood. While at Tempe, in Arizona, in the spring of 1887, Mr. Cushing heard of a large truncated mound in the desert 6 or 7 miles to the south-east. He visited it, and declared it to be of artificial formation. Workmen were brought from Tempe, and in a short time they came upon the ruins of an immense building. Mr. Cushing at once arrived at the conclusion that this building had been used as an Indian temple. He observed many things which corresponded in a remarkable degree with the Zuñi religion, and which he was able to recognize in consequence of the experience he had gained as a priest. Continuing his explorations, he found the remains of a city 3 miles long and at some places 1 mile wide. This city was somewhat irregularly laid out, consisting principally of large squares or blocks of houses surrounded by a high wall, apparently for protection. The state of the buildings clearly indicated that the city had been ruined by an earthquake. Many bodies crushed by fallen roofs and walls were found. Mr. Cushing also discovered a number of graves, believed to be the graves of priests. The symbols and decorations on the pottery found in these graves resemble the symbols and decorations on modern Zuñi pottery. About 10 or 15 miles from this ruined city, which Mr. Cushing calls Los Muertos, the City of the Dead, he has lately found the remains of another prehistoric town, in connection with which there are many traces of extensive works for irrigation.

THE Boston Society of Natural History proposes to establish a Zoological Garden in that city. The enterprise will be thoroughly educational. The chief object will be to show specimens of American animals, especially those of New England.

ACCORDING to a telegram from Sydney, the Conference upon the means of dealing with the rabbit-pest in Australia has resulted in the selection of an island where M. Pasteur's and other methods of extirpation will be thoroughly tried. The liability of other animals and birds to infection by the same means will also be tested.

DURING the month of July the following courses, for technical teachers and others, will be given in the new buildings of the City and Guilds of London Institute:—Elementary Principles of Machine-Designing, by Prof. W. C. Unwin, F.R.S.; Practical Lessons in Organic Chemistry, intended mainly for teachers of technological subjects, by Prof. Armstrong, F.R.S.; the Construction and Use of Electrical Measuring Instruments, by Prof.

Ayrton, F.R.S.; Experimental Mechanics, by Prof. Henrici F.R.S.; the Principles of Bread-making, by William Iago; Photography, by Capt. Abney, F.R.S.; Mathematical and Surveying Instruments, by Arthur Thomas Walmisley; Gas Manufacture, by Lewis T. Wright; the Application of Modern Geometry to the Cutting of Solids for Masonry and other Technical Arts, by Lawrence Harvey; and the Craft of the Carpenter, by John Slater.

THE additions to the Zoological Society's Gardens during the past week include two Long-eared Bats (*Plecotus auritus*), from Cornwall, presented by Mr. F. A. Allchin; a —— Roe (*Capreolus* —— ♀), from Corea, presented by Mr. F. Harston Eagles; two Burrowing Owls (*Speotyto cunicularia*), from Buenos Ayres, presented by Mr. J. Clark Hawkshaw; a Blue and Yellow Macaw (*Ara ararauna*), from Para, presented by Mrs. Yarrow; two Crested Ducks (*Anas cristatus*), from the Falkland Islands, presented by Mr. F. E. Cobb, C.M.Z.S.; an Asp Viper (*Vipera aspis*), from Italy, presented by Messrs. Paul and Co.; a Common Viper (*Vipera berus*), from Burnham Beeches, presented by Mr. F. M. Oldham; two Japanese Deer (*Cervus sika* ♂ ♂), from Japan; a Macaque Monkey (*Macacus cynomolgus* ♂), from India, a Vulpine Phalanger (*Phalangista vulpina* ♂), from Australia, two Burrowing Owls (*Speotyto cunicularia*), from Buenos Ayres, deposited; a Spotted Cavy (*Cœlogenys paca*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

NEW MINOR PLANETS.—Herr Palisa, at Vienna, discovered a new minor planet, No. 276, on April 17, and M. Charlois, at Nice, discovered a second, No. 277, on May 3, the sixty-fourth and third discoveries respectively of the two astronomers. No. 273 has been named Atropos.

COMET 1888 *a* (SAWERTHAL).—The following ephemeris (*Dun Echt Circular*, No. 155) is in continuation of that given in NATURE, vol. xxxvii. p. 520:—

For Greenwich Midnight.

1888.	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log *r*.	Brightness.
May 10	23 45 45	31 39·8 N.	0·2242	0·1003	0·14
12	23 50 0	32 33·7			
14	23 54 9	33 25·8	0·2360	0·1198	0·12
16	23 58 12	34 16·1			
18	0 2 8	35 4·7	0·2470	0·1386	0·11
20	0 5 58	35 51·7			
22	0 9 42	36 37·2	0·2572	0·1566	0·09
24	0 13 20	37 21·2			
26	0 16 51	38 3·9	0·2666	0·1738	0·08
28	0 20 16	38 45·3			
30	0 23 35	39 25·6	0·2752	0·1904	0·07

The brightness at discovery is taken as unity.

CINCINNATI ZONE CATALOGUE.—No. 9 of the Publications of the Cincinnati Observatory contains a zone catalogue of 4050 stars observed during 1885, 1886, and the early part of 1887 with the 3-inch transit instrument of the Observatory, made by Buff and Berger. The region covered by the zones is from S. Decl. 18° 50′ to S. Decl. 22° 20′, most of the stars down to mag. 8·5 having been observed, besides a considerable number of fainter ones. A low power was employed, so as to give a field of 50′ in breadth, and as the zones were taken 15′ apart, each star was thus usually observed in three zones. The R.A.'s were deduced from transits, recorded on a chronograph, over a system of five vertical wires; the declinations, from bisections by a micrometer wire, two readings being taken for each star whenever practicable. The probable error of a single observation was found to be R.A. ± 0·123s., Decl. ± 1″·84, the observations being a little rougher than could have been desired, in consequence of the low magnifying power used. An important portion of the work has been the comparison of the resulting places with those for the same stars in earlier catalogues, and a considerable number of errata in Lalande's, Lamont's, and other catalogues have been detected. A list of seventy-five proper motions, nearly all of them new, is likewise added.

PUBLICATIONS OF LICK OBSERVATORY.—The first volume of the Publications of the Lick Observatory has been received. It is chiefly occupied with the details of the progress of the institution from the date of Mr. Lick's first deed of trust, 1874, and with the description of the smaller instruments, the great refractor being reserved for a future volume. Meteorological observations taken on Mount Hamilton from 1880 to 1885, and reduction tables for the Observatory occupy a large part of the volume. Amongst the most interesting reports are those of Prof. Newcomb, on the glass for the great objective; of Mr. Burnham, on Mount Hamilton as an observing station; and of Prof. Todd, on the transit of Venus, 1882. A report on the structure of the mountain is also given by Profs. Irving and Jackson.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 MAY 13–19.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on May 13

Sun rises, 4h. 12m.; souths, 11h. 56m. 9·7s.; sets, 19h. 40m.: right asc. on meridian, 3h. 22·8m.; decl. 18° 34′ N. Sidereal Time at Sunset, 11h. 8m.

Moon (at First Quarter May 18, 23h.) rises, 5h. 58m.; souths, 13h. 54m.; sets, 21h. 57m.: right asc. on meridian, 5h. 21·2m.; decl. 19° 46′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	4 16	12 8	20 0	3 34·8	19 50 N.
Venus.....	3 45	10 54	18 3	2 20·6	12 38 N.
Mars......	15 36	21 19	3 2*	12 46·9	4 6 S.
Jupiter....	20 21*	0 38	4 55	16 3·2	19 43 S.
Saturn....	8 51	16 47	0 43*	8 14·5	20 29 N.
Uranus...	15 44	21 23	3 2*	12 51·5	4 47 S.
Neptune..	4 41	12 24	20 7	3 50·7	18 28 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

May.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
15	61 Geminorum	6	21 48	near approach	212 —
16	*d*′ Cancri	6	23 5	23 45	74 337

May.	h.	
15	0	Mercury at least distance from the Sun.
16	22	Saturn in conjunction with and 0° 42′ north of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m
U Cephei	0 52·4	81 16 N.	May 17,	1 38 *m*
ζ Geminorum	6 57·5	20 44 N.	,, 15,	0 0 *M*
U Hydræ	10 32·0	12 48 S.	,, 15,	*m*
W Virginis	13 20·3	2 48 S.	,, 17,	1 0 *M*
R Draconis	16 32·4	67 0 N.	,, 14,	*m*
U Ophiuchi	17 10·9	1 20 N.	,, 18,	0 36 *m*
W Sagittarii	17 57·9	29 35 S.	,, 17,	3 0 *m*
β Lyræ	18 46·0	33 14 N.	,, 17,	23 0 *M*
R Lyræ	18 51·9	43 48 N.	,, 18,	*m*
η Aquilæ	19 46·8	0 43 N.	,, 19,	23 0 *m*
W Cygni	21 31·8	44 53 N.	,, 18,	*M*
δ Cephei	22 25·0	57 51 N.	,, 15,	23 0 *M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near η Aquilæ	295	0	May 15. Very swift.
From Delphinus	314	15 N.	May 13–18. Very swift. Streaks.

THE PYGMY RACES OF MEN.[1]

I.

IT is well known that there existed among the nations of antiquity a wide-spread belief in the existence of a race or races of human beings of exceedingly diminutive stature, who dwelt in some of the remote and unexplored regions of the earth. These were called *Pygmies*, a word said to be derived from πυγμή, which means a fist, and also a measure of length, the distance from the elbow to the knuckles of an ordinary-sized man, or rather more than 13 inches.

In the opening of the third book of the *Iliad*, the Trojan hosts are described as coming on with noise and shouting, "like the cranes which flee from the coming of winter and sudden rain, and fly with clamour towards the streams of ocean, bearing slaughter and fate to the Pygmy men, and in early morn offer cruel battle," or, as Pope has it—

"So when inclement winters vex the plain,
With piercing frosts, or thick descending rain,
To warmer seas the cranes embodied fly,
With noise and order through the midway sky,
To Pygmy nations wounds and death they bring,
And all the war descends upon the wing."

The combats between the pygmies and the cranes are often alluded to by late classical writers, and are not unfrequently depicted upon Greek vases. In one of these in the Hope collection at Deepdene, in which the figures are represented with great spirit, the pygmies are dwarfish-looking men with large heads, negro features, and close woolly or frizzly hair. They are armed with lances. Notices of a less poetical and apparently more scientific character of the occurrence of very small races of human beings are met with in Aristotle, Herodotus, Ctesias, Pliny, Pomponius Melo, and others. Aristotle places his pygmies in Africa, near the sources of the Nile, while Ctesias describes a race of dwarfs in the interior of India. The account in Herodotus is so circumstantial, and has such an air of truthfulness about it, especially in connection with recent discoveries, that it is worth quoting in full.[2]

"I did hear, indeed, what I will now relate, from certain natives of Cyrênê. Once upon a time, they said, they were on a visit to the oracular shrine of Ammon, when it chanced that, in the course of conversation with Etearchus, the Ammonian king, the talk fell upon the Nile, how that its sources were unknown to all men. Etearchus upon this mentioned that some Nasamonians had once come to his Court, and when asked if they could give any information concerning the uninhabited parts of Libya, had told the following tale. The Nasamonians are a Libyan race who occupy the Syrtes, and a tract of no great size towards the east. They said there had grown up among them some wild young men, the sons of certain chiefs, who, when they came to man's estate, indulged in all manner of extravagancies, and among other things drew lots for five of their number to go and explore the desert parts of Libya, and try if they could not penetrate further than any had done previously. The young men therefore dispatched on this errand by their comrades with a plentiful supply of water and provisions, travelled at first through the inhabited region, passing which they came to the wild beast tract, whence they finally entered upon the desert, which they proceeded to cross in a direction from east to west. After journeying for many days over a wide extent of sand, they came at last to a plain where they observed trees growing: approaching them, and seeing fruit on them, they proceeded to gather it. While they were thus engaged, there came upon them some dwarfish men, under the middle height, who seized them and carried them off. The Nasamonians could not understand a word of their language, nor had they any acquaintance with the language of the Nasamonians. They were led across extensive marshes, and finally came to a town, where all the men were of the height of their conductors, and black-complexioned. A great river flowed by the town, running from west to east, and containing crocodiles."

It is satisfactory to know that the narrative concludes by saying that these pioneers of African exploration, forerunners of Bruce and Park, of Barth, Livingstone, Speke, Grant, Schweinfurth, Stanley, and the rest, "got safe back to their country."

Extension of knowledge of the natural products of the earth, and a more critical spirit on the part of authors, led to attempts of explanation of this belief, and the discovery of races of monkeys—of the doings of which, it must be said, more or less fabulous stories were often reported by travellers—generally sufficed the commentators and naturalists of the last century to explain the origin of the stories of the pygmies. To this view the great authority of Buffon was extended.

Still more recently-acquired information as to the actual condition of the human population of the globe has, however, led to a revision of the ideas upon the subject, and to more careful and critical researches into the ancient documents. M. de Quatrefages, the eminent and veteran Professor of Anthropology at the Muséum d'Histoire Naturelle of Paris, has especially carefully examined and collated all the evidence bearing upon the question, and devoted much ingenuity of argument to prove that the two localities in which the ancient authors appear to place their pygmies, the interior of Africa near the sources of the Nile, and the southernmost parts of Asia, and the characters they assign to them, indicate an actual knowledge of the existence of the two groups of small people which still inhabit these regions, the history of which will form the subject of this lecture. The evidence which has convinced M. de Quatrefages, and which, I have no doubt, will suffice for those who take pleasure in discovering an underlying truth in all such legends and myths, or in the more grateful task of rehabilitating the veracity of the fathers of literature and history, will be found collected in a very readable form in a little book published last year in the "Bibliothèque scientifique contemporaine," called "Les Pygmées," to which I refer my readers for fuller information upon the subject of this discourse, and especially for numerous references to the literature of the subject, which, as the book is accessible to all who wish to pursue it further, I need not give here.

It is still, however, to my mind, an open question whether these old stories may not be classed with innumerable others, the offspring of the fertile invention of the human brain, the potency of which as an origin of myths has, I think, sometimes been too much underrated. I shall therefore now take leave of them, and confine myself to giving you, as far as the brief space of time at my disposal admits, an account of our actual knowledge of the smallest races of men either existing or, as far as we know, ever having existed on earth, and which may therefore, taking the word in its current though not literal sense, be called the "pygmies" of the species.

Among the various characters by which the different races of men are distinguished from one another, *size* is undoubtedly one of considerable importance. Not but what in each race there is much individual variation, some persons being taller, and some shorter; yet these variations are, especially in the purer or less mixed races, restricted within certain limits, and there is a general average, both for men and women, which can be ascertained when a sufficient number of accurate measurements have been recorded. That the prevailing size of a race is a really deeply-seated, inherited characteristic, and depends but little on outward conditions, as abundance of food, climate, &c., is proved by well-known facts. The tallest and the shortest races in Europe are respectively the Norwegians and the Lapps, living in almost the same region. In Africa, also, the diminutive Bushmen and the tallest race of the country, the Kaffirs, are close neighbours. The natives of the Andaman Islands and those of many islands of the equatorial region of the Pacific, in which the conditions are similar, or if anything more favourable to the former, are at opposite ends of the scale of height. Those not accustomed to the difficulties both of making and recording such measurements will scarcely be prepared, however, to learn how meagre, unsatisfactory and unreliable our knowledge of the stature of most of the races of mankind is at present, although unquestionably it has been considerably increased within recent years. We must, however, make use of such material as we possess, and trust to the future correction of errors when better opportunities occur.

It is convenient to divide men, according to their height, into three groups—tall, medium, and short; in Topinard's system, the first being those the average height (of the men) of which is above 1·700 metres (5 feet 7 inches), the latter those below 1·500 metres (4 feet 11 inches), and the middle division those between the two. In the last division are included certain of the Mongolian or yellow races of Asia, as the Samoyedes, the Ostiaks, the Japanese, the Siamese, and the Annamites; also the Veddahs of Ceylon and certain of the wild hill-tribes of Southern India. These all range between 1·525 and 1·600 metres—say between 5 feet and 5 feet 3 inches.

[1] A Lecture delivered at the Royal Institution on Friday evening, April 13, 1888, by Prof. Flower, C.B., LL.D., F.R.S., Director of the Natural History Departments of the British Museum.

[2] Herodotus, Book II. 32, Rawlinson's translation, p. 47.

It is of none of these people of whom I am going to speak to-day. My pygmies are all on a still smaller scale, the average height of the men being in all cases below 5 feet, in some cases, as we shall see, considerably below.

Besides their diminutive size, I may note at the outset that they all have in a strongly-marked degree the character of the hair distinguished as frizzly—*i.e.* growing in very fine, close curls, and flattened or elliptical in section, and therefore, whatever other structural differences they present, they all belong to the same primary branch of the human species as the African Negro and the Melanesian of the Western Pacific.

I will first direct your attention to a group of islands in the Indian Ocean—the Andamans—where we shall find a race in many respects of the greatest possible interest to the anthropologist.

These islands are situated in the Bay of Bengal, between the 10th and 14th parallels of north latitude, and near the meridian 93° east of Greenwich, and consist of the Great and Little Andamans. The former is about 140 miles long, and has a breadth nowhere exceeding 20 miles. It is divided by narrow channels into three, called respectively North, Middle, and South Andaman, and there are also various smaller islands belonging to the group. Little Andaman is a detached island lying about 28 miles to the south of the main group, about 27 miles in length and 10 to 18 in breadth.

Although these islands have been inhabited for a very great length of time by people whose state of culture and customs have undergone little or no change, as proved by the examination of the contents of the old kitchen-middens, or refuse heaps, found in many places in them, and although they lie so near the track of civilization and commerce, the islands and their inhabitants were practically unknown to the world until so recently as the year 1858. It is true that their existence is mentioned by Arabic writers of the ninth century, and again by Marco Polo, and that in 1788 an attempt was made to establish a penal colony upon them by the East India Company, which was abandoned a few years after; but the bad reputation the inhabitants had acquired for ferocious and inhospitable treatment of strangers brought by accident to their shores caused them to be carefully avoided, and no permanent settlement or relations of anything like a friendly character, or likely to afford any useful information as to the character of the islands or the inhabitants, were established. It is fair to mention that this hostility to foreigners, which for long was one of the chief characteristics by which the Andamanese were known to the outer world, found much justification in the cruel experiences they suffered from the malpractices, especially kidnapping for slavery, of the Chinese and Malay traders who visited the islands in search of *bêche de mer* and edible birds'-nests. It is also to this characteristic that the inhabitants owe so much of their interest to us from a scientific point of view, for we have here the rare case of a population, confined to a very limited space, and isolated for hundreds, perhaps thousands, of years from all contact with external influence, their physical characters unmixed by crossing, and their culture, their beliefs, their language entirely their own.

In 1857, when the Sepoy mutiny called the attention of the Indian Government to the necessity of a habitation for their numerous convict prisoners, the Andaman Islands were again thought of for the purpose. A Commission, consisting of Dr. F. J. Mouat, Dr. G. Playfair, and Lieut. J. A. Heathcote was sent to the islands to report upon their capabilities for such a purpose; and, acting upon its recommendations, early in the following year the islands were taken possession of in the name of the East India Company by Captain (now General) H. Man, and the British flag hoisted at Port Blair, near the southern end of Great Andaman, which thenceforth became the nucleus of the settlement of invaders, now numbering about 15,000 persons, of whom more than three-fourths are convict prisoners, the rest soldiers, police, and the usual accompaniments of a military station.

The effect of this inroad upon the unsophisticated native population, who, though spread over the whole area of the islands, were far less numerous, may easily be imagined. It is simply deterioration of character, moral and physical decay, and finally extinction. The newly-introduced habits of life, vices, and diseases, are spreading at a fearful rate, and with deadly effect. In this sad history there are, however, two redeeming features which distinguish our occupation of the Andamans from that of Tasmania, where a similar tragedy was played out during the present century. In the first place, the British Governors and residents appear from the first to have used every effort to obtain for the natives the most careful and considerate treatment, and to alleviate as much as possible the evils which they have unintentionally been the means of inflicting on them. Secondly, most careful records have been preserved of the physical characters, the social customs, the arts, manufactures, traditions, and language of the people while still in their primitive condition. For this most important work, a work which, if not done, would have left a blank in the history of the world which could never have been replaced, we are indebted almost entirely to the scientific enthusiasm of one individual, Mr. Edward Horace Man, who most fortunately happened to be in a position (as Assistant Superintendent of the Islands, and specially in charge of the natives) which enabled him to obtain the required information with facilities which probably no one else could have had, and whose observations "On the Aboriginal Inhabitants of the Andaman Islands," published by the Anthropological Institute of Great Britain and Ireland, are most valuable, not only for the information they contain, but as correcting the numerous erroneous and misleading statements circulated regarding these people by previous and less well informed or less critical authors.

The Arab writer of the ninth century previously alluded to states that "their complexion is frightful, their hair frizzled, their countenance and eyes frightful, their feet very large, and almost a cubit in length, and they go quite naked," while Marco Polo (about 1285) says that "the people are no better than wild beasts, and I assure you all the men of this island of Angamanain have heads like dogs, and teeth and eyes likewise; in fact, in the face they are just like big mastiff dogs." These specimens of mediæval anthropology are almost rivalled by the descriptions of the customs and moral character of the same people published as recently as 1862, based chiefly on information obtained from one of the runaway sepoy convicts, and which represent them as among the lowest and most degraded of human beings.

The natives of the Andamans are divided into nine distinct tribes, each inhabiting its own district. Eight of these live upon the Great Andaman Islands, and one upon the hitherto almost unexplored Little Andaman. Although each of these tribes possesses a distinct dialect, these are all traceable to the same source, and are all in the same stage of development. The observations that have been made hitherto relate mostly to the tribe inhabiting the south island, but it does not appear that there is any great variation either in physical characters or manners, customs, and culture among them.

With regard to the important character of size, we have more abundant and more accurate information than of most other races. Mr. Man gives the measurements of forty-eight men and forty-one women, making the average of the former 4 feet $10\frac{3}{4}$ inches, that of the latter 4 feet $7\frac{1}{4}$ inches, a difference therefore of $3\frac{1}{2}$ inches between the sexes. The tallest man was 5 feet $4\frac{1}{4}$ inches; the shortest 4 feet 6 inches. The tallest woman 4 feet $11\frac{1}{2}$ inches; the shortest 4 feet 4 inches. Measurements made upon the living subject are always liable to errors, but it is possible that in so large a series these will compensate each other, and that therefore the averages may be relied upon. My own observations, based upon the measurements of the bones alone of as many as twenty-nine skeletons, give smaller averages, viz. 4 feet $8\frac{1}{2}$ inches for the men, and 4 feet $6\frac{1}{2}$ inches for the women; but these, it must be recollected, are calculated from the length of the femur, upon a ratio which, though usually correct for Europeans, may not hold good in the case of other races.[1] The hair is fine, and very closely curled; woolly, as it is generally called, or, rather, frizzly, and elliptical in section, as in the Negroes. The colour of the skin is very dark, although not absolutely black. The head is of roundish (brachycephalic) form, the cephalic index of the skull being about 82. The other cranial characters are fully described in the papers just referred to. The teeth are large, but the jaws are only slightly prognathous. The features possess little of the Negro type; at all events, little of the most marked and coarser peculiarities of that type. The projecting jaws, the prominent thick lips, the broad and flattened nose of the genuine Negro are so softened down in the Andamanese as scarcely to be recognized, and yet in

[1] See "On the Osteology and Affinities of the Natives of the Andaman Islands" (Journal Anthropological Institute, vol. ix. p. 108, 1879); and "Additional Observations on the Osteology of the Natives of the Andaman Islands" (*ibid.*, vol. xiv. p. 115, 1884).

the relative proportions of the limb-bones, especially in the shortness of the humerus compared with the fore-arm, and in the form of the pelvis, Negro affinities are most strongly indicated.

In speaking of the culture of the Andamanese, of course I only refer to their condition before the introduction of European civilization into the islands. They live in small villages or encampments, in dwellings of simple and rude construction, built only of branches and leaves of trees. They are entirely ignorant of agriculture, and keep no poultry or domestic animals. They make rude pots of clay, sun-dried, or partially baked in the fire, but these are hand-made, as they are ignorant of the use of the potter's wheel. Their clothing is of the scantiest description, and what little they have chiefly serves for decorative or ornamental purposes, and not for keeping the body warm. They make no use of the skins of animals. They have fairly well-made dug-out canoes and outriggers, but only fit for navigating the numerous creeks and straits between the islands, and not for voyages in the open sea. They are expert swimmers and divers. Though constantly using fire, they are quite ignorant of the art of producing it, and have to expend much care and labour in keeping up a constant supply of burning or smouldering wood. They are ignorant of all metals; but for domestic purposes make great use of shells, especially a species of *Cyrene* found abundantly on the shores of the islands, also quartz chips and flakes, and bamboo knives. They have stone anvils and hammers, and they make good string from vegetable fibres, as well as baskets, fishing-nets, sleeping-mats, &c. Their principal weapons are the bow and arrow, in the use of which they are particularly skilful. They have harpoons for killing turtle and fish, but no kind of shield or breastplate for defence when fighting. The natural fertility of the island supplies them with abundance and a great variety of food all the year round, the purveying of which affords occupation and amusement for the greater part of the male population. This consists of pigs (*Sus andamanensis*), which are numerous on the islands, paradoxurus, dugong, and occasionally porpoise, iguanas, turtles, turtles' eggs, many kinds of fish, prawns, mollusks, larvæ of large wood-boring and burrowing beetles, honey, and numerous roots (as yams), fruits, and seeds. The food is invariably cooked before eating, and generally taken when extremely hot. They were ignorant of all stimulants or intoxicating drinks—in fact, water was their only beverage; and tobacco, or any substitute for it, was quite unknown till introduced by Europeans.

(*To be continued.*)

THE INSTITUTION OF MECHANICAL ENGINEERS.

THE Institution of Mechanical Engineers held its annual meeting at the house of the Institution of Civil Engineers in Great George Street, Westminster, on the 3rd and 4th inst., under the presidency of Mr. E. H. Carbutt.

The papers brought forward for reading and discussion were: the Third Report of the Research Committee of the Institution on Friction; "Description of the Emery Testing Machine," by Mr. Henry R. Towne, of Stamford, Connecticut, U.S.A.; and "Supplementary Paper on the Use of Petroleum Refuse as Fuel in Locomotive Engines," by Mr. Thomas Urquhart, Locomotive Superintendent, Grazi and Tsaritsin Railway, South-East Russia; the third of which was deferred till the next meeting of the Institute.

The third report of the Friction Committee is on experiments on the friction of a collar-bearing. The general conclusions of the Committee are that this kind of bearing is inferior to a cylindrical journal in weight-carrying power. The coefficient of friction is also much higher than for a cylindrical bearing, and the friction follows the law of the friction of solids more nearly than that of liquids, due doubtless to the less perfect lubrication applicable to this form of bearing compared with a cylindrical one. The coefficient of friction appears to be independent of the speed, but to diminish somewhat as the load is increased, and may be stated approximately at $\frac{1}{20}$ at 15 lbs. per square inch, diminishing to $\frac{1}{30}$ at 75 lbs. per square inch.

In the broad principles of construction on which the Emery system of testing and weighing machinery rests are included two radically new and highly important elements—namely, an arrangement of hydraulic chambers and diaphragms capable of receiving without injury pressures and shocks of great intensity, and of transmitting them simultaneously, without loss from friction, to a convenient point for the purpose of measuring and recording them, and capable also of reducing them to such lower term of degree as may be desirable; and a means for flexibly uniting a vibrating scale-beam either to a fixed abutment or to another beam of the same system, in such a manner as absolutely to eliminate friction, and to preserve indefinitely the fulcrum intervals or distances precisely as first adjusted, and to resist and transmit all the pressures and shocks to which the fulcrums are subjected, without in the slightest degree impairing their sensitiveness or durability.

The hydraulic construction is such that through it the strain on the specimen is transmitted without loss to a hydraulic chamber containing a thin film of liquid, which is again transmitted through a small copper tube, without loss from friction or otherwise, to a much smaller chamber containing a similar thin film of liquid. The acting area of the liquid in the smaller chamber is less than that in the larger in the proportion in which the load on the specimen is desired to be reduced before it is received upon the beams in the scale-case where it is measured. In the scale-case containing the weighing mechanism, the pressure transmitted from the smaller chamber is received at one end of a system of levers, and measured by means of devices which are shown in detail in the figures which accompanied the paper.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Among the courses of lectures announced for this Term we may notice the following:—

In Physics, Prof. Clifton is lecturing on Optical Properties of Crystals, and Mr. Selby on Absolute Electrical Units, at the Clarendon Laboratory. At Christ Church, Mr. Baynes lectures on Thermo-dynamics, and on the Transfer of Energy in an Electro-magnetic Field.

The University has made a grant to Mr. Smith, in aid of the Millard Engineering Laboratory, and practical work on the physical basis of engineering is regularly carried on there.

In Chemistry, besides the usual courses, Mr. Veley is lecturing on Thermo-chemistry, and Mr. Marsh on Recent Organic Research.

The work of the Geological Chair is at present being done by Mr. W. W. Watts (M.A. Camb.), who is lecturing for a term in order that Prof. Green may complete his session at the Yorkshire College.

Owing to Prof. Moseley's continued illness, Dr. Hickson is still acting as Deputy Linacre Professor, and is lecturing on the Morphology of the Chordata. Mr. Bourne, who is to assume his post as Superintendent of the Plymouth Marine Station in a month, is lecturing on Embryology, and Prof. Westwood on the Winged Arthropoda.

Dr. Burdon-Sanderson lectures this Term on Nutrition, and Dr. Gilbert on the Rotation of Crops.

In the absence of any Professor of Botany, Mr. J. B. Farmer is conducting the necessary elementary courses.

CAMBRIDGE.—Prof. Adams is appointed one of the four representatives of Cambridge at the 800th anniversary of the foundation of the University of Bologna, in June next.

An additional class-room for students of Mineralogy is to be formed.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, April 19.—"The Radio-Micrometer." By C. V. Boys.

The author gave the result of a mathematical investigation made with a view to arrive at the best possible construction of the radio-micrometer already described by him. At the conclusion of the meeting he showed in action an instrument which he had made, having the best proportions, which was both simpler in construction and far more sensitive than the one he exhibited on a previous occasion.

"On the Compounds of Ammonia with Selenium Dioxide." By Sir Charles A. Cameron, M.D., F.R.C.S.I., and John Macallan, F.I.C.

On passing dry ammonia into a solution of selenium dioxide in absolute alcohol, a compound is formed to which the authors have assigned the name ammonium selenosamate, and the formula NH_4,SeO_2NH_2. It is the ammonium salt of a new acid; namely, H,SeO_2NH_2. It is unstable, continuously evolving ammonia, and ultimately becoming a stable acid salt, $NH_4,H,(SeO_2NH_2)_2$. The neutral salt forms hexagonal prisms and pyramids, and the acid forms prismatic crystals. The neutral salt dissolves in 116 parts of alcoholic ammonia, but is decomposed by absolute alcohol or by water.

April 26.—"On the Modifications of the First and Second Visceral Arches, with especial Reference to the Homologies of the Auditory Ossicles." By Hans Gadow, Ph.D., M.A., Strickland Curator and Lecturer on Comparative Anatomy in the University of Cambridge. Communicated by Prof. M. Foster, Sec. R.S.

The phylogenetic development of the first two visceral arches shows us some most interesting changes of function, which we can follow upwards from the lower Selachians to the highest Mammals.

Originally entirely devoted to respiration as gill-bearing structures, the whole hyoidean arch becomes soon a factor in the alimentary system. Its proximal half forms the hinge of the masticatory apparatus, its distal half remains henceforth connected with the process of deglutition. Then this suspensorial arrangement is superseded by a new modification; the hyomandibula is set free and would disappear (it does nearly do so in Dipnoi and certain Urodela), unless it were made use of for a new function; with its having entered the service of the conduction of sound, it has entered upon a new departure, and it is saved from degeneration. The whole system of the one to four elements of the middle ear, which all have the same function as conductors of sound, is to be looked upon as *one* organ of *one* common origin,—namely, as a modification of the hyomandibula, the primitive proximal paramere of the second visceral arch.

Successive Modifications of the Mandibular and Hyoidean Visceral Arches.

I. Primitive condition (Notidanidæ). The palato quadrate bar alone carries the mandible. The second arch is indifferent. Hyomandibula and quadrate (the palatine part is an outgrowth) are both attached to the cranium.

II. The hyomandibula gains a fibro cartilaginous connection with the mandible, the masticatory apparatus becomes amphistylic and occasionally hyostylic (Rajidæ, most Selachians).

The hyoid gains a cranial attachment (many Rajidæ).

III. The quadrate or autostylic suspensorium becomes preponderant; the hyomandibula is, as in Teleosteans, divided into a proximal and into a distal (symplectic) element. The proximal part is received into a fenestra of the otic capsule, and is converted into a stapes, whilst the distal half either remains (*Proteus*, *Siren*, *Menopoma*) or is lost (other Urodela). The whole hyomandibula would have been lost owing to its excalation from suspensorial functions, unless it had entered the auditory service.

IV. The autostylic arrangement prevails. The whole hyomandibula remains, gains an attachment on the "tympanum" and differentiates itself into several conjointed pieces, notably stapes or columella proper, and extra-columella or malleus.

The extra-columella gains connection with the parotic cartilage; this connection frequently remains, but in *Anura* alone it conatins a special element of probably parotic origin.

The quadrate forms an important part of the tympanic frame.

IV*a*. Collateral departure of the *Anura*. The connection between the tympanal part of the hyomandibula with the mandible is lost.

V. The quadrate still forms the principal suspensorial part of the mandible. The extra-columella, or malleus, retains for a long time its previously acquired connection with Meckel's cartilage (*Amniota*).

V*a*. The top end of the hyoid is attached to the cranium (*Geckos*, *Mammalia*), and is occasionally fused with the extra-columella (*Hatteria*).

V*b*. Or, the proximal portion of the hyoid is removed from the skull and remains otherwise well developed (*most Lizards*); or its proximal portion becomes reduced and lost (*Chelonia*, *Crocodilia*, *Ophidia*, *Aves*).

V*c*. The extra-columella gains an attachment to the quadrate, squamosal, or pterygoid, whilst its connections with the mandible (*Ophidia*, *Chamæleon*), and the tympanum, are lost.

VI. The quadrate gradually loses its articulation with the mandible; the latter gains a new outer articulation with the squamosal; the quadrate acts almost entirely as a tympanic frame. Incus and malleus fuse sometimes with each other, and lean on to the parotic region. The masticatory joint is doubly concave-convex (*Monotremata*).

VII. The quadrate is converted into the principal part of the tympanic frame, viz. annulus tympanicus. The mandible has lost its articulation with the quadrate, and the masticatory joint is a single concave-convex one, the convexity belonging to the mandible (*Monodelphia*).

EDINBURGH.

Royal Society, April 2.—Rev. Prof. Flint, Vice-President, in the chair.—Prof. Crum Brown communicated a paper by Dr. Prafulla Chandra Rây on the conjugated sulphates of the copper-magnesium group.—Dr. John Murray read a paper by Mr. A. Dickie on the chemical analysis of water from the Clyde area.—Sir W. Turner read a paper by Prof. His on the principles of animal morphology.—Prof. Tait communicated two mathematical notes.

April 16.—Prof. Chrystal, Vice-President, in the chair.—Dr. Buchan gave an analysis of the *Challenger* meteorological observations, pointing out various important meteorological conditions the existence of which had been revealed by the work of the *Challenger* Expedition.—Dr. John Murray read a description of the rocks of the Island of Malta, comparing them with deep-sea deposits.—Prof. Chrystal described an electrical method of reversing deep-sea thermometers.—Dr. Thomas Muir read a paper on a class of alternants expressible in terms of simple alternants.—Prof. Tait communicated a quaternion note.

PARIS.

Academy of Sciences, April 30.—M. Janssen, President, in the chair.—On the consequences of the equality assumed to exist between the true and the mean value of a polynome, by M. J. Bertrand. The author shows by a rigorous demonstration that the rule is not justified which gives *a posteriori* the precise value of a system of observations, although this rule is frequently applied with complete confidence in its accuracy.—On the theory of the figure of the earth, by M. Maurice Lévy. The point here mainly discussed is the difficulty of establishing a satisfactory agreement between the theory of fluidity and that of precession in connection with Clairaut's differential equation and the subsequent researches of Lipschitz inserted in vol. lxii. of the *Journal de Crelle*.—Remarks in connection with Père Dechevrens' recent note on the ascending movement of the air in cyclones, by M. H. Faye. In order to solve by direct observation the question of the ascending or descending movement of the atmosphere in cyclones, Père Dechevrens has devised a special anemometer for his observatory of Zi-Ka-Wei in China. But he suggests that more trustworthy results might perhaps be obtained by fitting up a similar apparatus at a greater elevation from the ground; for instance, on the top of Eiffel's Tower, 300 metres high, now being erected in Paris. M. Faye accepts this suggestion, confident that, if carried out, it cannot fail to confirm his own views on the movement of the atmospheric currents in cyclones.—An elementary proof of Dirichlet's theorem on arithmetical progressions in cases where the ratio is 8 or 12, by Prof. Sylvester. In this demonstration the author starts from the following principle: To show that the number of prime numbers of a given form is infinite, let an infinite progression be constructed of integers relatively prime to each other, and each containing a prime number at least of the given form.—Distribution in latitude of the solar phenomena recorded during the year 1887, by M. P. Tacchini. A table is given of the spots, eruptions, faculæ, protuberances, as observed in each zone of 10° in the two solar hemispheres. The hydrogenic protuberances occur in all the zones, whereas the other phenomena were almost entirely restricted to the central region between 0° and ± 40°, as in the previous year. The spots, faculæ, and metallic eruptions present an agreement in the respective zones of maximum frequency between 0° and ± 20°; a maximum for each of the three orders of phenomena corresponds to the zone 0°–10° exactly as in 1886. The spots were confined to the equatorial zone (+30°–20°); the eruptions and the faculæ occurred at much higher latitudes, in fact as far as +50° and −60°. Hence there are zones with faculæ and eruptions, but without spots, while on a great part of the solar surface hydrogenic protuberances are observed in the total absence of spots.—In a second communication, M. Tacchini gives a summary of the solar observations made at Rome during the first quarter of the year

1888. From this summary it appears that the phenomena of spots and faculæ still continue to decrease, while the protuberances have increased. This confirms the remark already made that there is no close relation between these two orders of phenomena.—Determination of the heats of combustion of the isomerous acids corresponding to the formulas $C_4H_4O_4$ and $C_5H_6O_4$, by M. W. Louguinine. The constituent formulas of the fumaric and pyromalic, as well as of the mesaconic, citraconic, and itaconic acids have been the subject of frequent discussions amongst chemists. In order to throw some light on these obscure questions, the author here determines the heats of combustion of the acids in question. He concludes generally that fumaric differs greatly from pyromalic acid, the former being the lower homologue of one of the three acids with formula $C_5H_6O_4$. The formulas corresponding to these three acids are evidently closely related, the difference here being of quite another order from that which exists between the formulas corresponding to the fumaric and pyromalic acids.—On the slow combustion of certain organic substances, by M. Th. Schlœsing. The author's experiments with tobacco seem to show that the combustion arising in heaps of foliage, hay, and the like is in the first instance due to the action of micro-organisms, but with the increase of temperature it gradually assumes a purely chemical character. The influence of living organisms appears to cease between 40° and 50° C., after which the chemical action rapidly increases.

BERLIN.

Meteorological Society, April 10.—Dr. Vettin, President, in the chair.—Dr. Zenker communicated the second part of his research on the distribution of heat over the earth's surface. In the first part, of which he had spoken at the last meeting of the Society, he had shown that the distribution of heat depends not only upon the radiation from the sun and absorption by the atmosphere, but additionally upon the nature of the earth's surface, whether it is land or water. In previous researches on the distribution of heat, the mean values were determined from and based upon empirical observations; Dr. Zenker, on the other hand, has calculated the distribution of heat over the surface of the sea with the help of Hann's isothermal charts, starting with the temperature of a point on its surface which was quite uninfluenced by the neighbouring continents, and was consequently equally unaffected by any warm or cold currents. Using this factor, and the formulæ deduced in the theoretical part of his paper, he has calculated the distribution of heat from the pole to the equator for each successive parallel, and compared it with the distribution of solar radiation. As a basis for the distribution of heat over the surface of the land, it was first necessary to determine the conditions under which the influence of the neighbouring sea is either nothing or minimal in amount. The starting-point for this was the fact that the temperatures on continents exhibit very great variations, and from these was determined for each area, as a percentage, the relative influences of the sea and continent upon its temperature. The region where the influence of the sea was proved to be nil (or where, as the speaker said, the "continentality" was 100 per cent.) was in the neighbourhood of the east coast of Asia, whereas all other points were found to be affected by the neighbouring sea to a greater extent; the observed temperature on the land was therefore only partly dependent upon the position of the place on any given parallel, other influences making themselves more or less felt. Hence it was possible to calculate for each parallel the real and "accessory" temperature. The amount of heat radiated down from the sun was compared with these temperatures, and was found to be about the same for each 10° C. of difference in temperature; from 0°–10° C., however, quite considerable differences of radiation were necessary. In conclusion, Dr. Zenker compared the temperatures which really exist on the earth's surface with those which he had deduced, and found that in reality the climate on the sea of the southern hemisphere is colder than it should be according to calculation—a result which must be attributed to the oceanic currents of cold water. The continental climate in the northern hemisphere is slightly too warm, in consequence of the disturbance introduced by the Gulf Stream.—Lieutenant Moedebeck gave an account of a balloon journey which he made on March 31. The marked phenomenon during the same was the influence of rivers; thus, after the balloon had risen to a height of 300–500 metres, and was passing away over Berlin, it sank so rapidly over the Spree that when it was about 50 metres above the earth a large quantity of ballast had to be thrown out. At an elevation of 1200 metres he met with a long narrow rain-cloud, in passing through which the dry-bulb thermometer registered 1°·5 C., the wet-bulb 1° C.; at an elevation of 1300–1400 metres, both thermometers recorded the same temperature of 2°·5 C. At this height, and in circumscribed areas, a few very small semi-soft hailstones were observed. Shortly after this the balloon began to sink, and while still above the cloud, but at a lower level, somewhat larger but similar hailstones were observed for the second time. As soon as the balloon had passed through the cloud, rain fell for a short time, as the result of which the balloon was so weighted that it descended rapidly to the earth. The atmosphere above the cloud was not clear but rather misty.

BOOKS, PAMPHLETS, and SERIALS RECEIVED FOR REVIEW.

Land and Fresh-water Mollusca of India, Parts 1 to 6, and plates: Lieut.-Col. H. H. Godwin Austen (Taylor and Francis).—Botany for Beginners, 4th edition: Rev. Prof. G. Henslow (Stanford).—Botany of the Afghan Delimitation Commission (Linnean Society): J. E. T. Aitchison (Longmans).—Report on the Meteorology of India in 1886: J. Eliot (Calcutta).—Indian Meteorological Memoirs, vol. iv. part 4 (Calcutta).—Memoirs on the Winds and Monsoons of the Arabian Sea and North Indian Ocean: W. L. Dallas (Calcutta).—A Short Text-book of Electricity and Magnetism: T. Dunman (Ward, Lock, and Co.).—A Short Text-book of Sound, Light, and Heat: T. Dunman (Ward, Lock, and Co.).—A Life of Matthew Fontaine Maury: D. F. M. Corbin (Low).—An Illustrated Manual of British Birds, part 2: H. Saunders (Gurney and Jackson).—Bibliothek der Gesellschaft für Erdkunde zu Berlin (Berlin).—Essai de Définition et de Nomenclature; Les Dislocations de l'écorce Terrestre: E. de Margerie and Dr. A. Heim (Zurich).—Nature's Fairy Land: H. W. S. Worsley-Benison (Stock).—Evolution and its Relation to Religious Thought: J. Le Conte (Appleton, New York).—Record of Experiments conducted by the Commissioner of Agriculture in the Manufacture of Sugar from Sorghum and Sugar Canes, 1887–88 (Washington).—The Constants of Nature, 1st Supplement to Part 1: F. W. Clarke (Washington).—The Vegetable Resources of the West Indies: D. Morris (Silver).—Fruit: Dr. Crespi (Heywood).—Journal of the Royal Agricultural Society, April (Murray).—Quarterly Journal of Microscopical Science, April (Churchill).—Geological Magazine, May (Trübner).—Journal of the Society of Telegraph Engineers and Electricians, vol. xvii. No. 72 (Spon).—Schriften der Naturforschenden Gesellschaft in Danzig, Siebenter Band, Erstes Heft (Danzig).—Bulletin of the California Academy of Sciences, vol. ii. No. 8.

CONTENTS.

THURSDAY, MAY 17, 1888.

FLORA OF THE HAWAIIAN ISLANDS.

Flora of the Hawaiian Islands; a Description of their Phanerogams and Vascular Cryptogams. By William Hillebrand, M.D. Annotated and Published after the Author's death by W. F. Hillebrand. 8vo, pp. 673, with Frontispiece and Four Maps. (London: Williams and Norgate, 1888.)

THE Sandwich Islands, from a botanical point of view, are a group of peculiar interest. There are about a dozen of them, and they form an area of which the northern end falls just within the tropical zone, at a distance of 2000 miles from America, and separated from it by a deep gulf. From the nearest points of Polynesia proper, the Marquesas Islands and Tahiti, they are distant 1860 and 2190 miles. The largest island, Hawaii, is the most southern of the group. It has an area of about 5000 square miles, and its mountains, one of which is an active volcano, rise to a height of nearly 15,000 feet. The other islands, all taken together, are not equal to more than half the area of Hawaii. The capital of the group, Honolulu, is situated on the south side of the small island of Oahu. The average annual temperature of Honolulu is 75° F., the general range of the thermometer being from 70° to 83°, so that within an area about equal to that of Yorkshire we have every variation of temperature from equatorial heat to perpetual snow. Dr. Hillebrand estimates the total flora of the islands (Phanerogamia and Vascular Cryptogamia) at 999 species, representing 365 genera, and 99 orders. Of these 999 species, 653 are absolutely restricted to the Sandwich Islands, 207 native species are known elsewhere, 24 species were introduced by the natives in remote times, and 115 species are weeds of recent introduction. Leaving the introductions out of account, we have therefore a native flora of 860 species, of which three out of four are endemic. A vegetation thus individualized makes the group one of the most interesting fields of study in the world.

Dr. Hillebrand may be said to have devoted his life to the study of this question. He was born in Westphalia in 1821, and studied medicine at Göttingen, Heidelberg, and Berlin. After taking his degree, he settled down for a short time in practice in Germany, but his health soon broke down, and he sailed for Australia. After visiting the Philippine Islands and California, he made the Sandwich Islands his home, and his health became quite restored. He lived at Honolulu, mastered the language, and practised his profession with great success. He became private physician to the king, a member of the Privy Council, an active member of the Board of Health, and physician to the Queen's Hospital and the principal lunatic asylum. During twenty years he devoted his leisure to working out the botany of the group, and sent large collections to Kew and other European herbaria. He left the islands in 1871, but kept up a regular correspondence with various residents who were interested in botany, and who sent him further collections. He died in July 1886, just after completing the descriptive portion of this present work, which has been edited by his son, who lives in America, and who has prefixed to it the introduction which was drawn up by Mr. Bentham for our British colonial floras. His name is commemorated by the genus *Hillebrandia*, which is the only representative of the *Begoniaceæ* in Polynesia, and which was named after him by Prof. Oliver. The type specimens of the present work have been presented to the Berlin Herbarium, and the Prussian Government has made a grant towards the expense of its publication.

The book, which is dedicated to the Hawaiian people, consists almost entirely of careful descriptions, in English, of the orders, genera, species, and varieties, that form the flora, accompanied by full details of their distribution through the different islands, and the sort of places in which they grow. Nearly all the native plants are trees, shrubs, or perennial herbs. Comparing the islands with one another, Dr. Hillebrand's general view is that the flora of Kaui, the comparatively small north-eastern island of the group, is the richest and most individualized, and that of the large southern island of Hawaii, where the mountains rise the highest, is the most monotonous and least attractive. The total number of species here described as new is 180, but in some cases, as, for instance, by Mr. C. B. Clarke, in his "Monograph of the Cyrtandraceæ," issued in 1883, the publication of these has been anticipated, and the earlier names will have to be adopted. It is much to be regretted that the author did not live to work out fully his generalizations. A great deal has been written during the last few years on the general subject of plant-distribution, and in particular Wawra and Engler in Germany, and in England Wallace in "Island Life," and Hemsley in the "Botany of the *Challenger*," have discussed the various points of interest connected with the flora of these islands. What is wanted now is that Dr. Hillebrand's added facts should be compared together and summarized, and that the general conclusions which they establish should be carefully traced out.

The following is his outline of the zones of vegetation and their characteristics:—

"(1) *The Lowland Zone.*—Open country, grass-covered after the rains, with isolated clumps of trees, represented by *Paritium tiliaceum*, *Erythrina*, *Reynoldsia*, *Pandanus*, *Capparis*, *Gossypium*, *Abutilon incanum*. This includes also the littoral zone.

"(2) *The Lower Forest Zone.*—Tropical in character, its upper limit between 1000 and 2000 feet above the sea. Its physiognomy is marked distinctly by *Aleurites moluccana*, the pale foliage of which, in contrast with the green colour around, attracts at once the eye of the beholder. The woods are rather open; *Zinziber Zerumbet* covers the ground. *Cordyline*, *Eugenia domestica*, *Zinziber Zerumbet*, and other species, are strictly confined to it. *Pandanus odoratissimus* and *Paritium tiliaceum* do not pass beyond it, but *Freycinetia* does. To its upper portion, but extending also into the lower part of the next zone, belong also most *Sapotaceæ*, *Apocynaceæ*, *Gardenia*, *Psychotria*, *Maba*, most *Urticaceæ*, *Pisinia*, *Elæocarpus*, *Aurantiaceæ*, and others.

"(3) *The Middle Forest Zone.*—This lies within the region of clouds, and develops the greatest luxuriance in trees and jungle. *Pelea* and *Cheirodendron* are representative genera. The prevailing trees are indeed *Metrosideros polymorpha* and *Acacia Koa;* but, although they reach here their greatest development in size and number, they are not confined to this zone, but ascend above and descend below it. It is the home of all Rutaceous and most Araliaceous trees, the ubiquitous *Dodonæa viscosa*, *Alphitonia*, and *Coprosma*. The ferns luxuriate in it, and tree-ferns attain only here their full dimensions. Old trunks are wrapped in creeping ferns, mosses, and lichens. Here also the *Lobeliaceæ*, the peculiar pride of our flora, exhibit their most striking forms, invariably in isolated individuals. The upper limit of this zone may be drawn at an elevation of 5000 to 6000 feet.

"(4) *The Upper Forest Zone.*—This extends as high as 8000 to 9000 feet, and is characterized by stunted trees, chiefly *Sophora chrysophylla*, *Cyathodes*, *Myoporum*, arborescent *Raillardiæ*, *Wikstromiæ*, and *Coprosma Menziesii*. Between them luxuriate shrubby *Compositæ* (*Raillardia*, *Dubautia*, *Camphylotheca*, and *Artemisia*), with strawberries, brambles, and *Vaccinium*. Ferns are scarce, and mostly belong to widely spread species .Our shrubby *Geraniums* and silvery-leaved *Argyroxiphium* extend beyond this zone to the upper limit of vegetation, which on Mauna Kea may be placed at 11,000 feet. *Santalum* belongs to this zone and the upper levels of the last.

"(5) A place apart must be assigned to the bog flora of the high table-land of Kaui and the broad top of Mount Eeka, on West Maui. The turfy soil is covered with tussock-like *Gramineæ* and *Cyperaceæ*, all endemic species, with *Sphagnum*, creeping forms of woody *Metrosideros*, *Cyathodes*, *Geranium*, *Lysimachia*, and a number of rare, mostly single, representatives of genera which have their home in the Antarctic regions, New Zealand, the Falkland Islands, and the Southern Andes."

As a whole the flora of the Sandwich Islands stands out remarkably isolated from those of the two nearest great botanical regions, Polynesia and Central America, and has curious affinities with those of Australia, North America, the north temperate zone of the Old World, the Mexican highlands, the Andes, and the Antarctic regions. The subject is well worth working out in the same thorough way in which Sir J. D. Hooker has dealt with the floras of Tasmania and New Zealand.

Dr. Hillebrand's book is also valuable as a contribution to the study of varieties. In the Sandwich Islands we get a comparatively small number of species, that have lived for a long time in a country where there are great variations in temperature and humidity and little interference from man. In many of the endemic genera the species are very difficult to individualize, and he has named and characterized a great number of varieties. Altogether the book is of exceptional value, not only to the systematic botanist, but to all who are interested in the problems connected with the origin and distribution of species.

J. G. BAKER.

THE GEOLOGICAL EVIDENCES OF EVOLUTION.

The Geological Evidences of Evolution. By Angelo Heilprin, Professor of Invertebrate Palæontology at the Academy of Natural Sciences of Philadelphia. (Philadelphia: Published by the Author, 1888.)

FEW chapters in the "Origin of Species" are more impressive, from their perfect candour and their far-sighted prescience, than those dealing with the objections which might be urged against the author's hypothesis, on the ground of the comparatively small palæontological evidence in its favour. But this evidence, as every student knows, has been almost surprisingly strengthened and augmented during the thirty years which have elapsed since the publication of Darwin's great work. It is, however, owing to the nature of the case, scattered up and down various scientific periodicals, many of which are practically inaccessible to the general public, so that both its amount and its force are under-estimated, and the old objections are confidently reiterated by that still numerous class to whom "Darwinism" is a bugbear, and the very name of "evolution" an absolute abomination. As Prof. Heilprin states in his preface, "There has not thus far appeared, to the knowledge of the author, any collective or consecutive statement of the evidence which geology and palæontology present in support of organic transmutation;" so "with the view of partially filling this gap in the literature of Darwinism, the author has prepared, at the request of many of his friends, the following pages, which represent, somewhat broadened, the substance of a Friday evening discourse delivered at the Academy of Natural Sciences of Philadelphia." Thus this little book, while scientific in conception and method, is popular in style. While there is no attempt at an appeal to prejudices, scientific terminology is as far as may be avoided, and the illustrations appended enable any reader, with a very moderate knowledge of natural history and palæontology, to comprehend the line of reasoning followed by the author.

It is needless to add that he is a thorough going evolutionist, though, like his master, he is candid in admitting defects in the record, and transitions which as yet are merely hypothetical. In one case, however, he ventures on a statement which seems to us over bold: "It is not my purpose to-night to discuss the status of evolution, which has long since passed from the realm of pure and simple theory, but to present to you such of the more salient facts bearing upon its proof, drawn from my own department of geology and palæontology, as will permit you to understand why the greater number of naturalists consider the doctrine as firmly established to-day as is the Copernican theory of planetary revolution, the theory of gravitation, or the undulatory theory of light."

We cannot but think that, in making this confident assertion, Prof. Heilprin has exposed a joint in his harness to the arrows of his adversaries. In years to come, evolution, as stated by Darwin, may assume, probably will assume, the position of the above-named theories in physical science, but surely the evidence for it is not yet either so complete or so conclusive as for them. Hence it is unwise thus abruptly to exclude any possible modification or supplement. In scientific arguments it is better not to imitate the practices of political orators, but to err, if at all, on the side of understating rather than of overstating a conviction, and to impress by caution in reasoning rather than to dazzle by rhetoric.

This, however, is a matter of opinion: we pass on to indicate briefly the line of argument followed by Prof. Heilprin. At the outset he calls attention to two misconceptions relating to evolution which are widely prevalent, and are often made the ground of assaults upon the hypothesis. These are: that if the missing forms of life could all be recovered, they would form a continuous chain, and that "the progressive modification of individual organic forms need be, or indeed has been, one of continuous advance." Past and present organic life, as Darwin himself carefully pointed out, are combined, not in a continuous chain, but in a genealogical tree: "evolution recognizes modifications in the most divergent directions, and the tree of life that it restores is not a straight stem growing from a continuous apical bud, but a stem, or possibly even a limited number of stems, branching in varying directions." Thus the progress among organic beings is analogous to that in the development of civilization. "The united world advances, whereas individual tribes or nations remain at a standstill, or even degenerate and decay. Such is precisely the history of the organic development of our planet: new and more complicated organic types are being continually evolved, but side by side with these forms we still meet with those of a lower grade of organization, while still others, belonging to the earlier periods of the world's history, have completely dropped out."

After a brief sketch of the first appearance of vertebrate life, Prof. Heilprin describes the relations of the fishes, the amphibians, and the reptiles, indicating the affinities of the first and second, which have led Prof. Huxley to treat them as sub-groups of a single division, the Ichthyopsida. In the structure of the heart, mode of breathing, and nature of circulation, the young frog agrees with a fish, while in these respects the mud-fishes (*Ceratodus*) agree with the amphibians. Now this link between these great groups exists in very early times, as the hypothesis would demand. "Dipterus and its allies are fishes that belong to the Devonian period of time," and Ceratodus itself was living in the Permian, and thus "represents the oldest living vertebrate type known to naturalists." The peculiar structure of the teeth of the labyrinthodonts, found also in some of the earliest fishes, and still retained by the alligator-gar, is another link. Next, in regard to the date of the appearance of birds and mammals, which is sometimes regarded as rather anomalous, Prof. Heilprin points out that both the earliest birds and the earliest mammals have marked reptilian affinities, which in the former are very distinct, so that such forms as *Archæopteryx* and some of the early dentigerous birds on the one side, and the *Pterosauria* on the other, do much to link together the two classes. Further, the ancestry of the non-flying birds, such as Dinornis and its allies, may be traced with greatest probability to members of the Dinosauria, such as Iguanodon, Hadrosaurus, and Compsognathus. In like way the affinities of the monotremes with the reptiles are pointed out, and attention is called to the significant fact that "the earliest reptilian forms—those of the Permian period—are the only animals which possess the remarkable dental characters of the Mammalia."

In the second section of the book Prof. Heilprin deals more especially with the development of the Mammalia themselves, instancing the position occupied by the Eocene *Creodonta* between the now widely divergent *Carnivora* and *Insectivora*, the relationships among the groups of the former, and of the latter to the lemurs, the well-known pedigree of the horse, the ancestry of the hornless ruminants, the development of the horns of the deer, from the simple forked crown in the early Cervines of the Middle Miocene to the complicated forms assumed in the Pliocene and more recent times. *Cervalcas americanus*, the newly-discovered link between the Canada stag and the elk, also receives notice, as does the relation of the homocercal and heterocercal to the primitive diphycercal fishes. Attention is also called to the development of the brain in various vertebrates.

In the third section the author glances at the question of the antiquity of man. In regard to some of the alleged evidence he exercises a wise scepticism, and states that up to the present time he has been unable "to find satisfactory proof of man's belongings having been found in deposits very much (if at all) older than the Post-Pliocene," though he thinks it not unlikely that such may ultimately be found. In connection with this subject he mentions some human vertebræ, mineralized by limonite, of unknown but evidently high antiquity, discovered by himself in Florida.

Lastly, he calls attention to a class of evidence which the comparative persistency of conditions in certain parts of the United States has rendered accessible to American geologists—namely, the relation of living forms to their more immediate predecessors. Instances of this may be obtained in the sheltered regions of the Gulf of Mexico and in the comparatively modern rocks of the Florida peninsula. As examples, species of the genera *Strombus*, *Voluta*, *Fulgur*, and *Melongena*, are figured, showing the gradual transition from an extinct to an existing species, and to these are added a group of Paludinæ from the Middle Tertiary of Slavonia, illustrating successive varietal and specific forms.

The book is attractively written, though we must venture to protest against two instances of American-English: "The swift-footed animal . . . elevates the body so as to *weight* it principally upon the extremities of the toes;" and "the evidence is . . . but a mere *figment* of that which pertains to zoology." The first gains so little that brevity can hardly be pleaded as its excuse; the second, unless a misprint, is worthy of Mrs. Malaprop. T. G. B.

THE SHELL-COLLECTOR'S HAND-BOOK FOR THE FIELD.

The Shell-Collector's Hand-book for the Field. By J. W. Williams, M.A., D.Sc. Small 8vo, pp. 148 (interleaved). (London: Roper and Drowley, 1888.)

HANDY books for collectors, whether of birds, beasts, fishes, mollusks, or other organisms, are always most acceptable when well put together and carefully contrived, even if they be not original. The present little book might at first sight lay claim to having fulfilled all these conditions. It is small enough for the pocket, and the type is clear and legible; but when we enter upon the work itself, alas! we do not find our dream of a typical collector's hand-book realized by any means. Chapter I. "The Anatomy of a Snail," and Chapter II. "The Anatomy of a Fresh-water Mussel," should have been altogether omitted. They are not cleverly compiled, they are sadly full of mistakes, and these too clearly betray the fact that the author himself is not familiar with Mollusca from an anatomical point of view, but rather has got up his subject after the style of "Cousin Cramchild." Thus, the *colour* of the shell (says Dr. Williams), *exists entirely* in the *periostracum* or *epidermis*. We would advise the learned author to try and remove the epidermis from a snail-shell and observe the result.

The lip or aperture of a snail's shell is not generally called the *peritreme* but the *peristome*. The lines of growth in a snail's shell are not "arranged *concentrically* with the *nucleus*," although this is the case with the growth-lines in bivalves.

We fail to understand how the *operculum* of a snail "differs from the true shell in having more *conchiolin* entering into its composition." Surely the author meant to say *less conchiolin* and *more chitine?*

The *epiphragm*, or layer of hardened mucus, sometimes strengthened with carbonate of lime, closing the aperture of the shell of land-snails during hibernation is called here also the *clausilium!* (p. 5). The description of the odontophore with its radula and jaws (pp. 6 and 7) is very inaccurately rendered, and in copying Prof. Lankester the author has carefully also quoted a mis-statement as to the formula of the teeth.

The eggs of snails are said by the author to be "laid in a string, which is called the *nidamental ribbon*, or inclosed in *horny capsules*." This is true of sea-snails, such as the whelks (*Fusus*, *Buccinum*, &c.), but it is not the case in land-snails, of which Dr. Williams is discoursing. In these the eggs are separate and protected by a shell, which is sometimes membranous and flexible, at others calcareous and brittle, while those of the fresh-water species are deposited in small glairy masses of soft transparent jelly-like consistence.

Turning from the snail to the fresh-water mussel (Chapter II.), the author, in describing the animal of the latter, appears to have made a mistake similar to that which he has made with regard to the garden snail: not knowing his subject well, he has in fact described a *siphonated Mya*, when he fondly imagined he was writing about a *non-siphonated Unio* or *Anodon*.

Turning to the species enumerated by the author, we regret to observe that here the discrimination of the expert is alike wanting. For example, *Anodonta anatina*, Linn., figures as a good species, whereas it is merely a variety of *A. cygnea*, Linn. It seems rather absurd to give in a shell-collector's hand-book such shells as *Physa acuta*, Drap., "Hab. In one of the lily-tanks in Kew Gardens, imported" (p. 72); *Bulimus Goodallii*, Miller (introduced into a green-house with exotic plants); *Vertigo tumida*, Westerlund, another "casual"; *P. dilatatus*, Gould, in the canals around Manchester, "introduced from America in cotton bales." If these are admitted, why omit *Clausilia parvula* and *C. solida*, also "casuals," which appear both in Sowerby's last edition, and in Gwyn Jeffreys, v. 161–62?

Far too much prominence is given to worthless varieties of the common snail *Helix aspersa*, such as *minor*, *maxima*, *albida*, and *sinistrorsum*, &c.; but, having put them in, why should the author omit such a one as *Unio timidus* var. *ponderosa?* Many of the genera, too, need revision to be brought up to date. Thus, *Achatina acicula* should be *Cæcilianella acicula; Bulimus acutus* should be *Helix* (*Cochlicella*) *acuta; Zonites* should be *Hyalinia*. By the way, *Zonites draparnaldi* is omitted altogether, although known for years.

The habitats of many of the species are badly given. Thus, *Testacella Maugei* is said to be found in gardens and fields, whereas it has been met with in the neighbourhood of Bristol, whence it has spread to a few limited localities.

Why are the three known localities for *Vertigo moulinsiana* (p. 129) omitted?—Itchen Valley, near Otterbourne; near Hitchin; and near Rye-House, Herts. Other quite local species are recorded as if they occurred everywhere, as *Helix pisana* and *H. obvoluta*, &c.

A few woodcuts are inserted, but they are very poor and not accurately drawn. *Testacella haliotidea* is reversed.

The minute characters of the shells, so useful in many instances in the field, are omitted. The book is interleaved, which doubles its thickness for field-work, and we at first wondered why so much plain paper was added. It has since occurred to us that the author had the convenience of the reviewer in his mind's eye, and we must say we found the blank pages most useful in correcting the text as we turned over the leaves.

Is it too much to hope that the author may be able to give some attention to the living land and fresh-water Mollusca before he brings out a new edition of his handy shell-collector's manual, and so avoid those pitfalls into which he who compiles unskilfully and without practical acquaintance with his subject is sure to slip?

OUR BOOK SHELF.

A Text-book of Biology. By J. R. Ainsworth Davis, B.A., Lecturer on Biology in the University of Wales, Aberystwith. (London: Griffin and Co, 1888.)

THIS is one of a class of books which the system of examining the whole world on a limited schedule, drawn up by a Board of disinterested philanthropists, is bound to produce. It will delight the misguided student whose sole desire is "to get through" with the least knowledge possible, and will disgust every competent teacher. Mr. Davis is in error in stating that his book supplies a gap in literature. The little text-book by Prof. Lloyd Morgan is on the same lines, and appears to us to be far less objectionable, inasmuch as it is, though of smaller dimensions, a more genuine exposition of the principles of the subject, less of a cram-book than the present work, and written with maturer judgment and literary power. The only way to prevent the study of biology, as directed by the University of London, from sinking into a worthless exercise of memory applied to the contents of such little books as this by Mr. Davis, is to change the animals and plants enumerated in the schedule every three years. This, however, would hardly suit the ubiquitous aspirants to a degree for whom alone the Imperial University arranges its curriculum. Nor would it suit Mr. Davis and other more distinguished authors of regulation cram-books. The fact is that genuine education in biology as a science, and the influence of personal contact and association with an active investigator and discoverer as teacher and friend, are destroyed by the Imperial system of schedule and examination; and their place is taken by weary grinding at little books written by teachers of no authority, and too often ignorant as well as unintelligent.

Mr. Davis has borrowed a number of excellent figures to illustrate his book, which is nothing more nor less than a strictly limited, and in minor points an inaccurate, description of the types named in the schedule of the University of London. The new figures are bad, and the short general introduction is not merely shallow but erroneous, *e.g.* the account of protoplasm and the tabular statement of differences between plants and animals.

Reports of the Geological Survey of New Zealand.

THE issue of an index to the Reports of the Geological Survey of New Zealand, from 1866 to 1885 inclusive, enables us to see at a glance how large an amount of valuable material has been accumulated by the staff of this Survey, under its accomplished and energetic Director, Sir James Hector. Several editions of the useful geological map of the colony have appeared, the latest dated 1885; and the volumes containing the yearly reports of progress are now eighteen in number. Monographs on the palæontology of New Zealand are stated to be in preparation, and there are, besides these, museum and laboratory reports, meteorological returns, and miscellaneous publications. The difficulties felt in correlating the strata of so isolated an area as New Zealand with the rocks of other districts must always be very great, and it is therefore not surprising to find that warm and animated discussions are taking place among the different geologists of the colony as to the age and relations of some of the fossiliferous deposits. We may feel assured that the solution of these questions will be fraught with important results having a direct bearing upon some of the most difficult problems that now confront geologists.

First Lessons in Geometry. For the Use of Technical, Middle, and High Schools. By B. Hanumanta Rau, B.A. (Vepery: Printed at the S.P.C.K. Press, 1888.)

THIS is a second edition, revised and enlarged, of a very good book for those who are beginning the study of geometry. Much stress is laid all through on the construction and careful drawing of the figures, and great pains seem to have been taken by the author to make his meaning as clear as possible by means of simple examples, thereby inducing the reader not to learn the propositions by heart.

The volume is well arranged as regards the order of the subjects, and teachers, as well as taught, will find in it a good amount of useful information.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Dissemination of Plants by Birds.

I FULLY agree with Dr. Guppy that birds may have effected much more in the distribution of plants than is generally admitted, and I think it is most desirable that his suggestion respecting the examination of the contents of the crops of birds shot at sea in high southern latitudes should be carried out. At the same time I am of opinion that his explanation of the probable origin of the vegetation of the distant islands in the South Atlantic and South Indian Oceans is insufficient to account for the endemic element, unless we suppose a former belt of vegetation in a higher latitude than these islands, which is now extinct. Assuming the existence of such a belt of vegetation at some remote period, it would not be difficult to explain the relationships between the floras of America and Australasia, as well as the presence in these islands of plants not known to exist elsewhere.

Pringlea antiscorbutica, the Kerguelen cabbage, is the most remarkable of the endemic plants. As a genus, it is as well characterized as the majority of the genera of the Cruciferæ; but, what is more significant, it has no near ally in the southern hemisphere, being most nearly related to the northern genus *Cochlearia*, differing from it more in habit of growth than in any structural peculiarity. It is one of the commonest plants in the islands, from Prince Edward Group to the Macdonald Group, and produces seeds in great abundance.

Lyallia kerguelensis is, so far as is known, confined to Kerguelen Island. It is one of the degraded types of the Caryophylleæ-Polycarpeæ, and nearly related to the Andine genus *Pycnophyllum*, and the North Mexican genus *Cerdia*.

To my mind there are other difficulties in the way of such a derivation of this insular vegetation as that suggested by Dr. Guppy, but I will not enter into them here, as it would occupy too much space. W. BOTTING HEMSLEY.

On the Reappearance of Pallas's Sand Grouse (*Syrrhaptes paradoxus*) in Europe.

THIS bird suddenly reappeared at the end of April of this year at different localities of Central Europe, not having migrated so far since 1863. A. R. Wallace, in his important work, "The Geographical Distribution of Animals," published in 1876, figured this sand grouse among the characteristic birds of Mongolia (vol. i. p. 226, plate 3), and remarks:—"A curious bird, whose native country seems to be the high plains of Northern Asia, but which often abounds near Pekin, and in 1863 astonished European ornithologists by appearing in considerable numbers in Central and Western Europe, in every part of Great Britain, and even in Ireland." Vol. ii. p. 337, the same author says in the work quoted:—"*Syrrhaptes* normally inhabits Tartary, Thibet, and Mongolia to the country around Pekin, and occasionally visits Eastern Europe. But a few years back (1863) great numbers suddenly appeared in Europe, and extended westward to the shores of the Atlantic, while some even reached Ireland and the Faroes."

Mr. Wallace, speaking here of the geographical distribution of *Syrrhaptes*, has in view the two species of the genus, viz. *S. paradoxus*, Pallas, from Tartary and Mongolia, and *S. tibetanus*, Gould, from Thibet; whereas in the following sentence, treating of the extraordinary migration, only *S. paradoxus* appears to be meant. At least I am not aware that the second species has ever been observed in Europe.

Two years later not one bird of those that immigrated in 1863 appears to have been observed again here; they may have died, or been cruelly killed, or may have returned to their native steppes. No special notice having been taken of their movements, we did not learn the reason of that uncommon migration, nor the rapidity of their wandering, nor whether they returned to Asia or not.

The reappearance of the sand grouse in large flocks, consisting apparently of innumerable individuals, now gives us the opportunity of watching their movements in detail. This should be done everywhere, and for this reason I communicate the following notes, comprising all that I have learned till to-day about it. I am sure that many more observations will have been made in these days, and perhaps those who can add something to the following list will do so through the columns of NATURE. Observers should especially try to find out whether there are specimens of *S. tibetanus* among them.

April 21, Plock, Poland. On the same day specimens on the River Pilica, near Radom, and in the market of Warsaw, Poland.
,, 24, at 5 p.m., near Pirna, Saxony.
,, 25–26, in the night, near Leipzig, Saxony.
,, 26, Kalisch, Poland.
,, 27, 3 p.m., near Grossenhain, Saxony; on the same day several flocks there.
,, 27, 4 p.m., near Pirna, Saxony.
,, 27, Brandenburg, Prussia.
,, 27, Elbing, Prussia.
,, 27, near Leipzig, Saxony.
,, 28, near Leipzig, Saxony.
,, 28, Kuchelberg, Silesia.
,, 28, Czerwinsk, Poland.
,, 28, Warscha, Poland.
,, 29, Cernowitz, Bohemia.
On the last days of April near Görgény, Transylvania, and near Königstein, Saxony.
May 1, near Grossenhain, Saxony.
,, 1, Liobschütz, Saxony.
,, 1, Niederfaulbrück, Silesia.
,, 2, Ratzeburg, Holstein.
,, 2–3, in the night, near Grossenhain, Saxony.
,, 3, near Grossenhain, Saxony.
,, 3, near Bautzen, Saxony.
,, 3, near Schneeberg, Saxony.
,, 3, near Friedeberg, Silesia.
,, 4, near Grossenhain, Saxony; several flocks.

May 5, 4 a.m., near Grossenhain, Saxony.
,, 5, Island of Rügen, on the Baltic.
,, 6, near Freiberg, Saxony.
,, 6, near Königstein, Saxony.
,, 6, near Rendsburg, Holstein.
,, 7, Reichenau, Saxony.
,, 7, near Soldin, Brandenburg, Prussia.
,, 7, Palczyn, Posen, Prussia.
,, 7, near Leipzig, Saxony.

A. B. MEYER.

Royal Zoological Museum, Dresden, May 12.

"Coral Formations."

IN a recent paper read before the Royal Society of Edinburgh, I have pointed out the importance of taking into consideration the molecular condition of carbonate of lime in relation to its solubility in sea-water.

The (tabulated) results of an exhaustive series of tests (see NATURE, vol. xxxvii. p 605) show in a striking manner this difference between the crystalline (or massive) and the amorphous conditions of that body.

In Table II. the amount of carbonate of lime taken up by sea-water from decomposing shell-fish is shown to be very great, the clear newly filtered solution giving 0·384 grammes per litre (other determinations since made giving still higher results); this is due no doubt to the formation of carbonic acid, the result of the oxidation of the organic matter in the putrefying mass.

The clear (foul-smelling) liquid on standing exposed to the air rapidly decomposes, ammoniacal salts being formed; and a great portion of the amorphous carbonate of lime which was dissolved during the first stages of putrefaction is thrown out of solution and deposited in a crystalline and practically nearly insoluble form.

This may be due to the loss of carbonic acid, or its combination with ammonia, produced during decomposition of nitrogenous organic matter; or to the well-known action certain salts of ammonia (especially the carbonate) exert in degrading the solubility of carbonate of lime in water; but the result so produced, I think, meets all the objections Mr. T. Mellard Reade brings forward against the solution theory, which is Dr. Murray's explanation of the formation of coral lagoons.

Again, when a clear saturated solution of amorphous carbonate of lime in sea-water (see Table II., *a* and *b*) is allowed to stand for a few hours at ordinary temperatures, the solution becomes turbid and ultimately throws out in a crystalline condition a considerable proportion of the carbonate of lime it held in solution.

Dr. Murray, in a paper on "Structure, Origin, and Distribution of Coral Reefs, &c.," read before the Royal Institution, London, on March 16, refers to this change of condition as follows:—

"The whole of a coral reef is permeated with sea-water like a sponge; as this sea-water is but slowly changed in the interior parts it becomes saturated, and a deposition of crystalline carbonate of lime frequently takes place among the interstices of the corals and coral debris."

These facts seem to me quite sufficient to account for the formation of coral lagoons by the more rapid solution of the amorphous form of carbonate of lime, found in dead and decomposing corals. At the same time other deposits are preserved from wholesale solution by the change in the molecular condition which carbonate of lime undergoes,—always the after result of solution.

I need not here refer to other influences at work in maintaining the balance of absorption and secretion of lime salts in the ocean, because I consider the difference in solubility of various forms which carbonate of lime assumes equally accounts for the formation of lagoons and the preservation of coral reefs and shell beds or banks.

ROBERT IRVINE.

Royston, Granton, Edinburgh, May 14.

Aurora Borealis.

THE aurora borealis was visible here on Sunday night, May 6. We have difficulty in identifying it in this neighbourhood without spectroscopic aid, because the lights of Liverpool and its suburbs extend over the eastern horizon, and the sky to the north-east and north is filled with a glow from Bootle and Birkenhead, these several lights often giving, with clouds of varying height, effects resembling northern lights.

On Sunday night, at 1.30, the brightness in the north-western sky was not to be mistaken; and shortly before 2 o'clock a curved bluish-white beam—two brilliant sides inclosing a still brighter rounded angle of about 70°—shot up from the west, the apex coming first, and attaining a height of 60°, the sides there being about ½° broad; the extremities of the sides, 1° broad, touching the horizon in the north-north-west and south-south-east. This beautiful beam remained a few seconds, then went as it came, the apex disappearing last. The general phenomenon seemed to increase in brightness, but subsequent observations show that it could not then be satisfactorily distinguished from the early dawn and reflected lights.

L. J. H.

Rock Ferry, May 11.

Weight and Mass.

THE *weight* of a body is the quantity which is measured out by the operation of *weighing*. To *weigh* a body it is placed in one of the scales of a balance, and equilibrated by standard *weights* formed of lumps of metal called pounds, hundredweights, tons, &c., or kilogrammes in the metric system; and the sum of these weights is (*pace* Mr. R. E. Baynes) called the *weight* of the body.

The mathematician may now call this quantity, if he likes, the *mass* of the body; but the world at large uses the word *weight*, with the advantage of having the corresponding verb "to weigh," which the substantive "mass" does not possess: we are not yet accustomed to speak of a body "massing" 100 tons. The numerous circumlocutions to express one single idea in Prof. MacGregor's examples arise from the want of the verb "to mass."

The "extraordinary and peculiar" language is, then, that of the elementary text-books of Mechanics, which tell us that the *weight* of a body is the *force* with which it is attracted by the earth (Lodge, "Elementary Mechanics," p. 66).

It is true, as Sir Philip Magnus points out in his "Mechanics," § 46, that the word *weight* is made to do double duty, sometimes standing for *force* and sometimes for *mass*; and that these two significations must be carefully distinguished.

But the "ordinary he or she" would no more accept the "pull or heft required to lift a body" as a correct measure of the weight, than the Red Indian of to-day would accept the weight of the Hudson Bay factor's fist as one pound.

The theorist must then exert his ingenuity to invent a new word to express the *force* idea, to associate with the word *mass*, already invented by him; but to attempt to restrict the meaning of the word *weight* in a manner not usual in ordinary language can only lead to confusion. In any engineering, chemical, or ordinary journal we shall always find *weight* used in the sense of *mass*, as defined in the text-books of elementary dynamics; and even in these treatises we shall find in the parts on Statics the word *weight* used in its ordinary sense. For instance, on p. 196 of Dr. Lodge's "Mechanics," we find, Ex. 10, "A mass of wood (sp. gr. 0·6) is counterpoised by 105 correct grammes of iron (sp. gr. 7·5); find the mass of the wood (or its true weight *in vacuo*)."

Sometimes it is not possible to employ the balance to estimate the weight (or mass) of a body; as, for instance, when the chemist evolves a certain *weight* of hydrogen in a chemical combination, when the artillerist speaks of a gun *weighing* 110 tons, and when the astronomer "*weighs* the earth,"—in such cases the weight or mass, whichever it is called, is calculated as the product of the volume and the density: determine for example the weight of 1000 cubic feet of steel. The weight W (or mass M) is then found theoretically from the formula W (or M) $= \rho V$, but really practically from the formula $W = 62{\cdot}4sV$, where W or M is given in pounds, when V is given in cubic feet, and ρ is then called the density, and s the specific gravity (the *density relative to water*), and it is the specific gravity for which tables are given; but in the metric system W (or M) $= \rho V = sV$, where W or M is given in grammes, when V is given in cubic centimetres, and the density ρ, and the specific gravity s, are then the same. But turn to the ordinary text-books, and we find these confusing equations—

$$W = Mg = g\rho V = sV,$$

where W is called the weight, M the mass, ρ the density, and s the specific gravity, followed often by a series of absurd examples on changes of units.

These relations are derived from the equation $W = Mg$, the source of all confusion in Dynamics, and it is gratifying to find from Prof. Mendenhall that a crusade against it is in progress in America.

It is needless to repeat here the objections against this equation, but it is easy to see how it arose.

Mathematicians now measure mass in pounds, so that the mass of a body is the number of pounds of matter in the body (*the weight* in the vernacular); and the equation $W = Mg$ means that the weight of M pounds is Mg poundals, according to their definition that "the weight of a body is the force with which it is attracted by the earth"; but this was not so originally.

Early writers on Dynamics, before Gauss invented the absolute unit of force, always employed the statical gravitational unit, and then if a weight of W pounds was acted on by a force of P pounds, the equation of linear motion was $\frac{W}{g}\frac{d^2x}{dt^2} = P$.

To avoid the necessity of writing and printing $\frac{W}{g}$, it was replaced by the letter M, and called the *mass*; the unit of mass being thus g pounds. But now the invariable quantity, the mass, is measured in terms of a variable unit, while the variable unit of force is the attraction of the earth on a 1-pound weight.

Although such words as "a force equal to the weight of the mass of 10 pound weights" do not occur in Prof. MacGregor's book, they are strictly derived from his own definitions; and so is the following, "the weight of 32 pound weights on the Earth is at the surface of Jupiter a force of 71 pounds' weight." I bring forward these illustrations to show that the fine distinction between "10 pound weights" and "10 pounds' weight" is not workable; and to show that the addition of the word *weight* to *pounds* does *not* convey the idea of *force* in ordinary language, and is not clear even in the language of the precisionists.

Nor can the equation $p = g\rho z$ in Hydrostatics be defended, as capable of expressing a pressure in pounds on the square foot (or more commonly on the square inch); for, if Prof. MacGregor applies this equation to a numerical example, he will find himself dividing by g in one operation, only to multiply by g in the next. The unreal character of these changes of units is apparent when we come to numerical examples; the defect of our dynamical teaching is that the student is so rarely brought before a practical numerical illustration on a large scale.

The rest of Prof. MacGregor's remarks I must answer very briefly, for fear of occupying too much space.

The *kilometre* was designed to be the centesimal minute of latitude, to replace the *geographical or sea mile*, which is the sexagesimal minute of latitude; the quadrant of the earth is therefore 10,000 kilometres, or 10^9 centimetres, and $90 \times 60 = 5400$ geographical or sea miles.

The cosmopolitan unit of speed at sea is the *knot*, which is a *velocity* of one geographical mile an hour; if 10 knots, spaced about 50 feet apart, pass over the taffrail in half a minute, the vessel is said to be going 10 knots. All civilized nations measure speed at sea in *knots*, in French *nœuds*, German *knoten*, Dutch *knoopen*, Italian *nodi*, Spanish *nudos*, &c. In precision *knots an hour* is on a par with *atmospheres per square inch*.

It is unfortunate that we have not yet reached uniformity in the use of the words *elongation* and *extension*. The French treatises, and our practical writers, Rankine, Unwin, &c., use *tension* and *extension*, *pressure* and *compression*, to denote simple longitudinal stresses and their corresponding strains; the ratio of *tension* to *extension*, or of *pressure* to *compression*, being the *modulus of elasticity*. This variation in terminology must be settled by some arbitrator, say Prof. Karl Pearson.

In conclusion, speaking on behalf of engineers and practical men, I beg to say that the treatment of the subjects of weight, mass, and force, in our ordinary text-books of Mechanics, is by no means clear or satisfactory, and requires careful revision.

Woolwich, May 4. A. G. GREENHILL.

Density and Specific Gravity.

IF Mr. Cumming's definition of *specific gravity* be accepted, the confusion, already serious enough, in the minds of beginners in physics between mass and weight will be much increased. Surely the best and clearest definitions of *density* and *specific gravity* are those given in Glazebrook and Shaw's "Practical Physics," p. 105. These make *density* a quantity having dimensions in mass and space, and *specific gravity* a pure number. There are many advantages in defining *specific gravity* as a ratio, and not the least among them is that the numbers in tables of specific gravities are independent of any system of units, while in a table of quantities having dimensions the numbers given depend on the system of units used. Thus the *density* of platinum would have to be given in an English table as 1343·75 pounds, or in a metrical table as 21·5 grammes. Again we should lose the very useful analogies between the definitions of *density* and *thermal capacity* and *specific gravity* and *specific heat*, to which I drew attention in a letter to NATURE, vol. xxxiii. p. 391.

Prof. Carey Foster seems to think it would be useful to have a table telling us the force with which unit volume of any body is attracted towards the earth, and that this should be called a table of *absolute* specific gravities. But I fail to see any advantage in this, for it is adding a totally new definition to be remembered, and one which would certainly create confusion in a beginner's mind; and the objection applies to this, that the numbers given would depend on the system of units used, to say nothing of the value of gravity at the place for which the table was calculated. Supposing even that the latter were ignored, it is not more troublesome to convert, with the aid of the known weight of unit volume of water, the specific gravity of any material into the weight of a given volume of it, than to convert a number given in one system of units into the number representing it in the system we may happen to be using.

If we are to take Mr. Cumming's definition as he expresses it, I would submit that a pound avoirdupois is a quantity of matter and not a force; and to say that the specific gravity of water is 62·5 *pounds avoirdupois* is simply taking the density of water and calling it specific gravity. *Pace* Mr. Greenhill and the engineers, it is hard enough to eradicate the notion that the quantity of stuff in a body and the force with which it is pulled towards the earth are one and the same without having the task made more difficult by our definitions.

50 City Road, E.C. HARRY M. ELDER.

The Cornish Blown Sands.

IN the description of the raised sea beach at Newquay, which Sir Henry De la Beche has given in his "Survey of Devon and Cornwall," he makes no reference to a curious feature observable in a part of the beach, and to which I should like to direct attention, with a view to obtaining some explanation of the cause of its formation. As far as I know, the appearance is only to be found at one spot, on what is known as Little Fistrel, to the westward of the town. It consists of a number of *cylinders* of indurated sand, separated from each other by thin walls, often only an inch or two thick, and forming the base of the cliff or bank, which is perhaps 10 or 15 feet high at the place. These cylinders rest upon a bed of rock (argillaceous slate?), which runs down from the bottom of the bank to the sea in a series of shelving ledges. The cylinders, which are locally known as *Pixie Holes*, weather out from the bank, but unfortunately few or none of them are now to be seen in a perfect state, their walls having been broken down by people scrambling up the bank, and also by quarrying operations, which I learn have recently been carried on close by. I am told that formerly the cylinders were very perfect, and often of large size; I myself have seen them, fifteen or sixteen years ago, standing up like little towers along the base of the cliff, and I have often sheltered myself perfectly from a shower of rain by standing in one and covering myself with my umbrella. I have recently had a photograph taken of the best group to be found, and a copy of this, together with a piece of the wall of one of the cylinders, is with Mr. Goodchild, of the Geological Survey, Jermyn Street, who will show it to anyone interested in the matter; the size of one of the cylinders photographed is 51 inches deep and 28½ inches in diameter.

R. H. CURTIS.

[The sand in question is well known to geologists as an example of blown sand agglutinated into a compact stone by carbonate of lime derived from the solution of calcareous organisms, which here on the surface consist largely of land-snails. The tubular cavities are no doubt due to the removal of the calcareous cement by percolating water, and are thus of the same nature as the pot-holes in chalk, and the cavernous holes and tunnels in hard limestone.—ED.]

Self-Induction in Iron Conductors.

MR. SUMPNER quotes (NATURE, May 10, p. 30), in support of the idea that iron conductors may have less self-induction than copper ones of the same dimensions, a suggestion of mine that for very feeble magnetizing forces, iron may be diamagnetic That suggestion was confessedly speculative ; its basis was the notion that the Weber-Ampère electro-magnetic molecules suffer something akin to static friction when the process of magnetization attempts to bring them into alignment. Since it was thrown out, Lord Rayleigh has proved that the susceptibility of iron is constant, and has a fairly high positive value, for magnetic forces ranging from 0·03 to 0·04 C.G.S. downwards. Below the lowest force he has investigated, it is still conceivable that there may be a change in the susceptibility, but it is extremely improbable. In all likelihood, Lord Rayleigh's straight line in the curve of B and H or of I and H extends back to the origin. This at least is certain, that if there is any region at the beginning of magnetization within which the permeability is less than unity, or even no more than unity, it must be so infinitesimally narrow that its existence has no practical interest. For such magnetic forces as act on a lightning-conductor when a discharge is passing, iron is, beyond any question, strongly paramagnetic, and the self-induction with the iron conductor consequently greater than with the copper. J. A. Ewing.

Dundee, May 11.

Notes on the Reproduction of Rudimentary Toes in Greyhounds.

At the present writing, I have under my observation a fine male, light clay-coloured, smooth haired greyhound, which at certain intervals well illustrates the reproduction of the rudimentary digits of its feet, after they have been accidentally amputated. To-day this dog has growing on the inner aspects of both its fore and hind feet, and situated some 9 centimetres above the soles, on each limb, a strong rudimentary toe. If we choose, say, this toe on the right hind foot as an example of them all, we find it to be loosely attached, rather more than a centimetre long to the base of the claw, which latter is large and strong, powerfully curved, and fully as big as any of the claws on the foot phalanges. I further find that this toe has a well-marked pad on its under side, but careful examination fails to detect any bone in the proximal joint, from which I also infer that the ungual phalanx likewise lacks one, though this is not so easily determined without cutting through the horny theca forming the claw. About four months ago this dog was coursing hares over the prairie of this region, which chances to be overgrown with a stiff growth of sage-brush, about 2 feet to 3 feet high. The wiry stems of this plant, as the dog bounded among them, snipped off all four of these rudimentary digits, close down to the leg in each case, as nicely as though it had been done with a knife, leaving linear wounds about half a centimetre long. Now, instead of the lips of these wounds healing across, as one would naturally suppose they would, they immediately form the basis, in each case, for the growth of another rudimentary clawed toe, fully as perfect as the one which originally sprang from the same site. These subsequent growths take about three months to attain their full size again, when they are very likely to be removed by a similar process, and once more grow out as before, and so on indefinitely.

From several points of view, this case, as occurring in a vertebrate so high in the scale as a dog, has interested me very much indeed, and I further find that it is no uncommon thing to meet with greyhounds that have never possessed these rudimentary pollices and halluces, and it is fair to presume that in this race they are gradually disappearing.

R. W. Shufeldt.

Fort Wingate, New Mexico, March 28.

Dreams.

In discussing the differences between dreams and real life, Schopenhauer expresses the opinion that the distinction between these two activities of our representative power consists merely in the possibility of the representations of real life being connected in an uninterrupted successive series, while dreams resemble the separate pages of a book torn asunder, and put together again in complete confusion. Some personal observations of my own do not quite agree with this view. I have watched my dreams for some years, and have remarked that many of them are connected with one another in separate series. It happens to me very often that my dreams consist of a series of representations logically developed (although sometimes the logic is absurd) from other series of representations dreamed long before. It would be interesting to know if anyone else has observed anything of this kind. A. Bialoveski.

Oostkamenogorsk, Western Siberia, April 6.

"Antagonism."

Mr. Collins (Nature, May 3, p. 7) claims that Mr. Herbert Spencer anticipated Sir Wm. Grove and Prof. Huxley in the expression of the idea of *antagonism*. I think that priority to all of them must be given to the author of Ecclesiasticus in the Apocrypha, who says (chap. xlii., verse 24), "All things are double, one against the other. He hath made nothing imperfect." Thomas Woods.

Parsonstown, May 13.

SUGGESTIONS ON THE CLASSIFICATION OF THE VARIOUS SPECIES OF HEAVENLY BODIES.[1]

V.

Classification into Species.

We are now in a position to apply all that has gone before in a summarized statement of the various spectral changes, including those connected with hydrogen, which take place not only in these objects studied by Dunér, but in those others to which I have referred as forming the true beginning of the group.

The following statement, however, must not be taken as anything else than a first approximation to the real criteria of specific differences. I am convinced that further thought is required on it, and that such further thought will be well repaid.

The Sequence of the Various Bands in the Spectra of the Elements indicated by Bodies of the Group.

In comparing the spectrum of an element which has been mapped in the laboratory with the absorption bands in the spectrum of a "star," we need only consider those bands and flutings which stand out prominently and are the first to flash out when there is only a small quantity present. Thus, in the flame spectrum of barium there is an almost continuous background of flutings with a few brighter bands in the green, and it is only important to consider the *bands*, as the flutings would mainly produce a general dimming of the continuous spectrum. In order to show at a glance what portions of the spectrum of an element it is most important for us to consider in this discussion, I have reconstructed the map of low-temperature spectra which I gave in my previous paper, with reference to those elements which are indicated in the spectra of bodies of Group II. Five orders of intensities are represented, the longest lines, flutings, or bands being the brightest. The lines, flutings, or bands in the lowest horizon, in the case of each element, are those which are seen at the lowest temperature, and which are the first to appear when only a small quantity of substance is present. Those in the upper horizons are the faintest, and are only seen when the temperature is increased, or a considerable amount of the substance is volatilized. The map shows that if there are any indications of magnesium, for instance, in bodies of low temperatures, the fluting at 500 will be seen, possibly without the other flutings or lines. The first indications of manganese will be the fluting at 558, and so on. Again, on account of the masking effect of the spectrum of one element upon that of another, we may sometimes have an element indicated in a star spectrum, not by the brightest band or fluting in its spectrum, but by the second or even third in brightness ; this, of course, only occurs when the darkest band falls on one of the brightest flutings of

[1] The Bakerian Lecture, delivered at the Royal Society on April 12, by J. Norman Lockyer, F.R.S. Continued from p. 35.

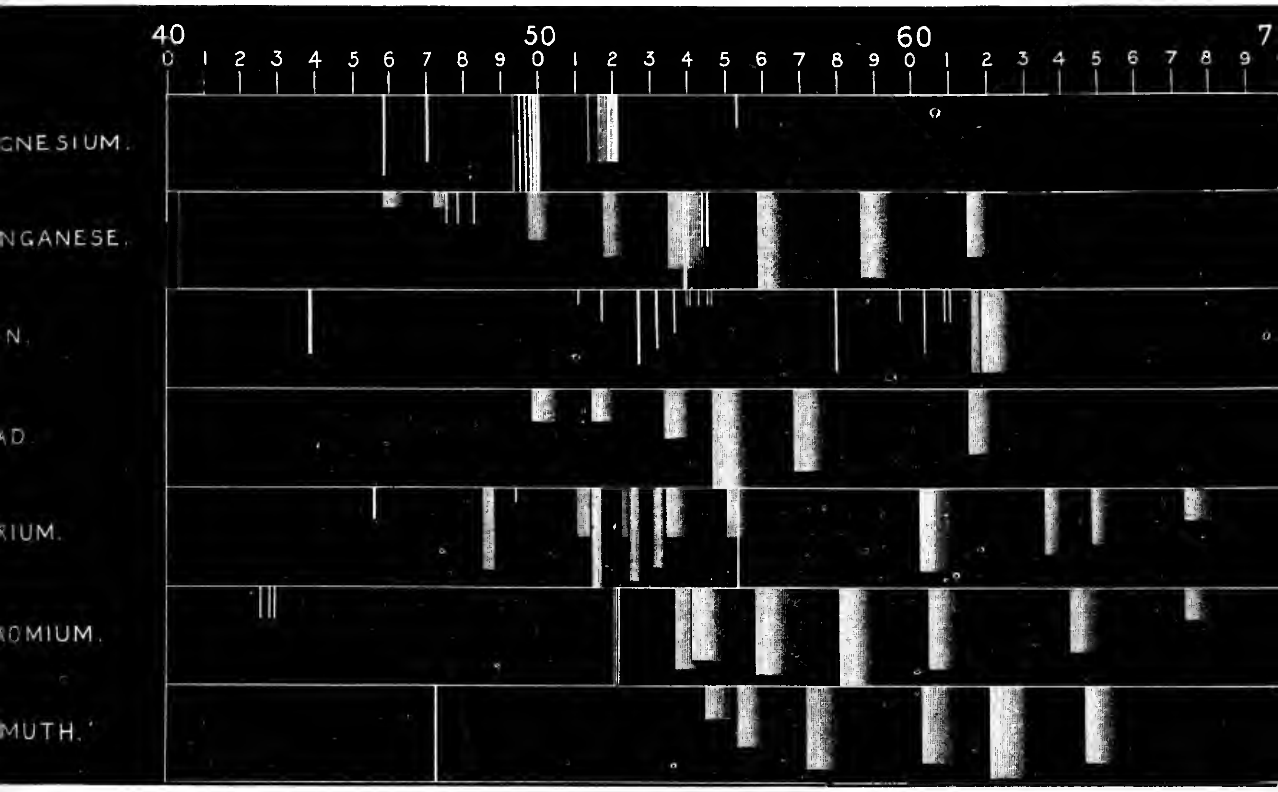


FIG. 8.—Map showing the lines, bands, and flutings seen in the spectra of the elements which are indicated in bodies of Group II. The map is intended to show also the relative intensities of the different lines, bands, and flutings, the lines, &c., seen in the lowest horizon being those seen at the lowest temperature.

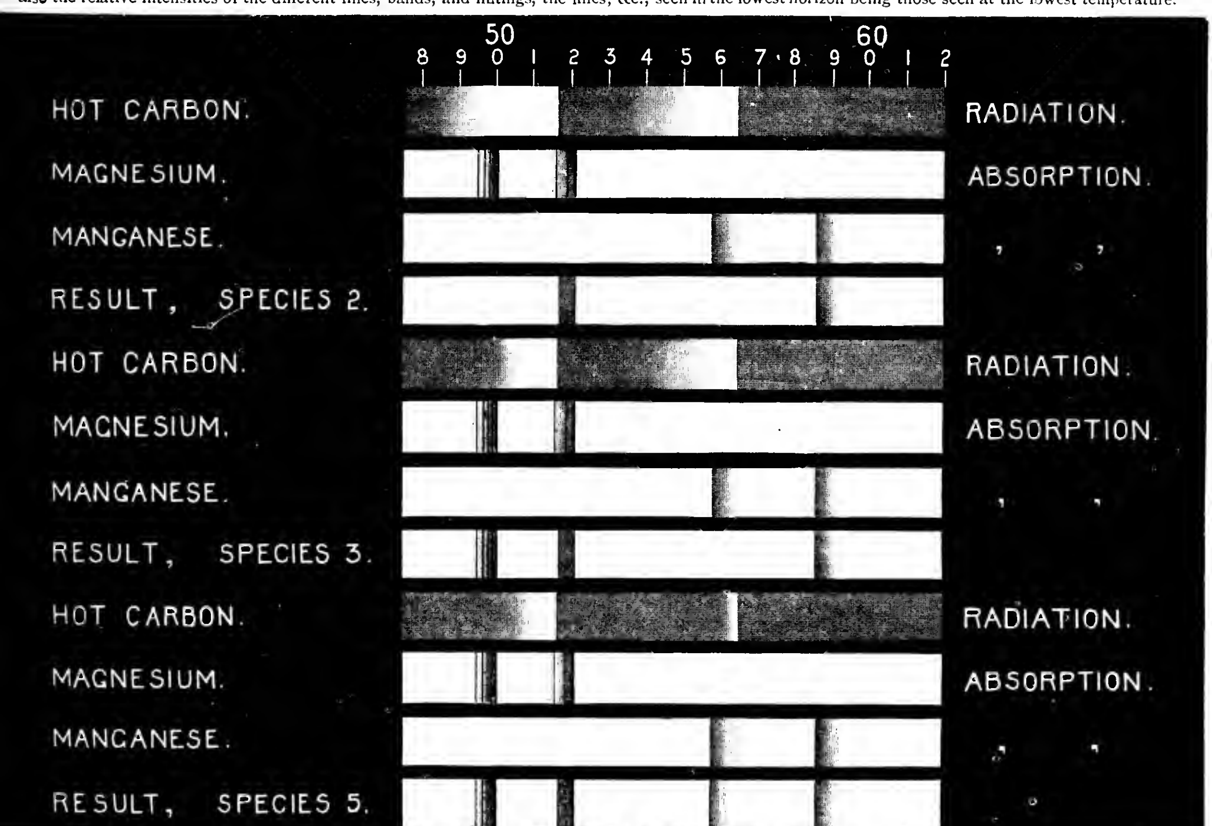


FIG. 9.—Diagram showing the effects of variations in width of the flutings of carbon upon the integrated spectra of carbon radiation, and magnesium and manganese absorption, as they occur in different species of bodies of Group II. The carbon radiation alone would give bright bands, while the absorption alone would give dark ones; but if the bright and dark bands fall in the same regions of the spectrum, the result will be enfeebled radiation, enfeebled absorption, or *nil*, according to the relative quantities of radiating and absorbing substances present. Thus, in species 2, the magnesium fluting at 500 is masked by the carbon fluting at 517, but as the quantity of carbon diminishes, it appears as an absorption band.

carbon, or upon a dark band in the spectrum of some other element. In the former case the dark band will be cancelled or masked; in the latter case the two absorptions will be added together, and form a darker band of a different shape.

The Question of Masking.

If we consider the masking effects of the bright carbon flutings upon the absorption spectrum of each of the elements which, according to the results obtained, enter into the formation of Dunér's bands, we have the following as the main results:—

Magnesium.—There are two flutings of magnesium to be considered, the brightest at 500 and the other at 5201. In the earlier stages of Dunér's stars only the fainter one at 5201 is visible, but the absence of the brightest at 500 is accounted for by the masking effect of the bright carbon fluting starting at 517. As the carbon fades, the 517 fluting narrows and the absorption of magnesium 500 becomes evident.

Manganese.—The two chief flutings of manganese are at 558 and 586, the former being the brightest fluting in the spectrum. The *second* fluting is seen in all of Dunér's stars. The first fluting, 558, however, does not appear as an absorption fluting until the radiation fluting of carbon starting at 564 has narrowed sufficiently to unmask it. It is thus easy to understand why, in some stars, there should be the second fluting of manganese without the first.

Barium.—The spectrum of barium consists of a set of flutings extending the whole length of the spectrum, and standing out on this as a background are three bright bands; the brightest band is at 515, the second is at 525, and the third, a broader band, is about 485. The *second* band is recorded as an absorption band in Dunér's stars, the apparent absence of the *first* band being due to the masking effect of the bright carbon at 517. The third band at 485 probably forms a portion of band 9. A fourth band, at 533, and the three brightest flutings at 602, 635, and 648 are also seen in α Orionis.

Lead.—The brightest fluting of lead is at 546. This first appears in species 5, as a result of increased temperature, and not on account of the removal of any previous mask. The second fluting of lead, at 568, also appears in two cases.

Chromium.—The flutings of chromium do not form portions of the ten principal bands of Dunér, but the brightest are seen in α Orionis. The brightest fluting is at 580, and this forms band I.; the second, at 557, is masked by the manganese fluting at 558, and the third at 536 is seen as line 2. The chromium triplet about 520, which is visible in the bunsen, is seen as line 3.

Bismuth.—The brightest fluting of bismuth is at 620, the second is at 571, the third at 602, and the fourth is at 646. The first is masked by the iron fluting at 615, the second is probably seen in α Orionis as band II. (570–577).

The points I consider as most firmly established are the masking effects of the bright carbon flutings and the possibility of the demonstration of the existence of some of the flutings in the spectrum by this means, if there were no other. There are two chief cases, the masking of the "nebula" fluting 500 by the bright carbon fluting with its brightest, less refrangible edge at 517, and that of the strongest fluting of Mn = Mn(1) 558, by the other with its brightest edge at 564. I have little doubt that in some quarters my anxiety not to be content to refer to the second fluting of Mn without being able to explain the absence of the first one, will be considered thrown away, as it is so easy to ascribe any non-understood and therefore "abnormal" spectrum to unknown physical laws; but when a special research had shown me that at all temperatures at which the flutings of manganese are seen at all, the one at 558 retained its supremacy, I felt myself quite justified in ascribing its absence in species 1–4 to the cause I have assigned, the more especially as the Mg fluting which is visible even in the nebula followed suit.

The Characteristics of the Various Species.

I append the following remarks and references to the number of the bodies in Dunér's catalogue, in which the specific differences come out most strongly, to the tabular statement. I also refer to some difficulties.

Sp. 1. The characteristic here is the almost cometary condition. All three bright carbon flutings generally seen in comets are visible; 474 standing out beyond the end of the dull blue continuous spectrum of the meteorites, 516 masking Mg 500, and 564 masking Mn(1) 558. The bands visible in the spectra of bodies belonging to this species will therefore be Mn(2) 586, and Mg(2) 521; band 9 will be so wide and pale that it would most likely escape detection. It is very doubtful whether any of the bodies the spectra of which have hitherto been recorded can be classed in this species, but laboratory work assuredly points to their existence; it will therefore be extremely interesting if future observations result in their discovery. It is possible, however, that No. 150 of Dunér's list belongs to this species, but the details are insufficient to say with certainty. His description is as follows:—"150. Il me parait y avoir une bande étroite dans le rouge, et une plus large dans le vert" (p. 55).

Sp. 2. Characteristics: appearance of Fe. The number of bands now visible is three—namely, 2, 3, and 7. The iron comes out as a result of the increased temperature. Mg(1) and Mn(1) are still masked by the bright carbon flutings, and there is still insufficient luminosity to make the apparent absorption band 9 dark enough to be noticed.

Sp. 3. Characteristics: appearance of Mg 500, which has previously been masked by the carbon bright fluting 517. 8 and 7 are now the darkest bands in the spectrum, 37.

Sp. 4. Characteristics: appearance of Pb(1) 546, *i.e.* band 5. This, if present in the earlier species at all, would be masked by the bright carbon at 564.

Sp. 5. Characteristics: Mn(1) is now unmasked. The bands now visible are 2, 3, 4, 5, 7, and 8, the two latter still being the widest and darkest, because they are essentially low-temperature phenomena.

Sp. 6. Characteristic: band 6, *i.e.* Ba(2), 525, is now added. The first band of Ba at 515 is masked by the bright carbon at 517. The bands now visible are 2–8, 7 and 8 still being widest and darkest. They will all be pretty wide, and they will be dark because the continuous spectrum will be feebly developed.

Sp. 7. Characteristics: appearance of band 9. This, which has been already specially referred to, has been too wide and pale to be observed in the earlier species. Its present appearance is due to the narrowing and brightening of the carbon at 474 and the brightening of the continuous spectrum, the result being a greater contrast. Bands 7 and 8 still retain their supremacy, but all the bands will be moderately wide and dark.

Sp. 8. Characteristics: all the bands 2–9 are more prominent, so that 7 and 8 have almost lost their supremacy.

Sp. 9. Characteristic: appearance of band 1, the origin of which has not yet been determined. All the bands are well seen, and are moderately wide and dark.

Sp. 10. Characteristics: appearance of band 10, and in some cases 11. These become visible on account of the brightening of the carbon B fluting and the hydrocarbon fluting at 431. The spectrum is now at its greatest beauty, and is discontinuous.

Sp. 11. Characteristics: the bands are now becoming wider, and 2 and 3 are gaining in supremacy; 7 and 8 become narrower on account of the increased temperature. 1 and 10 are only occasionally seen in this species.

Sp. 12. Characteristics: with the expansion of the continuous spectrum towards the blue, band 9 becomes very narrow, and cannot be observed with certainty. The other bands, with the exception of 7 and 8, are becoming wider and paler, while 2 and 3 still gain in supremacy.

Sp. 13. Characteristics: 9 has now entirely disappeared, 2 and 3 still retaining their supremacy.

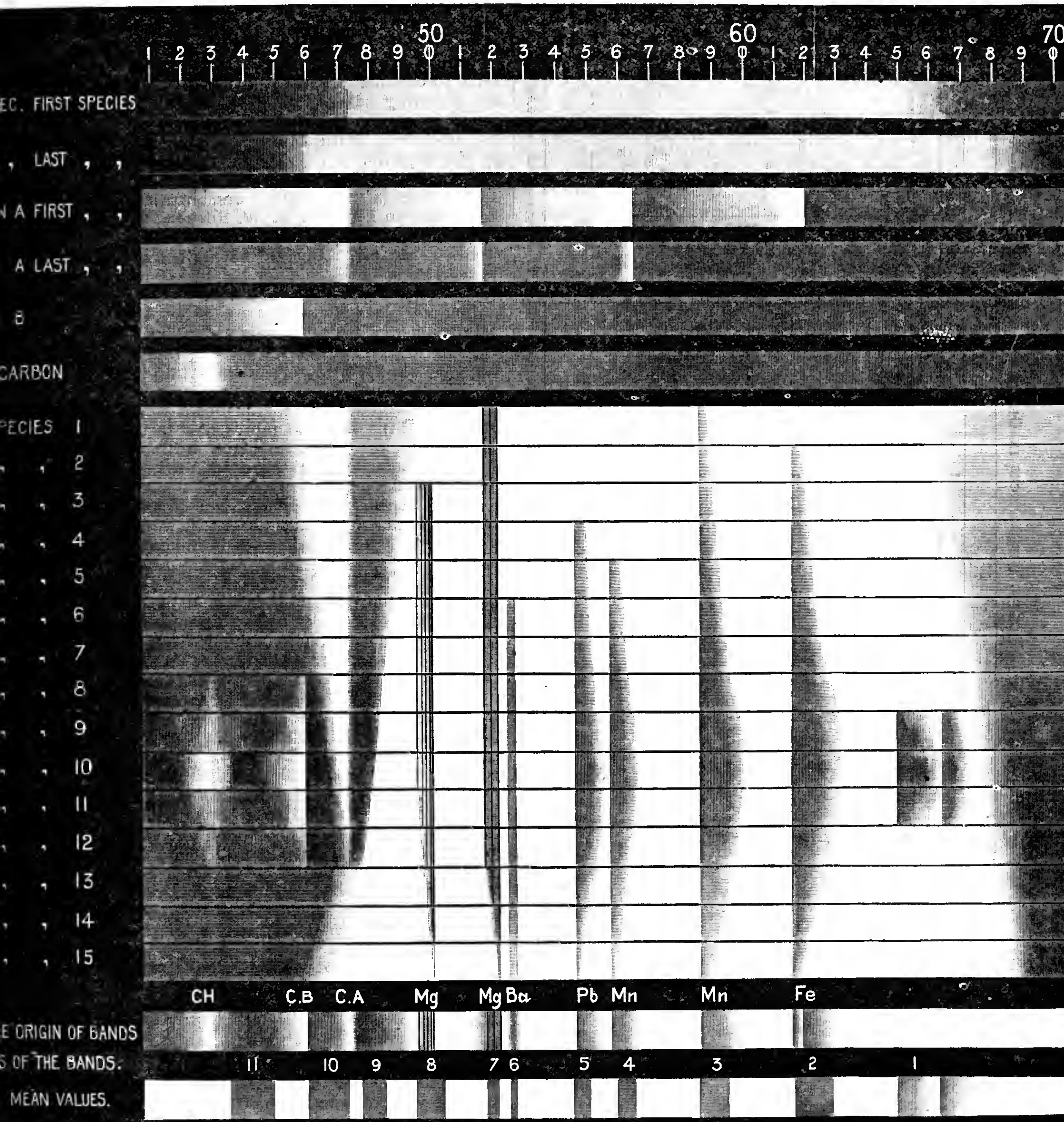


FIG. 10.—Map showing the spectra of the various species of the bodies of Group II., and the probable origin of the bands. The carbon flutings are widest in the first species, and gradually narrow until, in the last species, only a trace of 517 remains. The length of the continuous spectrum gradually increases as the carbon flutings narrow. The carbon B[1] fluting, and the hydrocarbon fluting are only seen in species 8 to 12.

Sp. 14. Characteristics: all the bands are pale and narrow; 2 and 3 will still be darkest, but the difference will not be so great as in the species preceding.

Sp. 15. Characteristics: in ordinary members of this group, 2 and 3 now alone remain visible: they are wide, but feeble, as the continuous spectrum which has been rapidly developing during the last changes is now strong.

TABLE A.—SPECIFIC DIFFERENCES IN GROUP II.

Species	Radiation flutings of carbon.					Absorption flutings.										Whether hydrogen lines.
	Hydro-carbon, 431.	Carbon B, 461.	474.	517.	564.	10.	9.	8. Mg.	7. Mg.	6. Ba.	5. Pb(1).	4. Mn(1).	3. Mn(2).	2. Fe.	1.	
1	━	━	Very wide and pale	Wide and pale	Wide and pale	━	━	If present mask'd by 517	Thin & dark	━	━	Present but mask'd by 564	Thin and pale	Absent	━	Yes
2	━	━	,,	,,	,,	━	━	,,	,,	━	━	,,	,,	Appears thin and pale	━	No
3	━	━	Narrowing & brightening	Narrowing & brightening	Narrowing & brightening	━	━	Appears dark	,,	━	━	,,	,,	,,	━	No
4	━	━	,,	,,	,,	━	━	Widens	Darkens	━	Appears dark	,,	,,	,,	━	No
5	━	━	,,	,,	Very narrow	━	━	,,	,,	━	,,	Unmasked dark	Darkens	Darkens	━	No
6	━	━	Weaker	Brighter and narrower	,,	━	━	Still darker and wider	Widens	Appears	,,	,,	,,	,,	━	No
7	━	━	,,	,,	,,	━	Appears	,,	,,	Darkens	Widens	Widens	Widens	Widens	━	No
8	Very narrow	Very narrow	,,	,,	,,	━	Darkens	Narrows	Narrows	,,	,,	,,	,,	,,		No
9	Widens	Widens	,,	,,	,,	,,	Strong	,,	,,	,,	,,	,,	,,	,,	Appears	Yes, bright and variable (possibly dark in α Orionis) ?
10	,,	,,	Fading	Fading	,,	Appears	,,	,,	,,	,,	,,	,,	,,	,,	Still present	
11	Narrows	Narrows	,,	,,	,,	Narrow in all but the brightest stars	Pales	Pales	Pales	,,	Pales	Pales	Now very broad and faint	Now very broad and faint	Fading	
12	Gone	,,	,,	Almost gone	,,	Disappears	Almost gone	,,	,,	Pales	,,	,,	Pales	Pales	Gone	No
13	━	Gone	,,	,,	,,	━	Gone	,,	,,	,,	,,	,,	Wide and faint	Wide and faint	━	No
14	━	━	Gone	? Gone	Gone	━	━	,,	Thin & faint	Thin & faint	Thin & faint	Thin & faint	Narrows	Narrows	━	No
15	━	━				━	━	Gone	Gone	Gone	Gone	Gone	,,	,,	━	No

NOTE.—The lecturer here referred to tables showing the bodies of this group recorded in Dunér's Catalogue arranged in species, in accordance with the above scheme.

(*To be continued.*)

THE ROYAL SOCIETY CONVERSAZIONE.

THE first *conversazione* of the season was held on May 9, and was very numerously attended. More pains than ever seemed to have been bestowed on the arrangements, and the results entirely justified them. As the carefully prepared programme covers eighteen closely printed pages, we can only give a very summary account of the most important demonstrations and exhibits.

Following recent precedents, the meeting-room was devoted to demonstrations by means of the electric lantern, the following being given: image of electric spark, by Dr. Marcet; Mr. Poulton's teeth of Ornithorhynchus, by Dr. Hickson; Forth Bridge, by Mr. Baker; collieries, by Mr. Sopwith.

The chief exhibits in the other rooms were as follow:—

Experiments on the optical demonstration of electrical stress, shown by Prof. A. W. Rücker, F.R.S., and Mr. C. V. Boys. These experiments are similar to those devised by Dr. Kerr, the arrangements being modified so as to render them suitable for exhibition in public. Conductors of various forms are immersed in bisulphide of carbon and placed between crossed Nicol prisms. When the conductors are oppositely electrified the medium is thrown into a state of stress, and the light which had been extinguished by the analyzing prism is restored. The various forms of conductors employed are—parallel cylinders, concentric cylinders, parallel planes, a plane and cylinder, and plates bent so as to represent a section of a Leyden jar. Many of the phenomena exhibited by crystals in plane polarized light are imitated—*e.g.* the black cross and the production of colours similar to those in Newton's rings. A bright field can be maintained by the introduction of a plate of selenite between the Nicols, in which case the electrical stress is indicated by change of colour.

Large electrical influence machine, exhibited by Mr. James Wimshurst. It has twelve disks of 2 feet 6 inches in diameter; each disk carries sixteen metal sectors. The machine is self-exciting in any condition of atmosphere. It shows large and perfect brush discharge at its terminals. With Leyden jars it will give sparks $13\frac{3}{8}$ inches in length.

Photographs of flashes of lightning, exhibited by the Royal Meteorological Society.

Radio-micrometer, exhibited by Mr. C. V. Boys. This is probably the most delicate instrument for measuring radiant heat yet made. It consists of a circuit made of antimony, bismuth, and copper hung by an exceedingly fine fibre of quartz in a strong magnetic field. A scale model of the circuit, twenty times the size or 8000 times the weight, shows the construction of the suspended part of the instrument. The fibre, if magnified to the same extent, would still be finer than spun glass. The proportions of the several parts are those which have been found by calculation (confirmed by experiment) to give the greatest possible delicacy.

Experiments with soap-bubbles, also shown by Mr. Boys. These experiments are arranged to show chiefly the power of an air-film to prevent two bubbles from coming into real contact. Thus, among other experiments, the outer of two bubbles may be pulled out until it squeezes the inner one into a long oval, but no real contact takes place. An inner bubble filled with gas will carry up an outer one to which are attached a wire ring and other things without really touching it at all. A bubble will roll down a spiral groove, also made of soap-film, or jump one or two steps at a time down a spiral staircase made of soap-film, without touching the spiral film or being injured in the least. Some of the experiments show the effects of diffusion, of vibration, of magnetism, or of electricity upon bubbles or groups of bubbles.

Maps and diagrams illustrative of the recent work of the Geological Survey in the North-West Highlands, exhibited by the Director-General of H.M. Geological

Survey. The maps, on the scale of 6 inches to a mile, show the remarkable geological structure of the west of Sutherland. A series of enormous dislocations runs in a southerly direction from the mouth of Loch Eriboll to Skye. By these disruptions the most ancient rocks have been torn up from great depths, and have been launched bodily westwards, sometimes for several miles. The displaced masses now rest upon other shifted portions or upon wholly undisturbed rocks, and the extraordinary structure is presented of vertical and highly inclined strata, with their unconformable junctions standing upon gently inclined and much younger rocks. The diagrams are taken across some of the more typical parts of the district, and give some idea of the physical problems presented by this region, which undoubtedly exhibits the most complicated geological structure in the British Isles.

Sections and specimens illustrating the recent borings in the Delta of the Nile, exhibited by Prof. J. W. Judd, F.R.S., on behalf of the Delta Committee. The whole of the samples obtained in these borings have now reached the Royal Society, and the examination of the materials reveals some facts of great geological interest. The alterations and mixtures of blown sand and Nile alluvium were found to continue down to the depth of 121 feet from the surface and 95 feet below the level of the Mediterranean. At that depth a remarkable change in the deposits took place, and beds of gravel containing both pebbles and subangular fragments of quartzite, chert, compact limestone, with some metamorphic and igneous rocks, were found; and similar beds occur at intervals down to the greatest depth reached. Up to the present time no contemporaneous organic remains have been found in these deposits.

Fossil plants from Ardtun in Mull, exhibited by Mr. J. Starkie Gardner. These plants are from a small patch of limestone beneath the gravels and silts of an old river course sealed up in the great trap flows of Western Scotland. The limestone is rather below the leaf-bed found at Ardtun by the Duke of Argyll, and directly overhangs the sea, the cliffs beneath being columnar and worn into caverns. The plants were until recently believed to be Miocene, but are now recognized to be very low down in the Eocene—*vide* recent writings of Sir W. Dawson and the Marquis de Saporta. The same plants ranged over Greenland and North America during the Tertiary, perhaps not synchronously, and an allied flora seems to exist at the present day in China and Japan.

Photographs illustrating experiments in mountain-building, exhibited by Mr. Henry M. Cadell, H.M. Geological Survey of Scotland. These have already been referred to in NATURE.

Set of thermometers specially constructed by Casella for use by Mr. Symons in determining the present temperature of the mineral springs in the Pyrenees, exhibited by Mr. G. J. Symons, F.R.S.; and Immisch's avitreous thermometer, constructed for the above investigation. This thermometer is absolutely perfect, its verification at Kew, before and after its use in the Pyrenees, being 0°·0 at all points from 50° to 130°.

An apparatus for determining the hardness of metals or other substances, exhibited by Mr. Thomas Turner.

Robertson's writing telegraph, exhibited by Mr. John M. Richards.

A Coulomb-meter, exhibited by Prof. George Forbes, F.R.S. This consists essentially of a conductor of iron wire in the form of a spiral, or a double ring with cross wires. Above the conductor a set of vanes is pivoted. This consists of a circular disk of mica with a hole in the centre in which is fixed a paper cone carrying at its apex a pinion with a concentric ruby cup. Round the circumference of the mica disk eight small cylinders of pith are fixed at equal distances, and eight vanes inclined at 45° to the mica disk are attached to the pith cylinders, these vanes being made of the thinnest mica. This set of vanes is supported by the ruby cup resting on a steel point fixed to the base of the instrument. The pinion engages with the first wheel of a train of wheelwork actuating the indexes, which show upon dials the number of revolutions made by the vanes. The action of the instrument is very simple. The electric current passing through the iron conductor creates heat, which sets up a convection current in the air, and this causes the vanes to rotate about the vertical axis and drive the clockwork. The number of revolutions indicated on the dials is, through a considerable range of currents, an exact indication of the number of coulombs or ampere-hours which have passedth rough the conductor. The friction of the ruby cup on the pivot determines the smallest current which can be accurately measured, and the friction of the clockwork is barely perceptible. The resistance of a meter to read from 1 ampere upwards is 0·02 ohm.

Electrical translucent balloon for flashing signals by night, invented and exhibited by Mr. Eric Stuart Bruce.

The new iridio-platinum incandescent gas-burner (Lewis and Sellon's patents), exhibited by Messrs. Johnson, Matthey, and Co.

Apparatus for measuring the changes produced by magnetization in the dimensions of rods and rings of iron and other metals, exhibited by Mr. Shelford Bidwell, F.R.S. The instrument exhibited is capable of measuring changes of length to a millionth of a millimetre or a twenty-five-millionth of an inch. An *iron* rod when magnetized becomes (as is well known) at first slightly lengthened. But if the magnetizing force is sufficiently increased it again contracts, and ultimately becomes actually shorter than when unmagnetized. A *cobalt* rod contracts under magnetization, reaching a minimum length in a field of about 500 C.G.S. units, beyond which point it becomes longer. A *nickel* rod also contracts; the limit of its contraction not having been reached with the greatest magnetizing forces yet used. *Bismuth* is slightly elongated in intense fields. (See Proc. Roy. Soc., vol. xliii., 1888, p. 406.)

Experiments illustrating low-temperature spectra, in connection with the spectra of meteorites, shown by Mr. J. Norman Lockyer, F.R.S.

Skeleton of an Akka, a Negro tribe from Central Africa, the smallest known race of men. (Height exactly 4 feet.) Sent by Dr. Emin Pasha for the British Museum, and exhibited by Prof. Flower, C.B., F.R.S.

Charts showing lines of equal values of the magnetic elements (epoch 1880)—declination or variation, inclination or dip, horizontal force (British units), vertical force (British units)—exhibited by Staff-Commander E. W. Creak, R.N., F.R.S. From the original charts at the Admiralty, compiled by Staff-Commander E. W. Creak, and prepared in their present form for the "Report on the Magnetical Results obtained in H.M.S. *Challenger*," in the concluding volume of the "Voyage of H.M.S. *Challenger*." The small maps show—(1) The track of H.M.S. *Challenger* where magnetic observations were made. (2) The approximate distribution of the secular change in the declination or variation (epoch 1840–80).

Photographs of the polar axis of a 5-foot telescope, December 1887, January 1888, exhibited by Mr. A. A. Common, F.R.S.

Sir William Thomson's models of foam or froth consisting of equal bubbles, exhibited by Prof. G. H. Darwin, F.R.S. Each bubble is a curvilinear fourteen-faced space. If a single bubble be dissected from the mass, it is found to be derived from the regular octahedron (two square pyramids base to base) by truncating the six solid angles. Thus the eight faces of the octahedron give rise to eight curvilinear hexagons, and the six solid angles to six solid curvilinear squares. In the foam three films meet at 120 at each edge, and of the three which meet two are hexagons and one is a square. (See *Phil. Mag.*, vol. xxiv., 1887, p. 503.)

Model of maximum pressure anemometer, designed by Mr. Whipple, Superintendent of Kew Observatory, exhibited by the Kew Committee. In this instrument eight small metal disks, each of 0·01 foot in area, are supported vertically against the wind by levers weighted in accordance with the various pressures of the wind on Beaufort's, or some other accepted scale of force. A vane keeps their surfaces normal to the wind's direction. By their displacement the maximum wind pressure during any desired period is registered. The large perforated disk against which they are pressed serves the purpose of removing the indicating disks beyond the action of the eddies of the wind playing round the edges of the plate.

Specimens of gold showing the effect of small quantities of impurity on the fracture of the metal, exhibited by Mr. W. C. Roberts-Austen, F.R.S.

Miners' electric safety-lamps, exhibited by the Schanschieff Electric Light and Power Company. (1) A three-cell lamp capable of giving 1½ candle-power for 9 hours. Each cell contains 5 fluid ounces of solution, and consumes ⅝ pound of zinc in 48 hours. The light is more than four times more powerful than that of the Clanny oil lamp, and its working cost is ⅝*d.* per shift of 9 hours, or 3¾*d.* per week. The weight when fully charged is about 3¼ pounds. The elements consist of carbon and zinc, and the excitant is a mercurial solution of Mr. Schanschieff's invention. (2 and 3) Four-cell batteries, one round and one square. Each cell contains 5 fluid ounces of solution, and at a cost of 1*d.* furnishes a light of nearly 2 candle-power for 9 hours. The weight when fully charged is 4½ pounds. (4) A four-cell reversible battery, *i.e.* put in or out of action by reversing it. The charge consists of 24 ounces of solution, and giving a light of 2 candle-power will burn from 10 to 12 hours at a cost of 1*d.* The batteries can be used for many purposes other than mining-lamps, viz. for microscopical purposes, house-lighting, photography, diving, railway-lighting, gun-firing, gas-works, &c.

THE ZOOLOGICAL SOCIETY OF AMSTERDAM.

THE celebration of the jubilee of the Zoological Society of Amsterdam (*Natura Artis Magistra*), on Tuesday and Wednesday, May 1 and 2, passed off with great *éclat*. Dr. Westerman, who has been Director of the Gardens for more than fifty years, may well be congratulated on the success of the jubilee *fêtes;* and the vigour with which he spoke at the banquet on May 1, and again at the distribution of honours on Wednesday, shows that his eighty years sit lightly upon him. One of the most interesting features of the jubilee commemoration was the performance of a festival cantata, specially composed for the occasion by Mr. De Langa, and this had to be repeated on Thursday for the benefit of half the members of the Society, for whose accommodation the enormous concert-room proved insufficient on the opening day. All the streets in the vicinity of the Zoological Gardens were gaily decorated with flags, and the rooms of the Society were ornamented in the day-time by a mass of gorgeous flowers and at night with brilliant illuminations. After the reception of the guests by the Committee on Tuesday morning, an adjournment was made to the King's Saloon, which was densely crowded, to hear an address from Prof. Stockvis. Luncheon followed, and then the cantata was given in the concert hall, and in the afternoon the new Ethnographical Museum was formally opened. The excellent way in which the collections had been arranged was generally remarked, and the Curator, Mr. Pleyte, was warmly congratulated. The public spirit which characterizes modern Amsterdam will doubtless soon cause this new Museum to become famous, as there is a vast field for research among the Netherland possessions in the East Indies. At the banquet in the evening, covers were laid for nearly 200 persons, and after the usual toasts, the health of the Queen of England was drunk by the assembled company with the greatest enthusiasm, and was responded to by Mr. Bowdler Sharpe, of the British Museum, who spoke in English, and took the opportunity of thanking the Dutch nation for the hospitality which he and his countrymen always received from the Netherlanders, to which he could testify from an experience of over twenty years. Speeches were also given by the Ministers of Finance and of the Interior, the Burgomaster of Amsterdam, and others; and the company then adjourned to witness a torchlight procession of students, who sent a deputation of their Senate to congratulate the venerable Director and the Committee of the Society. The young President of the Students' Senate, Mr. Van Schevichaven, made a most eloquent address, and was enthusiastically received. On Wednesday, May 2, a special reception of the Committee was held to confer diplomas on the new honorary members, and Prof. Hubrecht, of Utrecht, Dr. Jentink, the Director of the Royal Museum of Natural History at Leyden, and Mr. Büttikofer, of the same Museum, were the first recipients; being followed by Mr. A. D. Bartlett, the Superintendent of our Zoological Gardens in the Regent's Park, and Mr. Bowdler Sharpe. Amongst those who were unable to be present, but to whom the honorary membership of the Society was given, were Prof. Flowers, Dr. A. B. Meyer, &c. The large bronze medal of the Society was conferred on Mr. Charles Jamrach and Mr. G. A. Frank for services rendered in the formation of zoological collections, as well as on several other well-known zoologists. Mr. Jansen, the Librarian of the Society, and Mr. F. E. Blaauw, the Secretary, also received medals and diplomas. The latter gentleman has a large private menagerie, and is an enthusiastic supporter of the Society. Simultaneously with the festival celebration, the Society has issued a jubilee number of its *Bijdragen tot de Dierkunde*, containing several important memoirs, of which the following is a list:—(1) The opening address of Prof. Stockvis. (2) Mr. Maitland's review of the Society and its work, with a plan of the Gardens. (3) An account of the aquarium with 2 plates, by Dr. C. Kerbert, the Curator. (4) A list of all the animals which have lived in the Gardens from 1838 to 1888 by Mr. K. N. Swierstra. (5) A list of the birds of the Netherlands, by Mr. H. Koller, with an enumeration of the specimens in the Society's collection. (6) Description of a new species of *Proechidna* (*P. villosissima*) and an account of *Canis jubata*, by Prof. Max Weber: this article is illustrated by 2 plates. (7) A list of the *Macrolepidoptera* of Holland, by Dr. J. T. Oudemans. The Gardens of the Society seemed to be in flourishing condition, and the collections of Cranes and Antelopes were as remarkable as ever.

NOTES.

THE ceremony at Utrecht on May 28 to celebrate the seventieth birthday of Prof. Donders, and his consequent retirement from his Professorship, will comprise a formal presentation, at 1.30 p.m., of the sum collected, together with the roll of subscribers, and a public dinner at 5.30 p.m. After the ceremony of presentation the Professor will name the scientific purpose to which he proposes that the fund shall be applied. The complete list of subscribers from this country is to be seen in our advertising columns on page xviii. Any subscriber may verify the amount of his subscription by applying to Mr. Brailey, 11 Old Burlington Street, where the audited list may be seen. The total amount collected here is £280 11*s.* 10*d.* Prof. Humphry, Dr. Hughlings

Jackson, Mr. Hutchinson, and Mr. Brailey have been invited to attend as delegates to represent the subscribers, and it is hoped that many others may be able to attend, and by their presence do honour to Prof. Donders.

THE meeting of the National Academy of Sciences, lately held at Washington, seems to have been remarkably successful. According to *Science*, the most important papers read at the meeting were, the orbits of aërolites, by Prof. H. A. Newton; preliminary notice of the object, methods, and results of a systematic study of the action of definitely related chemical compounds upon animals, by Profs. Wolcott Gibbs and Hobart Amory Hare; and report of progress in spectrum photography, and note on the spectrum of carbon and its existence in the sun, by Prof. H. A. Rowland. Prof. Newton, in his paper, submitted the two following propositions:—(1) The meteorites which we have in our collections, and which have been seen to fall, were originally (as a class, and with a very small number of exceptions) moving about the sun in orbits that had inclinations to the ecliptic of less than 90°; that is, their motions in the solar system were direct and not retrograde. (2) The reason why we have only this class of stones in our collections is not a reason wholly, or even mainly, dependent on the habits of men; nor on the times when men are out of doors; nor on the places where men live; nor on any other principle of selection acting at or after the arrival of the stones at the ground. Either the stones which are moving across the earth's orbit in the solar system move in general in direct orbits, or else, for some reason, the stones which have retrograde orbits do not in general come through the air to the ground in solid form.

Two gold medals were presented at this meeting: the Lawrence Smith gold medal to Prof. Newton for his study of meteors; the Henry Draper gold medal to Prof. E. C. Pickering for researches in stellar photography. On the evening on which these presentations were made the following obituary memoirs were read: on the late Prof. Henry Draper, of New York, by Prof. G. F. Barker, of the University of Pennsylvania; on Prof. Watson, of the University of Michigan, by Prof. Comstock; on Capt. J. B. Eady, by Mr. W. Sellers, of Philadelphia.

WE are glad to see that the National Association for the Promotion of Technical Education is hard at work, and that it is likely to do excellent service to the cause it supports. In reply to circulars sent out in August and September 1887 a good deal of information has been provided from various industrial centres, which it is hoped may form the basis of a fairly complete report as to what is being done for technical education in the United Kingdom at the present time. Meetings have been held in a good many towns, and in some cases branches or corresponding Associations have been established. The Association is also issuing a series of publications, each consisting of a page or two, and presenting in a clear, popular style some important aspect of the subject. Some of these papers are sold at sixpence, others at a shilling, per hundred, and we may hope that large numbers of them will be widely circulated. In a series of more elaborate publications the Association has included the admirable address delivered by Prof. Huxley at a meeting held in the Town Hall, Manchester, on November 29 last.

COLONEL TURNER'S Report on the present state of the borings in the Delta of the Nile has been received at the Royal Society. The total result of the whole operations is to prove that no rock exists at a depth of 345 feet at Zagazig; at a depth of 45 feet at Kasr-el-Nil; at 84 feet at Kafr-Zayat; or at 73 feet at Tantah.

THE May number of the Kew Bulletin contains an interesting paper, giving an account of the attempts that have been made to introduce ipecacuanha into India, and the successful cultivation of the plant in the Straits Settlement. There are also valuable papers on Brazilian gum arabic, Trinidad coffee, patchouli, Cochin China vine, Madagascar ebony, and Shantung cabbage.

ABOUT a year ago the Botanical Department, Jamaica, began to issue Bulletins. Six numbers have been sent to us, and each of them contains some contribution or contributions worthy of attention. The compilers very wisely keep local industrial needs steadily in view.

IN a Report on the province of Florence, just laid before Parliament, Mr. Colnaghi, British Consul-General, says that meteorological stations, both public and private, are now established at the following places in the province:—Florence (5), Fiesole, Vallombrosa, Prato, Pistoia, Scandicci, Empoli, Fiorenzuola, Castaletti, and thermo-pluviometrical stations at S. Miniato, Mercatale (in Rocca San Casciano), Pontassieve, and Barberino di Mugello. Amongst the more important of these, he mentions the Observatory of the Royal Museum of Physical Science, that attached to the medical section of the Reale Istituto di Studi Superiori, chiefly devoted to the study of the variations of the atmosphere, and the Osservatorio Ximeniano, which is, at the same time, astronomical, meteorological, and seismical, and is under the direction of the Fathers of the Scuole Pie. For many years, he adds, experiments have been made by Prof. F. Meucci, of the Observatory of the Royal Museum of Physical Sciences, for the purpose of ascertaining the correlation of meteorological phenomena with the productiveness of the soil, and a series of Reports have been published. In 1880 the Royal Tuscan Society of Horticulture established, in its experimental garden at Florence, a Meteorological and Physical Observatory, by means of which the relation existing between the vegetation of plants and the meteoric phenomena can be studied. The Royal Astronomical Observatory of Florence is established at Arcetri, and is under the direction of Prof. Tempel.

VOLUME X. of the *Repertorium für Meteorologie*, issued by the Imperial Academy of Sciences of St. Petersburg, and edited by Dr. H. Wild, contains, among other interesting discussions, one upon the anticyclones in Europe, by Dr. P. Brounow. He has investigated by means of synoptic charts the barometrical maxima which passed over Europe in the years 1876-79, with especial reference to their movements and their causes—questions which up to the present time have received but little attention, although they are intimately connected with the movements of cyclonic areas. The number of the maxima whose paths are drawn on the charts, are most frequent in August, and least so in July and March; and, generally speaking, their motion is towards east-south-east, while their motion towards the north-westerly portion of the compass is very rare. Among the chief results of his inquiry may be mentioned that the prevalent movement of the maxima does not coincide with that of the barometric minima, but deviates from it by an angle of $67\frac{1}{2}°$. There appears to be no important difference in the mean velocity of their motion in different seasons, and although they move more slowly than the depressions, the difference of velocity is not so great as is generally assumed. Their origin is attributed to two principal causes: (1) terrestrial radiation, and (2) the proximity of two or more barometric minima. The work is accompanied by sixteen charts, from which the author concludes that the maxima advance generally in the direction in which the lowest temperature exists, and that the lower the temperature sinks the quicker the centre of the maximum advances, without reference to the season of the year.

AN important addition to the chemistry of the element tellurium is contributed by MM. Berthelot and Fabre to the May number of the *Annales de Chimie et de Physique*. They find that this metalloid, one of the most remarkable links between the non-metals and true metals, is capable of existing in three distinct allotropic forms. Besides the well-known crystalline form, exhibiting so strongly the metallic lustre, the form in which one always obtains it by volatilization in an atmosphere of hydrogen, it may be obtained by precipitation in two very different amorphous varieties. One of these is the product of the reduction of tellurous or telluric acids by sulphurous acid, and the other is formed when solutions of the alkaline tellurides are exposed to the oxidizing action of the air. Both these amorphous varieties are dark-coloured powders very liable to oxidation in the air, and only to be obtained pure by working in an atmosphere of nitrogen. The physical difference between the two is most strikingly shown, however, by their thermo-chemical behaviour. All three varieties are rapidly dissolved by a mixture of bromine and bromine water, and during the reaction in case of both the crystalline variety and the amorphous form obtained by oxidation of tellurides 33·4 heat units are evolved, while in case of amorphous tellurium derived by reduction with sulphurous acid only 21·3 units are disengaged. There was no mistake as to the purity of this latter kind, for it was completely converted to crystals on sublimation in a current of hydrogen. Hence it follows that one of the precipitated forms of tellurium corresponds to the crystalline state, and the other possesses an entirely different physical nature. It is curious, moreover, that all three modifications have the same specific heat. These interesting facts render the analogy between sulphur, selenium, and tellurium still more complete. MM. Berthelot and Fabre have also discovered a new and far superior method of preparing telluretted hydrogen. They first pass vapour of tellurium over metallic magnesium heated in a current of hydrogen, and afterwards treat the magnesium telluride thus formed with dilute hydrochloric acid in an apparatus previously filled with an atmosphere of nitrogen. The telluretted hydrogen, which is obtained in a very pure state by this new method, is very unstable, decomposing on standing in a tube over mercury even in the dark, coating the interior with tellurium and leaving its own volume of hydrogen. Decomposition is immediate in contact with moist air. In conclusion, the French chemists show that the combination of the elements of the sulphur group with hydrogen is attended with a beautifully graduated series of thermal changes, from water with heat of formation + 59 units down to telluretted hydrogen with − 35 units.

Science gives an interesting account of a magnificent ethnographical collection from Alaska, brought together by Lieut. Emmon. It has been presented to the American Museum of Natural History in New York, and forms a valuable supplement to the Powell collection from British Columbia, in the same Museum. While the latter includes principally specimens of Haida and Tsimpshian origin, the objects in the new collection come from the territory of the Tlingit, in whose country Lieut. Emmon spent more than five years. The collection includes a large number of masks. They are especially valuable, as Lieut. Emmon took great pains to ascertain the meaning of the masks, which thus became a rich source of information for the student of ethnology. A comparison of these masks with others collected on Vancouver Island and in Dean Inlet shows that the style of North-West American art, although uniform in general outlines, has its specific character in various localities. The imitation of animal forms is much closer here than in the southern regions, where the forms are more conventional, certain attributes of the animal alone being added to human figures. Another and a very interesting peculiarity of these masks is to be found in the figures of animals attached to the faces. The Eskimo tribes of Southern Alaska carve their masks in the same fashion, numerous attachments belonging to each. This is another proof of the influence of Indian art upon that of the Eskimo. The figures attached to the faces refer, as a rule, to certain myths; and the like is true of the Eskimo masks and their characteristic wings and figures. A considerable number of masks show deep hollow eyes and sunken cheeks. They represent the heads of dead men. Masks with thick lips and beards, and eyebrows made of otter skin, represent the fabulous Kushtaka, the otter people, of which many tales and traditions are told. Another remarkable mask is that of the mosquito. This is of special interest, as the mosquito is among the southern tribes the genius of the cannibal; and as cannibalistic ceremonies are not known to be practised by the Tlingit, it may be assumed that the myth referring to the mosquito is found in a somewhat altered form among the Tlingit.

We learn from *Science* that a Bill providing for the establishment of a zoological park in Washington has been introduced into the United States Senate. The Bill creates a Commission, which is directed to secure one hundred acres of land bordering on Rock Creek, about one mile from the city, to prepare the grounds and erect suitable buildings upon it. The park is then to be transferred to the regents of the Smithsonian Institution for their future custody and care. The site indicated is one of the most beautiful in the District of Columbia. It is composed of rolling ground, with the beautiful Rock Creek flowing through it, and it is adjacent to Woodley Park, one of the most charming of the recent additions to Washington. A street-railway to it is already projected.

The United States Bureau of Education has issued an elaborate report of the proceedings of the Department of Superintendence of the National Educational Association at its meeting at Washington from March 15 to 17, 1887. The volume includes addresses and papers by some of the most eminent American authorities on questions relating to education.

The people of Cleveland, where the American Association for the Advancement of Science will meet in August, have already begun to prepare for the meeting, which is expected to mark an epoch in the history of the city. At a recent meeting of citizens, summoned for the purpose of appointing various local committees, an interesting address on the history of the Association and its objects was delivered by Prof. F. W. Putnam, the Peabody Professor of American Archæology and Ethnology in the University of Harvard, and Permanent Secretary of the Association since 1873.

According to the *Colonies and India*, the Government of South Australia have issued Part 8 of a work on "The Forest Flora of South Australia," which is said to be the best illustrated publication ever issued in the colony. Mr. Brown, Conservator of Forests, under whose direction the book is brought out, supplies the letterpress descriptions of the plants pictorially represented.

The American publishers, Messrs. D. C. Heath and Co., have in the press a book of "Chemical Problems," by Drs. Grabfield and Burns, of the Massachusetts Institute of Technology.

A fourth edition of Prof. G. Henslow's "Botany for Beginners" (Stanford) has just been issued. In this little book Prof. Henslow provides a short course of elementary instruction in practical botany, for junior classes and children.

At the anniversary meeting of the Hertfordshire Natural History Society, held on February 21 last, Mr. F. Maule Campbell, the President, delivered an interesting address on the means of protection possessed by plants. This address is printed in the Transactions of the Society, and has now been issued separately.

The London Geological Field Class, under the direction of Prof. H. G. Seeley, begins the summer excursions on Whit Monday, May 21, and will continue them on Saturday afternoons thereafter up to July 14. The following are among some of the places which will be visited: Leatherhead and Boxhill, to examine the gorge of the Mole in chalk; Maidstone and the vicinity, for gravels; Woolwich and Reading beds, chalk gault, and lower greensand; Erith and Crayford, for river gravels; Grays (in Essex), Northfleet, and Oxsted, for studies in chalk; and other places besides. Intending students should apply for tickets at once, as only a limited number are issued. Application forms may be had from Mr. W. P. Collins, 157 Great Portland Street.

THE additions to the Zoological Society's Gardens during the past week include a Blue and Yellow Macaw (*Ara ararauna*), from South America, presented by Mrs. Warrand; two White Ibis (*Eudocimus albus*), from South America, deposited; two Black-backed Geese (*Sarcidiornis melanonota* ♂ ♀), from India, purchased; a Puma (*Felis concolor*), two Long-fronted Gerbilles (*Gerbillus longifrons*), a Hog Deer (*Cervus porcinus*), a Sambur Deer (*Cervus aristotelis* ♂), born in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *MAY* 20-26.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on May 20

Sun rises, 4h. 2m.; souths, 11h. 56m. 21·0s.; sets, 19h. 50m.: right asc. on meridian, 3h. 50·6m.; decl. 20° 8′ N. Sidereal Time at Sunset, 11h. 45m.

Moon (Full on May 25, 14h.) rises, 13h. 0m.; souths, 19h. 49m.; sets, 2h. 23m.*: right asc. on meridian, 11h. 44·2m.; decl. 6° 3′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	4 25 ...	12 43 ..	21 1 ...	4 37·3 ...	23 42 N.
Venus ...	3 35 ...	11 0 ..	18 25 ...	2 54·0 ...	15 26 N.
Mars ...	15 7 ...	20 49 ...	2 31*...	12 44·8 ...	4 12 S.
Jupiter ...	19 49*...	0 7 ...	4 25 ..	15 59·5 ...	19 33 S.
Saturn ...	8 27 ...	16 22 ...	0 17*...	8 16·7 ...	20 22 N.
Uranus...	15 16 ...	20 55 ...	2 34*...	12 50·8 ...	4 43 S.
Neptune..	4 13 ...	11 57 ...	19 41 ...	3 51·8 ...	18 31 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

May.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
20 ...	B.A.C. 3996 ...	6 ...	19 0	near approach	142 —
21 ...	*b* Virginis...	... 6 ...	0 57 ...	1 44 ...	139 254
24 ...	η Libræ ...	... 6 ...	22 52 ...	23 23 ...	128 184
25 ...	θ Libræ ...	... 4½ ...	3 45 ...	4 39† ...	85 322
26 ...	B.A.C. 5700 ...	6½ ...	4 14 ..	4 44 ...	175 236

† Below horizon at Greenwich.

May.	h.	
21 ...	2 ...	Jupiter in conjunction with and 0° 2′ north of β Scorpii.
21 ...	23 ...	Mars in conjunction with and 4° 32′ south of the Moon.
22 ...	5 ...	Jupiter in opposition to the Sun.
23 ...	11 ...	Mars stationary.
25 ...	7 ...	Jupiter in conjunction with and 3° 34′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m	
U Cephei ...	... 0 52·4 ...	81 16 N. ...	May 22,	1 17	*m*
S Persei ...	... 2 14·8 ...	58 4 N. ...	,, 22,		*m*
W Virginis ...	... 13 20·3 ...	2 48 S. ...	,, 26,	3 0	*m*
U Coronæ ...	... 15 13·6 ...	32 3 N. ...	,, 25,	2 55	*m*
U Ophiuchi...	... 17 10·9 ...	1 20 N. ...	,, 23,	1 22	*m*
S Sagittæ ...	... 19 50·9 ...	16 20 N. ...	,, 25,	3 0	*m*
R Sagittæ ...	... 20 9·0 ...	16 23 N. ...	,, 23,		*m*
T Vulpeculæ	... 20 46·7 ...	27 50 N. ...	,, 23,	2 0	*M*
δ Cephei ...	... 22 25·0 ...	57 51 N. ...	,, 25,	2 0	*m*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES.

AT Monday's meeting of the Royal Geographical Society Lieut. F. E. Younghusband gave an account of his journey across Central Asia, from Manchuria and Peking to Kashmir and the Mustagh Pass. This is the most important paper which has been read at the Society during the present session, and the journey one of the most remarkable ever made, considering its length, the time taken—April to November, 1887—and the novelty and value of the results. We have only space to refer briefly to Lieut. Younghusband's observations on the Mustagh Pass, which he has been the first European to cross. He crossed the Gobi Desert to Hami by a route lying between those of Marco Polo and Mr. Ney Elias. His observations in the Gobi are of much interest. The clearness and dryness of the atmosphere were remarkable. Everything became parched up, and so charged with electricity that a sheepskin coat or blanket, on being opened out, would give out a loud crackling noise, accompanied by a sheet of fire. At the western end of the Hurku Hills, beyond the Galpin Gobi—the most sterile part of the whole Gobi—is a most remarkable range of sand-hills. It is about 40 miles in length, and is composed of bare sand, without a vestige of vegetation of any sort on it, and in places it is as much as 900 feet in height, rising abruptly out of a gravel plain. With the dark outline of the southern hills as a background, this white, fantastically-shaped sand-range presents a very striking appearance. It must have been formed by the action of the wind, for to the westward of this range is an immense sandy tract, and it is evident that the wind has driven the sand from this up into the hollow between the Hurku Hills and the range to the south, thus forming these remarkable sand-hills. It was near this region that traces of the wild camel were met with, and both wild asses and wild horses seen. As far as Hami the country continues to be mainly desert. From Hami, Lieut. Younghusband went on to Yarkand, and by the Yarkand River to the Karakorum Range, which he meant to cross by the Mustagh Pass. The difficulties, owing to the enormous glaciers, the rugged nature of the mountains, and great height of the pass, were very great for Mr. Younghusband, his men, and his ponies. The glaciers here are of enormous size, and Mr. Younghusband has added considerably to the information obtained by Colonel Godwin-Austen, who surveyed the region to the south of the pass twenty-six years ago. "The appearance of these mountains," Lieut. Younghusband stated, "is extremely bold and rugged as they rise in a succession of needle peaks like hundreds of Matterhorns collected together; but the Matterhorn, Mont Blanc, and all the Swiss mountains would have been two or three thousand feet below me, while these mountains rose up in solemn grandeur thousands of feet above me. Not a living thing was seen, and not a sound was heard; all was snow and ice and rocky precipices; while these mountains are far too grand to support anything so insignificant as trees or vegetation of any sort. They stand bold and solitary in their glory, and only permit man to come amongst them for a few months in the year, that he may admire their magnificence and go and tell it to his comrades in the world beneath." After some extremely difficult prospecting, Lieut. Younghusband made up his mind to cross the old and long-abandoned Mustagh Pass, instead of the new one. "Next morning," he stated, "while it was yet dark, we started for the pass, leaving everything behind, except a roll of bedding for myself, a sheepskin coat for each man, a few dry provisions, and a large tea-kettle. The ascent to the pass was quite gentle, but led over deep snow in which we sank knee-deep at every step. We were now about 19,000 feet above the sea-level, and quickly became exhausted. In fact, as we got near the summit, we could only advance a dozen or twenty steps at a time, and we would then lean over on our alpenstocks, and gasp and pant away as if we had been running up a steep hill at a great pace. But it was not till midday that we reached the summit, and then on looking about for a way down we could see none. Huge blocks of ice had fallen from the mountains which overhang the pass, and had blocked up the path by which travellers used formerly to descend from it, and the only possible way now of getting to the bottom was by crossing an icy slope to a cliff, which was too steep for a particle of snow to lodge on it, even in that region of ice and snow. From this we should have to descend on to some more icy slopes which could be seen below. . . . We had first to cross the icy slope; it was of smooth ice and very steep, and about thirty yards below us it ended abruptly, and we could see nothing over the edge for many hundreds of feet. As Wali hewed the steps we advanced step by step after him, leaning back against the slope, all the time facing the precipice, and knowing that if we slipped (and the ice was very slippery, for the sun was just powerful enough to melt the surface of it), we should roll down the icy slope and over the precipice into eternity. After a time we reached *terra firma* in the shape of a projecting piece of rock, and from here began the descent of the cliff. We had to let ourselves down from any little ledge, taking every step with the greatest possible care, as the rock was not always sound; and once a shout came from above, and a huge rock, which had been dislodged, came crashing past me and as nearly as possible hit two of the men who had already got some way down. At the bottom of the cliff we came to another steep ice-slope." After eighteen hours of this task the party were glad to lie down for a few hours' rest. At daybreak next morning they were on their legs again, and after a few hours' travelling emerged on to the great Baltoro Glacier, which was explored by Colonel Godwin-Austen in 1862, when making the Kashmir survey. They travelled all that day, and for two days more, till they reached Askoli, a little village on the Braldo River, surrounded by trees and cultivated lands.

LIEUT. YOUNGHUSBAND remarked as follows on the Altai Mountains:—"These mountains are perfectly barren, the upper portion composed of bare rock and the lower of long gravel slopes formed of the debris of the rocks above. In such an extremely dry climate, exposed to the icy cold winds of winter and the fierce rays of the summer sun, and unprotected by one atom of soil, the rocks here, as also in every other part of the Gobi, crumble away to a remarkable extent, and there being no rainfall sufficient to wash away the debris, the lower features of the range gradually get covered with a mass of debris falling from the upper portions, and in the course of time a uniform slope is created, often 30 or 40 miles in length, and it is only for a few hundred feet at the top that the original jagged rocky outline is seen." Again, with regard to Chinese Turkistan:—"If you could get a bird's-eye view of Chinese Turkistan, you would see a great bare desert surrounded on three sides by barren mountains, and at their bases you would see some vivid green spots, showing out sharp and distinct like blots of green paint dropped on to a sepia picture. In the western end round Kashgar and Yarkand the cultivation is of greater extent and more continuous than in the eastern half, where the oases are small and separated from each other by 15 or 20 miles of desert. These oases are, however, extraordinarily fertile, every scrap of land that can be cultivated is used up, and every drop of water is drained off from the stream and used for irrigation." At the conclusion of Monday's meeting of the Royal Geographical Society, General J. T. Walker proposed, and Sir Henry Rawlinson seconded, that the peak in the Karakorum known as K2, 28,500 feet high, be baptized Mount Godwin-Austen—a proposal heartily approved by the meeting.

THE Paris Geographical Society has awarded gold medals to the Rev. P. Roblet, for his map of Madagascar; to MM. Bonvalot, Capus, and Pépin, for their journeys in Kafiristan and the Pamir; to M. Chaffanjon, for his exploration of the sources of the Orinoco.

GENERAL PRJEVALSKY will start in August next on his fourth journey in Central Asia. His ultimate destination will be Lhassa, the capital of Tibet, and he will be equipped for two years' travel. He will be accompanied by an escort of twenty-eight persons, including twelve Cossacks, and two scientific companions, Lieut. Robrowsky and Sub-Lieut. Koslow.

THE PYGMY RACES OF MEN.[1]

II.

LIKE all other human beings existing at present in the world, however low in the scale of civilization, the social life of the Andamanese is enveloped in a complex maze of unwritten law or custom, the intricacies of which are most difficult for any stranger to unravel. The relations they may or may not marry, the food they are obliged or forbidden to partake of at particular epochs of life or seasons of the year, the words and names they may or may not pronounce: all these, as well as their traditions, superstitions, and beliefs, their occupations, games, and amusements of which they seem to have had no lack, would take far too long to describe here; but, before leaving these interesting people, I may quote an observation of Mr. Man's, which, unless he has seen them with too *couleur-de-rose* eyesight, throws a very favourable light upon the primitive unsophisticated life of these poor little savages, now so ruthlessly broken into and destroyed by the exigencies of our ever-extending Empire.

"It has been asserted," Mr. Man says, "that the 'communal marriage' system prevails among them, and that 'marriage is nothing more than taking a female slave'; but, so far from the contract being regarded as a merely temporary arrangement, to be set aside at the will of either party, no incompatibility of temper or other cause is allowed to dissolve the union; and while bigamy, polygamy, polyandry, and divorce are unknown, conjugal fidelity till death is not the exception but the rule, and matrimonial differences, which, however, occur but rarely, are easily settled with or without the intervention of friends." In fact, Mr. Man goes on to say, "One of the most striking features of their social relations is the marked equality and affection which subsists between husband and wife," and "the consideration and respect with which women are treated might with advantage be emulated by certain classes in our own land."

It should also be mentioned that cannibalism and infanticide, two such common incidents of savage life, were never practised by them.

We must now pass to the important scientific question, Who are the natives of the Andaman Islands, and where, among the other races of the human species, shall we look for their nearest relations?

It is due mainly to the assiduous researches into all the documentary evidence relating to the inhabitants of Southern Asia and the Indian Archipelago, conducted through many years by M. de Quatrefages, in some cases with the assistance of his colleague M. Hamy, that the facts I am about to put before you have been prominently brought to light, and their significance demonstrated.

It is well known that the greater part of the large island of New Guinea, and of the chain of islands extending eastwards and southwards from it, including the Solomon Islands, the New Hebrides, and New Caledonia, and also the Fijis, are still inhabited mainly by people of dark colour, frizzly hair, and many characters allying them to the Negroes of Africa. These constitute the race to which the term Melanesian is commonly applied in this country, or Oceanic Negroes, the "Papouas" of Quatrefages. Their area at one time was more extensive than it is now, and has been greatly encroached upon by the brown, straight-haired Polynesian race with Malay affinities, now inhabiting many of the more important islands of the Pacific, and the mingling of which with the more aboriginal Melanesians in various proportions has been a cause, among others, of the diverse aspect of the population on many of the islands in this extensive region. These Papouas, or Melanesians, however, differ greatly from the Andamanese in many easily defined characters; which are, especially, their larger stature, their long, narrow, and high skulls, and their coarser and more Negro-like features. Although undoubtedly allied, we cannot look to them as the nearest relations of our little Andamanese.

When the Spaniards commenced the colonization of the Philippines, they met with, in the mountainous region in the interior of the Island of Luzon, besides the prevailing native population, consisting of Tagals of Malay origin, very small people, of black complexion, with the frizzly hair of the African Negroes. So struck were they with the resemblance, that they called them "Negritos del Monte" (little Negroes of the mountain). Their local name was Aigtas, or Inagtas, said to signify "black," and from which the word Aëta, generally now applied to them, is derived. These people have lately been studied by two French travellers, M. Marche and Dr. Montano; the result of their measurements gives 4 feet $8\frac{3}{4}$ inches as the average height of the men, and 4 feet $6\frac{1}{2}$ inches the average for the women. In many of their moral characteristics they resemble the Andamanese. The Aëtas are faithful to their marriage vows, and have but one wife. The affection of parents for children is very strong, and the latter have for their father and mother as much love and respect. The marriage ceremony, according to M. Montano, is very remarkable. The affianced pair climb two flexible trees placed near to each other. One of the elders of the tribe bends them towards each other. When

[1] A Lecture delivered at the Royal Institution on Friday evening, April 13, 1888, by Prof. Flower, C.B., LL.D., F.R.S., Director of the Natural History Departments of the British Museum. Continued from p. 45.

their heads touch, the marriage is legally accomplished. A great *fête*, with much dancing, concludes the ceremony.

It was afterwards found that the same race existed in other parts of the archipelago, Panay, Mindanao, &c., and that they entirely peopled some little islands—among others, Bougas Island, or "Isla de los Negros."

As the islands of these eastern seas have become better known, further discoveries of the existence of a small Negroid population have been made in Formosa, in the interior of Borneo, the Sandal Islands (Sumba), Xulla, Bourou, Ceram, Flores, Solor, Pantar, Lomblem, Ombay, the eastern peninsula of Celebes, &c. In fact, Sumatra and Java are the only large islands of this great area which contain no traces of them except some doubtful cross-breeds, and some remains of an industry which appears not to have passed beyond the Age of Stone.

The Sunda Islands form the southern limit of the Negrito area; Formosa, the last to the north, where the race has preserved all its characters. But beyond this, as in Lew-Chew, and even the south-east portion of Japan, it reveals its former existence by the traces it has left in the present population. That it has contributed considerably to form the population of New Guinea is unquestionable. In many parts of that great island, small round-headed tribes live more or less distinct from the larger and longer-headed people who make up the bulk of the population.

But it is not only in the islands that the Negrito race dwelt. Traces of them are found also on the mainland of Asia, but everywhere under the same conditions: in scattered tribes, occupying the more inaccessible mountainous regions of countries otherwise mainly inhabited by other races, and generally in a condition more or less of degradation and barbarism, resulting from the oppression with which they have been treated by their invading conquerers; often, moreover, so much mixed that their original characters are scarcely recognizable. The Semangs of the interior of Malacca in the Malay peninsula, the Sakays from Perak, the Moys of Annam, all show traces of Negrito blood. In India proper, especially among the lowest and least civilized tribes, not only of the central and southern districts, but even almost to the foot of the Himalayas, in the Punjab, and even to the west side of the Indus, according to Quatrefages, frizzly hair, Negro features, and small stature, are so common that a strong argument can be based on them for the belief in a Negrito race forming the basis of the whole pre-Aryan, or Dravidian as it is generally called, population of the peninsula. The crossing that has taken place with other races has doubtless greatly altered the physical characters of this people, and the evidences of this alteration manifest themselves in many ways; sometimes the curliness of the hair is lost by the admixture with smooth straight-haired races, while the black complexion and small stature remain; sometimes the stature is increased, but the colour which seems to be one of the most persistent of characteristics, remains.

The localities in which these people are found in their greatest purity, either in almost inaccessible islands, as on the Andamans, or elsewhere in the mountainous ranges of the interior only; their social positions and traditions, wherever they exist—all point to the fact that they were the earliest inhabitants; and that the Mongolian and Malay races on the east, and the Aryans on the west, which are now so rapidly exterminating and replacing them, are later comers into the land, exactly as, in the greater part of the Pacific Ocean, territory formerly occupied by the aboriginal dark, frizzly-haired Negroid Melanesians has been gradually and slowly invaded by the brown Polynesians, who in their turn, but by a much more rapid process, are being replaced by Europeans.

We now see what constitutes the great interest of the Andamanese natives to the student of the ethnological history of the Eastern world. Their long isolation has made them a remarkably homogeneous race, stamping them all with a common resemblance not seen in the mixed races generally met with in continental areas. For although, as with most savages, marriages within the family (using the term in a very wide sense) are most strictly forbidden, all such alliances have necessarily been confined to natives of the islands. They are the least modified representatives of the people who were, as far as we know, the primitive inhabitants of a large portion of the earth's surface, but who are now verging on extinction. It is, however, not necessary to suppose that the Andaman Islanders give us the exact characters and features of all the other branches of the race. Differences in detail doubtless existed—differences which are almost always sure to arise whenever races become isolated from each other for long periods of time.

In many cases the characters of the ancient inhabitants of a land have been revealed to us by the preservation of their actual remains. Unfortunately we have as yet no such evidence to tell us of the former condition of man in Southern Asia. We may, however, look upon the Andamanese, the Aëtas, and the Semangs, as living fossils; and by their aid conjecture the condition of the whole population of the land in ancient times. It is possible, also, to follow Quatrefages, and to see in them the origin of the stories of the Oriental pygmies related by Ctesias and by Pliny.

We now pass to the continent of Africa, in the interior of which the pygmies of Homer, Herodotus, and Aristotle have generally been placed. Africa, as is well known, is the home of another great branch of the black, frizzly-haired, or Ethiopian division of the human species, who do, or did till lately, occupy the southern two-thirds of this great continent, the northern third being inhabited by Hamite and Semite branches of the great white or Caucasian primary division of the human species, or by races resulting from the mixture of them and the Negroes. Besides the true Negro, there has long been known to exist in the southern part of the continent a curiously modified type, consisting of the Hottentots, and the Bushmen—Bosjesmen (men of the woods) of the Dutch colonists—the latter of whom, on account of their small size, come within the scope of the present subject. They lead the lives of the most degraded of savages, dwelling among the rocky and more inaccessible mountains of the interior, making habitations of the natural caves, subsisting entirely by the chase, being most expert in the use of the bow and arrow, and treated as enemies and outcasts by the surrounding and more civilized tribes, whose flocks and herds they show little respect for when other game is not within reach. The physical characters of these people are well known, as many specimens have been brought to Europe alive for the purpose of exhibition. Their hair shows the extreme of the frizzly type, being shorter and less abundant than that of the ordinary Negro; it has the appearance of growing in separate tufts, which coil up together into round balls compared to "peppercorns." The yellow complexion differs from that of the Negro, and, combined with the wide cheek-bones and form of the eyes, so much recalls that of certain of the pure yellow races that some anthropologists are inclined to trace true Mongolian affinities and admixture, although the extreme crispness of the hair makes such a supposition almost impossible. The width of the cheek-bones and the narrowness of the forehead and the chin give a lozenge shape to the front view of the face. The forehead is prominent and straight; the nose extremely flat and broad, more so than in any other race, and the lips prominent and thick, although the jaws are less prognathous than in the true Negro races. The cranium has many special characters by which it can be easily distinguished from that of any other. It has generally a very feminine, almost infantile, appearance, though the capacity of the cranial cavity is not the smallest, exceeding that of the Andamanese. In general form the cranium is rather oblong than oval, having straight sides, a flat top, and especially a vertical forehead, which rises straight from the root of the nose. It is moderately dolichocephalic or rather mesaticephalic, the average of the index of ten specimens being 75·4. The height is in all considerably less than the breadth, the average index being 71·1. The glabella and infra-orbital ridges are little developed except in the oldest males. The malar bones project much forwards, and the space between the orbits is very wide and flat. The nasal bones are extremely small and depressed, and the aperture wide; the average nasal index being 60·8, so they are the most platyrhine of races.

With regard to the stature, we have not yet sufficient materials for giving a reliable average. Quatrefages, following Barrow, gives 4 feet 6 inches for the men, and 4 feet for the women, and speaks of one individual of the latter sex, who was the mother of several children, measuring only 3 feet 9 inches in height; but later observations (still, however, insufficient in number) give a rather larger stature: thus Topinard places the average at 1·404 metre, or 4 feet 7½ inches; and Fritsch, who measured six male Bushmen in South Africa, found their mean height to be 1·444 metre, or nearly 4 feet 9 inches. It is probable that, taking them all together, they differ but little in this respect from the Andamanese, although in colour, in form of head, in features, and in the proportions of the body, they are widely removed from them.

There is every reason to believe that these Bushmen represent the earliest race of which we have, or are ever likely to have, any knowledge, which inhabited the southern portion of the African continent, but that long before the advent of Europeans upon the scene, they had been invaded from the north by Negro tribes, who, being superior in size, strength, and civilization, had taken possession of the greater part of their territories, and mingling freely with the aborigines, had produced the mixed race called Hottentots, who retained the culture and settled pastoral habits of the Negroes, with many of the physical features of the Bushmen. These, in their turn, encroached upon by the pure-bred Bantu Negroes from the north, and by the Dutch and English from the south, are now greatly diminished, and indeed threatened with the same fate that will surely soon befall the scanty remnant of the early inhabitants who still retain their primitive type.

At present the habitat of the Bushman race is confined to certain districts in the south-west of Africa, from the confines of the Cape Colony, as far north as the shores of Lake Ngami. Further to the north the great equatorial region of Africa is occupied by various Negro tribes, using the term in its broadest sense, but belonging to the divisions which, on account of peculiarities of language, have been grouped together as Bantu. They all present the common physical characteristics typical of the Negro race, only two of which need be specially mentioned here—medium or large stature, and dolichocephalic skull (average cranial index about 73·5).

It is at various scattered places in the midst of these, that the only other small people of which I shall have to speak, the veritable pygmies of Homer, Herodotus, and Aristotle, according to Quatrefages, are still to be met with.[1]

The first notice of the occurrence of these in modern times is contained in "The strange adventures of Andrew Battell of Leigh in Essex, sent by the Portugals prisoner to Angola, who lived there, and in the adjoining regions near eighteen yeares" (1589 to 1607), published in "Purchas his Pilgrimes" (1625), lib. vii. chap. iii. p. 983:—

"To the north-east of *Mani-Kesock*, are a kind of little people, called *Matimbas;* which are no bigger than Boyes of twelve yeares old, but very thicke, and live only upon flesh, which they kill in the woods with their bows and darts. They pay tribute to *Mani-Kesock*, and bring all their elephants' teeth and tayles to him. They will not enter into any of the *Maramba's* houses, nor will suffer any to come where they dwell. And if by chance any *Maramba* or people of *Longo* pass where they dwell, they will forsake that place, and go to another. The women carry Bows and Arrows as well as the men. And one of these will walk in the woods alone and kill the Pongos with their poysoned Arrows."

Battell's narrative, it should be said, is generally admitted as having an air of veracity about it not always conspicuous in those of travellers of his time. In addition to the observations on the human inhabitants, it contains excellent descriptions of animals, as the pongo or gorilla, and the zebra, now well known, but in his day new to Europeans.

Dapper, in a work called "Description de la Basse Ethiopie," published at Amsterdam in 1686, speaks of a race of dwarfs inhabiting the same region, which he calls *Mimos* or *Bakke-Bakke*, but nothing further was heard of these people until quite recent times. A German scientific expedition to Loango, the results of which were published in the *Zeitschrift für Ethnologie*, 1874, and in Hartmann's work, "Die Negritier," obtained, at Chinchoxo, photographs and descriptions of a dwarf tribe called "Baboukos," whose heads were proportionally large and of roundish form (cephalic index of skull, 78 to 81). One individual, supposed to be about forty years of age, measured 1·365 metre, rather under 4 feet 6 inches.

Dr. Touchard, in a "Notice sur le Gabon," published in the *Revue Maritime et Coloniale* for 1861, describes the recent destruction of a population established in the interior of this country and to which he gives the name of "Akoa." They seem to have been exterminated by the M'Pongos in their expansion towards the west. Some of them, however, remained as slaves at the time of the visit of Admiral Fleuriot de Langle, who in 1868 photographed one (measuring about 4 feet 6 inches high) and brought home some skulls, which were examined by Hamy, and all proved very small and sub-brachycephalic.

[1] The scattered information upon this subject was first collected together by Hamy in his "Essai de co-ordination des Matériaux récemment recueillis sur l'ethnologie des Négrilles ou Pygmées de l'Afrique équatoriale," *Bull. Soc. d'Anthropologie de Paris*, tome ii. (ser. iii.), 1879, p. 79.

Another tribe, the M'Boulous, inhabiting the coast north of the Gaboon River, have been described by M. Marche as probably the primitive race of the country. They live in little villages, keeping entirely to themselves, though surrounded by the larger Negro tribes, M'Pongos and Bakalais, who are encroaching upon them so closely that their numbers are rapidly diminishing. In 1860 they were not more than 3000; in 1879 much less numerous. They are of an earthy-brown colour, and rarely exceed 1·600 metre in height (5 feet 3 inches). In the rich collections of skulls made by Mr. R. B. Walker and by M. Du Chaillu, from the coast of this region, are many which are remarkable for their small size and round form. Of many other notices of tribes of Negroes of diminutive size, living near the west coast of Equatorial Africa, I need only mention that of Du Chaillu, who gives an interesting account of his visit to an Obongo village in Ashango-land, between the Gaboon and the Congo; although unfortunately, owing to the extreme shyness and suspicion of the inhabitants, he was allowed little opportunity for anthropological observations. He succeeded, however, in measuring one man and six women; the height of the former was 4 feet 6 inches, the average of the later 4 feet 8 inches.[1]

Far further into the interior, towards the centre of the region contained in the great bend of the Congo or Livingstone River, Stanley heard of a numerous and independent population of dwarfs, called "Watwas," who, like the Batimbas of Battell, are great hunters of elephants, and use poisoned arrows. One of these he met with at Ikondu was 4 feet 6½ inches high, and of a chocolate-brown colour.[2] More recently Dr. Wolff describes under the name of "Batouas" (perhaps the same as Stanley's Watwas), a people of lighter colour than other Negroes, and never exceeding 1·40 metres (4 feet 7 inches) high, but whose average is not more than 1·30 (4 feet 3 inches), who occupy isolated villages scattered through the territory of the Bahoubas, with whom they never mix.[3]

Penetrating into the heart of Africa from the north-east, in 1870, Dr. George Schweinfurth first made us acquainted with a diminutive race of people who have since attained a considerable anthropological notoriety. They seem to go by two names in their own country, *Akka* and *Tikki-tikki*, the latter reminding us curiously of Dapper's Bakke-bakke, and the former, more singularly still, having been read by the learned Egyptologist, Mariette, by the side of the figure of a dwarf in one of the monuments of the early Egyptian Empire.

It was at the court of Mounza, king of the Monbuttu, that Schweinfurth first met with the Akkas. They appear to live under the protection of that monarch, who had a regiment of them attached to his service, but their real country was further to the south and west, about 3° N. lat. and 25° E. long. From the accounts the traveller received they occupy a considerable territory, and are divided into nine distinct tribes, each having its own king or chief. Like all the other pygmy African tribes, they live chiefly by the chase, being great hunters of the elephant, which they attack with bows and arrows.

In exchange for one of his dogs, Schweinfurth obtained from Mounza one of these little men, whom he intended to bring to Europe, but who died on the homeward journey at Berber. Unfortunately all the measurements and observations which were made in the Monbuttu country by Schweinfurth perished in the fire which destroyed so much of the valuable material he had collected. His descriptions of their physical characters are therefore chiefly recollections. Other travellers—Long, Marno, and Vossion—though not penetrating as far as the Akka country, have given observations upon individuals of the race they have met with in their travels. The Italian Miani, following the footsteps of Schweinfurth into the Monbuttu country, also obtained, by barter, two Akka boys, with the view of bringing them to Europe. He himself fell a victim to the fatigues of the journey and climate, but left his collections, including the young Akkas, to the Italian Geographical Society. Probably no two individuals of a savage race have been so much honoured by the attentions of the scientific world. First at Cairo, and afterwards in Italy, Tebo (or Thibaut) and Chairallah, as they were named, were described, measured, and photographed, and have been the subjects of a library of memoirs, their bibliographers including the names of Owen, Panceri, Cornalia, Mantegazza, Giglioli and Zannetti, Broca, Hamy, and de Quatrefages. On their arrival in Italy they were presented to the King and Queen, introduced into the

[1] "A Journey to Ashango-land," 1867, p. 3[illegible]5.
[2] "Through the Dark Continent," vol. ii.
[3] *La Gazette Géographique*, 1887, p. 153, quoted by Quatrefages.

nost fashionable society, and finally settled down as members of the household of Count Miniscalchi Erizzo, at Verona, where hey received a European education, and performed the duties of pages.

In reply to an inquiry addressed to my friend Dr. Giglioli, of Florence, I hear that Thibaut died of consumption on January 28, 1883, being then about twenty-two years of age, and was buried in the cemetery at Verona. Unfortunately no scientific examination of the body was allowed, but whether Chairallah still lives or not I have not been able to learn. As Giglioli has not heard of his death, he presumes that he is still living in Count Miniscalchi's palace.

One other specimen of this race has been the subject of careful observation by European anthropologists—a girl named Saida, brought home by Romolo Gessi (Gordon's lieutenant), and who is still, or was lately, living at Trieste as servant to M. de Gessi.

The various scattered observations hitherto made are obviously insufficient to deduce a mean height for the race, but he nearest estimate that Quatrefages could obtain is about 4 feet 7 inches for the men, and 4 feet 3 inches for the women, decidedly inferior, therefore, to the Andamanese. With egard to their other characters, their hair is of the most frizzly ind, their complexion lighter than that of most Negroes, but he prognathism, width of nose, and eversion of lips characteristic f the Ethiopian branch of the human family are carried to an xtreme degree, especially if Schweinfurth's sketches can be rusted. The only essential point of difference from the ordinary Negro, except the size, is the tendency to shortening and breadth f the skull, although it by no means assumes the "almost pherical" shape attributed to it by Schweinfurth.

Some further information about the Akkas will be found in ie work, just published, of the intrepid and accomplished raveller in whose welfare we are now so much interested, Dr. Emin Pasha, Gordon's last surviving officer in the Soudan, who in the course of his explorations spent some little time lately n the country of the Monbuttu. Here he not only met with ving Akkas, one of whom he apparently still retains as a omestic in his service, and of whose dimensions he has sent me most detailed account, but he also, by watching the spots where wo of them had been interred, succeeded in obtaining their keletons, which, with numerous other objects of great scientific nterest, safely arrived at the British Museum in September of ast year. I need hardly say that actual bones, clean, imperishble, easy to be measured and compared, not once only, but ny number of times, furnish the most acceptable evidence that n anthropologist can possess of many of the most important hysical characters of a race. There we have facts which can ways be appealed to in support of statements and inferences ased on them. Height, proportions of limbs, form of head, haracters of the face even, are all more rigorously determined om the bones than they can be on the living person. Therefore ie value of these remains, imperfect as they unfortunately are, nd of course insufficient in number for the purpose of establishing verage characters, is very great indeed.

As I have entered fully into the question of their peculiarities sewhere, I can only give now a few of the most important nd most generally to be understood results of their examination. he first point of interest is their size. The two skeletons are oth those of full-grown people, one a man, the other a woman. here is no reason to suppose that they were specially selected exceptionally small; they were clearly the only ones which min had an opportunity of procuring; yet they fully bear out, ore than bear out, all that has been said of the diminutive e of the race. Comparing the dimensions of the bones, one one, with those of the numerous Andamanese that have passed rough my hands, I find both of these Akkas smaller, not than e average, but smaller than the smallest; smaller also than y Bushman whose skeleton I am acquainted with, or whose mensions have been published with scientific accuracy. In t, they are both, for they are nearly of a size, the smallest rmal human skeletons which I have seen, or of which I can d any record. I say normal, because they are thoroughly ll grown and proportioned, without a trace of the deformity nost always associated with individual dwarfishness in a taller e. One only, that of the female, is sufficiently perfect for iculation. After due allowance for some missing vertebræ, and the intervertebral spaces, the skeleton measures from the wn of the head to the ground exactly 4 feet, or 1·218 metre. out half an inch more for the thickness of the skin of the head and soles of the feet would complete the height when alive. The other (male) skeleton was (judging by the length of the femur) about a quarter of an inch shorter.

The full-grown woman of whom Emin gives detailed dimensions is stated to be only 1·164 metre, or barely 3 feet 10 inches.[1] These heights are all unquestionably less than anything that has been yet obtained based upon such indisputable data. One very interesting and almost unexpected result of a careful examination of these skeletons is that they conform in the relative proportions of the head, trunk, and limbs, not to dwarfs, but to full-sized people of other races, and they are therefore strikingly unlike the stumpy, long-bodied, short-limbed, large-headed pygmies so graphically represented fighting with their lances against the cranes on ancient Greek vases.

The other characters of these skeletons are Negroid to an intense degree, and quite accord with what has been stated of their external appearance. The form of the skull, too, has that sub-brachycephaly which has been shown by Hamy to characterize all the small Negro populations of Central Africa. It is quite unlike that of the Andamanese, quite unlike that of the Bushmen. They are obviously Negroes of a special type, to which Hamy has given the appropriate term of *Negrillo*. They seem to have much the same relation to the larger longer-headed African Negroes that the small round-headed Negritos of the Indian Ocean have to their larger longer-headed Melanesian neighbours.

At all events, the fact now seems clearly demonstrated that at various spots across the great African continent, within a few degrees north and south of the equator, extending from the Atlantic coast to near the shores of the Albert Nyanza (30° E. long.), and perhaps, if some indications which time will not allow me to enter into now (but which will be found in the writings of Hamy and Quatrefages), even further to the east, south of the Galla land, are still surviving, in scattered districts, communities of these small Negroes, all much resembling each other in size, appearance, and habits, and dwelling mostly apart from their larger neighbours, by whom they are everywhere surrounded. Our information about them is still very scanty, and to obtain more would be a worthy object of ambition for the anthropological traveller. In many parts, especially at the west, they are obviously holding their own with difficulty, if not actually disappearing, and there is much about their condition of civilization, and the situations in which they are found, to induce us to look upon them, as in the case of the Bushmen in the south and the Negritos in the east, as remains of a population which occupied the land before the incoming of the present dominant races. If the account of the Nasamonians related by Herodotus is accepted as historical, the river they came to, "flowing from west to east," must have been the Niger, and the northward range of the dwarfish people far more extensive twenty-three centuries ago than it is at the present time.

This view opens a still larger question, and takes us back to the neighbourhood of the south of India as the centre from which the whole of the great Negro race spread, east over the African continent, and west over the islands of the Pacific, and to our little Andamanese fellow subjects as probably the least modified descendants of the primitive members of the great branch of the human species characterized by their black skins and frizzly hair.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—In a recent discussion on the proposed appropriation of the whole of the Botanic Gardens site for Natural Science Departments, it seemed to be generally agreed that the Mechanical Department ought to be removed from a locality where it must cause vibrations injurious to microscopical or physical research. The suggested removal of the Herbarium to the Botanic Gardens was disapproved of by the Professor and his Assistant-Curator. The proposed appropriation of the present Chemical Rooms for Pathology was generally approved. Mr. J. W. Clark emphatically condemned the present Museum of Human Anatomy and Surgery as a discredit to the University. Prof. Hughes further put in a claim that the Geological Museum should extend to the extreme east of the site, and that the erection of the buildings should be begun at once.

[1] In his letters Emin speaks of an Akka man as "3 feet 6 inches" high, though this does not profess to be a scientifically accurate observation, as does the above. He says of this man that his whole body was covered by thick, stiff hair, almost like felt, as was the case with all the Akkas he had yet examined.

The first Harkness Scholarship for Geology and Palæontology is to be awarded in June next; names of candidates are to be sent in by May 31 next. Candidates must be Bachelors of Arts of not more than two-and-a-half years' standing.

The Sheepshanks Astronomical Exhibition will be awarded next December, at Trinity College. It is open to all undergraduates of the University, but the person elected must become a member of Trinity College. The conditions may be learnt from Dr. Glaisher, Trinity College.

SOCIETIES AND ACADEMIES.

London.

Royal Society, April 26.—"On the Development of the Electric Organ of *Raia batis*." By J. C. Ewart, M.D., Regius Professor of Natural History, University of Edinburgh. Communicated by J. Burdon Sanderson, F.R.S.

The paper consists of a short description of the electric organs found in the skate genus, and of an account of the development of the electric organ of the common grey skate (*Raia batis*).

It is shown that while in some skates (*e.g.*, *Raia batis*) the organ is made up of disk-shaped bodies, in others (*e.g.*, *Raia fullonica*) it consists of numerous cup-shaped structures provided with long or short stems.

The disks (with the development of which the paper chiefly deals) consist essentially of three layers, viz. (1) an electric plate in front in which the nerves end; (2) a striated layer which supports the electric plate; and (3) an alveolar layer, posterior to which is a thick cushion of gelatinous tissue. Each disk is formed in connection with a muscular fibre. In young embryos there is no indication of an electric organ, but in an embryo 6 or 7 cm. in length, some of the muscular fibres at each side of the notochord are found in process of conversion into long slender clubs having their heads nearest the root of the tail.

The club-stage having been reached, the muscular fibre next assumes the form of a mace, and, later, the anterior end further expands to form a relatively large disk, while the remainder of the original fibre persists as a slender ribbon-shaped appendage. As the head of the club enlarges to form a disk, it passes through an indistinct cup-stage, which somewhat resembles the cups of the adult *Raia fullonica*, hence it may be inferred that in *Raia fullonica* the organ has been arrested in its development. The conversion of the muscular fibre into a club is largely caused by the increase, at its anterior end, of muscle-corpuscles. These corpuscles eventually arrange themselves, either in front of the head of the club, to give rise to the electric plate, or they migrate backwards to form at the junction of the head of the club with its stem the alveolar layer. The striated layer, which is from the first devoid of nuclei, seems to be derived from the anterior striated portion of the club.

The gelatinous tissue between the disks, and the connective tissue investing them, are derived from the embryonic connective tissue corpuscles, which exist in great numbers around the clubs and developing disks.

May 3.—"On the Relations of the Diurnal Barometric Maxima to certain Critical Conditions of Temperature, Cloud, and Rainfall." By Henry F. Blanford, F.R.S.

The author refers to an observation of Lamont's that the diurnal barometric variation appears to be compounded of two distinct elements, viz. a wave of diurnal period, which is very variable in different places, and which appears to depend on the horizontal and vertical movements of the atmosphere and changes in the distribution of its mass, and a semi-diurnal element which is remarkably constant and seems to depend more immediately on the action of the sun. Then, referring to the theory of the semi-diurnal variation, originally put forward by Espy, and subsequently by Davies and Kreil, the author points out that the morning maximum of pressure approximately coincides with the instant when the temperature is rising most rapidly. This is almost exactly true at Prague, Yarkand, both in winter and summer, and in winter months at Melbourne. At the tropical stations, Bombay, Calcutta, and Batavia, and at Melbourne in the summer, the barometric maximum follows the instant of most rapid heating by a shorter or longer interval; and the author remarks that this may probably be attributed to the action of convection, which must accelerate the time of most rapid heating near the ground surface; while the barometric effect, if real, must be determined by the condition of the atmosphere up to a great height. With reference to Lamont's demonstration of the failure of Espy's theory, a condition is pointed out which alters the data of the problem, viz. the resistance that must be offered to the passage of the pressure-wave through the extremely cold and highly attenuated atmospheric strata, whose existence is proved by the phenomena of luminous meteors.

With respect to the evening maximum of pressure, it is pointed out that very generally, and especially in India, and also at Melbourne, there is a strongly-marked minimum in the diurnal variation of cloud between sunset and midnight, which, on an average, as at Allahabad and Melbourne, coincides with the evening maximum of the barometer. A similar coincident minimum, even more strongly marked, characterizes the diurnal variation of the rainfall at Calcutta and Batavia in their respective rainy seasons. In the author's opinion these facts seem to point to a compression and dynamic heating of the cloud-forming strata, and he points to the existence of a small irregularity in the diurnal temperature curves of Prague, Calcutta, and Batavia, which may possibly be due to such action. It is further remarked that the evening maximum about coincides with the time when the evening fall of temperature, after a rapid reduction between 6 or 7 and 10 p.m., becomes nearly uniform in rate, and it is suggested that the former may possibly be determined by the check of the rate of collapse of the cooling atmosphere. But it is observed that both the morning and evening waves of pressure probably involve other elements than the forced waves, and are in part rhythmic repetitions of previous waves.

Geological Society, April 25.—W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—Report on the recent work of the Geological Survey in the North-West Highlands of Scotland, based on the field-notes and maps of Messrs. Peach, Horne, Gunn, Clough, Hinxman, and Cadell. Communicated by Dr. A. Geikie. At the outset a review was given of the researches of other observers, in so far as they forestalled the conclusions to which the Geological Survey had been led. Reference was made to the observations of Macculloch, Hay Cunningham, C. W. Peach, and Salter; to the prolonged controversy between Sir Roderick Murchison and Prof. Nicol; to the contributions of Hicks, Bonney, Hudleston, Callaway, Lapworth, Teall, and others. It was shown that Nicol was undoubtedly right in maintaining that there was no conformable sequence from the fossiliferous quartzites and limestones into the eastern schists. It was also pointed out that the conclusions of Prof. Lapworth regarding the nature and origin of the eastern schists involve an important departure from Nicol's position, and are practically identical with those obtained independently by the Geological Survey. The results of the recent survey work among the Archæan rocks may be thus summarized: (1) the eruption of a series of igneous rocks of a basic type in which pegmatites were formed; (2) the development of rude foliation in these masses, probably by mechanical movement, and their arrangement in gentle anticlines and synclines, the axes of which generally run N.E. and S.W.; (3) the injection of igneous materials, mainly in the form of dykes, into the original gneisses, composed of (*a*) basalt rocks, (*b*) peridotites and palæopicrites, (*c*) microcline-mica rocks, (*d*) granites; (4) the occurrence of mechanical movements giving rise to disruption-lines trending N.W. and S.E., E. and W., N.E. and S.W.; (5) the effects of these movements on the dykes were to change the basalt-rocks into diorites and hornblende-schists, the peridotites and palæopicrites into talcose schists, the microcline-mica rocks into mica schists, and the granites into granitoid gneiss; (6) the effects on the gneiss resulted in the formation of sharp folds trending generally N.W. and S.E., the partial or complete reconstruction of the original gneiss along the old foliation-planes, and finally the development of newer schistosity more or less parallel with the prominent disruption-lines. There is an overwhelming amount of evidence to prove that all these various changes had been superinduced in the Archæan rocks in pre-Cambrian time. After reviewing the facts bearing on the denudation of the Archæan land-surface, the order of succession and thickness of the Cambrian strata were given, from which it is apparent that the deposits gradually increase in thickness as we pass southwards from Durness to Loch Broom. Prior to the deposition of the Silurian sediments the Cambrian strata were folded and extensively denuded. By these means various Cambrian outliers were formed far to the east of the present limits of the formation. The order of succession of the Silurian strata along the line of complicated structure from

Eriboll to Ullapool was described, reference being made to the further subdivision of the "Pipe-rock" and the Ghrudaidh Limestones (Group I. of Durness section). None of the richly fossiliferous zones of Durness is met with along this line, as they occupy higher horizons. An examination of the fossils recently obtained by the Geological Survey from the Durness Limestones confirms Salter's conclusions that they are distinctly of an American type, the Sutherland quartzites and limestones being represented by the Potsdam Sandstones and Calciferous Sand Group of North America. After the deposition of the limestones, the Cambrian and Silurian strata were pierced by igneous rocks, mainly in the form of sheets, producing important alterations in the sedimentary deposits by contact-metamorphism, the quartzites becoming crystalline, and the limestones being converted into marble. When this outburst of volcanic activity had ceased, terrestrial displacements ensued on a stupendous scale. By means of powerful thrusts the Silurian strata were piled on each other, and huge slices of the old Archæan platform, with the Cambrian and Silurian strata resting on it, were driven westwards for miles. With the view of illustrating the extraordinary complications produced by these movements, a series of horizontal sections was described, drawn across the line between Eriboll and Ullapool. The evidence relating to regional metamorphism was next referred to, from which it is obvious that with each successive maximum thrust there is a progressive amount of alteration in the displaced masses, as the observer passes eastwards to the higher thrust-planes. Eventually the Archæan gneiss is so deformed that the pre-Cambrian foliation disappears and is replaced by new divisional planes; the Cambrian grits and shales are converted into schists; the Silurian quartzites into quartz-schists; the limestones become crystalline; the sheets of intrusive felsite, diorite, and granitoid rock pass into sericite schist, hornblende-schist, and augen-gneiss respectively. These researches furnish a vast amount of evidence in support of the theory that regional metamorphism is due to the dynamical and chemical effects of mechanical movement acting on crystalline and clastic rocks. It is also clear that regional metamorphism need not be confined to any particular geological period, because in the N.W. Highlands, both in pre Cambrian time and after the deposition of the Durness Limestone (Lower Silurian), crystalline schists and gneiss were produced on a magnificent scale. After the reading of this Report, the Survey was congratulated on its work by the President, Prof. Lapworth, Prof. Judd, and other speakers.—On the horizontal movements of rocks, and the relation of these movements to the formation of dykes and faults, and to denudation and the thickening of strata, by Mr. William Barlow.—Notes on a recent discovery of *Stigmaria ficoides* at Clayton, Yorkshire, by Mr. Samuel A. Adamson.

Zoological Society, April 30.—Fifty-ninth Anniversary Meeting.—Prof. Flower, F.R.S., President, in the chair.—After the Auditors' Report had been read, and some other preliminary business had been transacted, the Report of the Council on the proceedings of the Society during the year 1887 was read by Mr. P. L. Sclater, F.R.S., the Secretary of the Society. It stated that the number of Fellows on January 1, 1888, was 3104, showing a decrease of 42 as compared with the corresponding period in 1887. A large number of valuable communications received at the usual scientific meetings held during the session of 1887 had been published in the annual volume of Proceedings, which contained 730 pages, illustrated by 55 plates. Besides this, one part of the twelfth volume, viz. Part C, of the Society's quarto Transactions, illustrated by seven plates, had been issued, and several other parts of Transactions were in a forward state. The volume of the *Zoological Record* for 1886 had been sent out in the month of January of this year to about 140 subscribers. The new edition of the Library Catalogue, spoken of in the last Annual Report as ready for issue had been published last summer. Two important additions had been made to the buildings in the Society's Gardens during the past year. The first of these, the wolves' and foxes' dens, which were commenced in 1886, had been erected by the Society's staff, under the supervision of Mr. Trollope, by whom the plans were drawn, and completed in November last. The second addition was a new aviary for flying birds which had been erected on the water-fowls' lawn, opposite the eastern aviary. This aviary is 105 feet long, 62 feet broad, and 27 feet high in the centre of the roof, which is formed of galvanized wire. The visitors to the Society's Gardens during the year 1887 had been altogether 562,898; the corresponding number in 1886 was 639,674. Mr. F. E. Beddard, Prosector to the Society, had been appointed Davis Lecturer for the present year, and had commenced a course of ten lectures on "Reptiles, living and extinct." These lectures were a continuation of a series given last year in connection with the London Society for the Extension of University Teaching. The number of animals in the Society's collection on the 31st of December last was 2525, of which 735 were mammals, 1331 birds, and 459 reptiles. Amongst the additions made during the past year, 13 were specially commented upon as of remarkable interest, and in most cases representing species new to the Society's collection. About 29 species of mammals, 21 of birds, and 3 of reptiles, had bred in the Society's Gardens during the summer of 1887. The Report concluded with a long list of the donors and their various donations to the Menagerie during the past year.—A vote of thanks to the Council for their Report was then moved by Dr. David Sharp, seconded by Mr. Robert McLachlan, and carried unanimously.—The Report having been adopted, the meeting proceeded to elect the new Members of Council and the Officers for the ensuing year. The usual ballot having been taken, it was announced that Dr. John Anderson, F.R.S., F. Du Cane Godman, F.R.S., John W. Hulke, F.R.S., Osbert Salvin, F.R.S., and Lord Walsingham, F.R.S., had been elected into the Council in place of the retiring members, and that Prof. Flower, C.B., F.R.S., had been re-elected President, Mr. Charles Drummond, Treasurer, and Dr. Philip Lutley Sclater, F.R.S., Secretary to the Society, for the ensuing year.—The meeting terminated with the usual vote of thanks to the Chairman, proposed by Lord Arthur Russell, seconded by Prof. G. B. Howes, and carried unanimously.

Mineralogical Society, May 8.—Prof. Bonney, F.R.S., Treasurer, in the chair.—The following papers were read:—Notes on some minerals from the Lizard, by Mr. J. J. H. Teall.—Contributions to the study of pyrargyrite and proustite, with analyses by Mr. G. T. Prior, by Mr. H. A. Miers.—On Cornish dufrenite, by Prof. E. Kinch.—On a peculiar variety of hornblende from Mynydd Mawr, Carnarvonshire; on a picrite from the Clicker Tor District, by Prof. T. G. Bonney, F.R.S.

PARIS.

Academy of Sciences, May 7.—M. Janssen, President, in the chair.—Note on the introduction of the element of mean averages in the interpretation of the results of statistical returns, by M. J. Bertrand. A demonstration is offered of the following theorem: Whatever be the number of urns (ballot-boxes and the like) and their composition, the law of discrepancies is the same for a single urn of given composition; but this urn will not yield the desired mean average. Hence in order to compare the results of statistical returns with those of abstract calculation two different urns must be assumed, the mean results being assimilated to the drawings made from the first, and the discrepancies to the results yielded by the second.—New theory of the equatorial *coudé* (continued), by MM. Lœwy and Puiseux. In this paper an explanation is given of the special processes applicable to the equatorial region, and of the physical methods employed to estimate the flexion of the axes. In a final paper the results will be given which have already been obtained in the application of this theory to the equatorial *coudé* of the Paris Observatory.—On the convergence of a continuous algebraic fraction, by M. Halphen. Three years ago the author communicated to the Academy the results of his researches concerning continuous fractions, which serve to develop the square root of a polynome of the third degree. In the present paper he extends his investigations to the case of a continuous fraction obtained by developing the function $f(x) = \frac{\sqrt{F(y)} - \sqrt{F(x)}}{y - x}$, where F indicates a polynome of the fourth or of the third degree.—On M. Massieu's characteristic functions in thermodynamics, by M. H. Le Chatelier. It is shown that these functions may be presented under a form somewhat different from that which they are usually made to assume, but which is more convenient for practical purposes.—On the variation of the specific heat of quartz with the temperature, by M. Pionchon. From the experiments the results of which are here tabulated it appears that from about 400° to 1200° C. the specific heat of quartz is constant and equal to 0·305. Thus the increase in the specific heat of this mineral is entirely confined to the interval between 0° and 400° C., a result which presents several points of interest in connection with M. Joubert's researches on the optical properties of the same substance.—On the theory of diamagnetism, by M. R. Blondlot. The author's experiments tend completely to confirm M. Ed. Becquerel's views regarding the mutual relations of paramagnetic and diamagnetic bodies. It is shown that these views are in no way affected by Tyndall's experiment, which fails to prove the existence of diamagnetic polarity, and which is perfectly explicable by Becquerel's theory.—On the electric phenomena produced by the ultra-violet rays, by M. Auguste Righi. In connection with M. Stoletow's recent communication on this subject, the author points out that several of the results here given were previously announced by him in a note presented to the Academy dei Lincei on March 4, and printed at the time.—On the acid phosphites of the alkaline metals, by M. L. Amat. To the acid phosphite of ammonia $(PO_3HO)NH_4O,HO$, previously prepared by him, the author here adds the corresponding salts of potassa and soda $(PO_3HO)KO,HO$ and $(PO_3HO)NaO,HO$, and explains their method of preparation.—On the crystalline form of the trithionate of soda, by M. A. Villiers. The author has succeeded in obtaining crystals of this substance, the measurements of which are here given.—On terpinol, by MM. G. Bouchardat and R. Voiry. It is shown that certain derivatives of the terebenthenes generally supposed to be identical with List's terpinol are really of different composition, although presenting some marked analogies with that substance.—M. G. Demeny describes a number of instruments which he has devised for the purpose of accurately determining the exterior form of the thorax, the extent of the respiratory movements, the profiles and sections of the trunk, and the volume of air inhaled and exhaled. The last-mentioned is described as a self-registering "spirometer."

BERLIN.

Physical Society, April 20.—Prof. du Bois-Reymond, President, in the chair.—Prof. Vogel communicated the results of his researches on the spectrum of carbon. In recent times the spectra of all the carbon compounds have been recognized as being those due to carbon itself, the sole exception being in the case of cyanogen, whose spectrum was considered to be that of the compound, not of carbon itself. The speaker had therefrom investigated the spectrum of cyanogen, with the help of photography. He obtained a spectrum which was marked, from the red to the ultra-violet, by very characteristic lines. The spectrum of a Bunsen burner was next photographed, and it was found that its first three lines coincide in all respects with those of the spectrum of cyanogen; in addition a series of lines lying between the above and also in the blue were found to be identical in both spectra. On the other hand, the two bands in the blue and ultra-violet were absent in the spectrum of the compounds of carbon and hydrogen, being replaced by a series of very characteristic double lines. Prof. Vogel next photographed the spectrum of carbonic oxide, and found that its more highly refracted portion corresponded completely with that of cyanogen. The bands in the blue and ultra-violet were particularly well marked, whereas the less highly refracted half of this spectrum did not correspond with that of cyanogen. Finally, the light emitted by the electric arc was photographed, and its spectrum resembled in all respects that of cyanogen. The speaker drew the conclusion from these observations that in all four cases he was really dealing with the spectrum of carbon. The differences in the several spectra are not dependent upon differences of temperature, inasmuch as the temperature of a Bunsen flame is higher than that of cyanogen, and notwithstanding this the latter gave a more highly developed and complicated spectrum. The speaker was much more inclined to assume the existence of modifications of carbon, of which one yields its spectrum in the Bunsen flame, the other in the flame of carbon monoxide, the two spectra being met with united in those of cyanogen and the electric arc respectively. In photographs of the solar spectrum, the dark background on which the line G is conspicuous shows such a marked correspondence with narrow bands in all the above four spectra that the existence of carbon in the sun must necessarily be assumed.—Prof. Vogel then spoke on colour-perceptions, which he explained by means of experiments. It is well known that when a colour-chart is seen illuminated by the light of a sodium flame it appears colourless: the yellow appears to be pure white, and the other colours appear gray, graduating into black. This result is not observed with other monochromatic light, such as that of thallium or strontium. The speaker was, however, able to produce the same result by means of coloured glasses, whether red, green, or blue; those colours always appeared to be white or very bright which most strongly reflected the light with which the colour-chart was illuminated, all the other colours appearing to be either gray or black. When a second monochromatic light was added to a previous one, such as blue to a yellow light, then definite colour-sensations were observed, which increased in number when a third source of monochromatic light was superadded to the other two. Prof. Vogel laid great stress on the perception of white by monochromatic illumination of a uniformly coloured field of view. He was not prepared to give any explanation of the phenomena, but simply to bring them to notice, with the intention of investigating them further.

BOOKS, PAMPHLETS, and SERIALS RECEIVED

Nature's Hygiene, 3rd edition: C. T. Kingzett (Baillière, Tindall, and Cox).—Œuvres Complètes de Christiaan Huygens: Tome Premier, Correspondance 1638–56 (Nijhoff, La Haye).—Longmans' Junior School Geography: G. G. Chisholm (Longmans).—Kurzes Handbuch der Kohlenhydrate: Dr. B. Tollens (Trewendt, Breslau).—Geology for All: J. L. Lobley (Roper and Drowley).—The Elements of Logarithms: W. Gallatly (Hodgson).—Natural Causation: C. E. Plumptre (Unwin).—Text-book of Practical Metallurgy: A. R. Gower (Chapman and Hall).—Recherches sur le Ceratium Macroceros: E. Penard (Genève).—The Old Babylonian Characters and their Chinese Derivates: Dr. T. de Lacouperie (Nutt).—The Natural History and Epidemiology of Cholera: Sir J. Fayrer (Bale).—The Study of History in American Colleges and Universities: H. B. Adams (Washington).—Tōkyō Sūgaku Butsurigaku Kwai Kiji, Maki No. III. Dai 3.—Asbestos; its Production and Use: R. H. Jones (C. Lockwood).—A Chapter in the Integral Calculus: A. G. Greenhill (Hodgson).—Journal of the Chemical Society, May (Gurney and Jackson).—Annalen der Physik und Chemie, 1888, No. 6 (Barth, Leipzig).—Bulletins de la Société d'Anthropologie de Paris, Tome X. (3 Serie), 4e. Fasc. (Masson, Paris).—Mémoires de la Société d'Anthropologie de Paris, Tome III. (2e. Serie) Fasc. 3 and 4 (Masson, Paris).—Quarterly Journal of the Geological Society, vol. 44, part 2, No. 174 (Longmans).—Bulletin of the American Geological Society, vol. xix., Supplement 1887, vol. xx. No. 1 (New York).—Jamaica, Annual Report on the Public Gardens and Plantations for the year ended September 30, 1887 (Jamaica).

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THURSDAY, MAY 24, 1888.

THE POLYTECHNIC INSTITUTE.

EVERY middle-aged inhabitant of the British Islands must recall more than one occasion when the mind of our country has been strongly stirred on the question of national defence. The adverse evidence of an expert, a rousing article in a newspaper, has often awakened general anxiety of more or less continuance, and followed by more or less adequate results. But it is far more difficult to awaken any widespread concern on behalf of those great abiding national interests which it is our charge and heritage to defend. And yet there are signs of no uncertainty which must to all thoughtful and instructed minds, from many directions, suggest the question whether that industrial leadership which has hitherto made our small and crowded country the world's workshop, and almost the world's mart, is not slipping from us. This is a question not of more or less wealth or luxury, but of very livelihood to the masses of the people under the special conditions of our national existence. If work ceases to come to a workshop, there is nothing for it but prompt dispersal of the workmen. All authorities seem agreed that the population of five or six millions inhabiting England and Wales in the time of Queen Elizabeth represents pretty nearly what their areas can sustain as agricultural, self-supporting countries. But the population of England and Wales alone was shown by the census of 1881 to have reached nearly twenty-six millions. So that seven years ago there was in the southern half of Great Britain an excess of twenty millions above what the country could reasonably support, except as a community of artificers and traders, and general carriers, by import and export, of the world's merchandise. It needs only a glance into past history to see that this, while an enviable position for a nation while prosperity lasts, is practical extinction when the channels of commerce are turned, or lost advantages have transferred production to new centres. Macaulay's fancy picture of the New Zealander sketching the ruins of St. Paul's from the broken arches of London Bridge seems of very little concern to the present citizen, whose ears are deafened with the ceaseless roar and traffic of the streets. And yet precisely that doom of silence and decay has befallen many a proud mother-city of which now "even the ruins have perished." It would far exceed present limits to show in detail how many articles of our own immemorial production we ourselves now largely import, because the foreign workman produces them better, or produces them at less cost. The evidence will be fresh in the recollection of the readers of this journal. Neither can they fail to recall with what persistence we have pointed out the remedy. There is but one real remedy: the better training of the workman; and—if we may be allowed to say it—of his employer too. Everyone who, without prejudice, has opportunity to watch a fair specimen of the British workman at his work must admit that the raw material is as good as ever it was; that in the quantity and quality of the work he can turn out in a given time, few of any nationality can equal, and none surpass him. But in the training he receives, and in the opportunities of his receiving it, there is much left to be desired. And, meantime, there is not only the grave fear, but, in many branches of industry, the accomplished fact, that other nations may and do outstrip us in the race.

Perhaps there is some belated merit in seeing that now; but all honour to those who, with heart and means to labour towards the better training of our artisans, devoted themselves to the endeavour when the need for it was less comparatively obvious. Honour especially to one man, Mr. Quintin Hogg, who, close upon a quarter of a century ago, at an age when most young men are concentrating their best energies on cricket, or football, or lawn tennis (all good things in their way), made it his life's task to raise the skilled workman of London, and furnish him more fully for his labour, for his own sake and for ours. Probably most of our readers know how that small enterprise has become a great one indeed, with the old Polytechnic for its present home and centre, and with a fuller variety of classes and branches, and with a greater comprehensiveness of scheme, than we can now attempt to describe. But all has hitherto rested on the shoulders, and been sustained by the purse, of Mr. Hogg himself, who, during the past six years, has spent, speaking broadly, some £100,000 in establishing and sustaining these admirable schools. But the time has now come when so great a burden, for the work's sake as well as for his own, should no longer depend upon the means and life of a single man; and there is now an opportunity of securing for the Institute something like an adequate endowment. The Charity Commissioners have offered to endow it with £2500 per annum on condition that the public find £35,000 as a supplementary fund. £18,000 have already been promised by the personal friends of the founder; but £17,000 still remain to be raised—a large sum no doubt, but a small one compared to our still unrivalled resources, and the national value of the Institute, not only for its own immediate results, but as a model for similar efforts in all the great centres of our industry. Those who believe in science—that is, in faithfully accurate and exact knowledge—as the only sure basis for any national prosperity that is to bear the stress of the fierce competition of our times, are earnestly invited to make themselves acquainted with the work of the Institute, and to contribute to its funds. Eighty-one thousand members and students have joined since it was moved to the Polytechnic, 309 Regent Street, in 1882. All donations or subscriptions will be thankfully received there, or by Mr. Quintin Hogg, 3 Cavendish Square, W.

THE GEOGRAPHICAL DISTRIBUTION OF THE FAMILY CHARADRIIDÆ.

The Geographical Distribution of the Family Charadriidæ; or the Plovers, Sandpipers, Snipes, and their Allies. By Henry Seebohm. (London: H, Sotheran and Co., 1888.)

THIS is a handsome volume of more than 500 pages, and it is illustrated by twenty-one coloured plates, drawn in Mr. Keuleman's best style. Mr. Seebohm has eschewed giving much information as to the habits of these families of wading birds, and has made a special point of the geographical distribution, a branch of the subject which cannot fail to attract the interest of every true naturalist. The introductory chapters treat of (1) the "Classification," and (2) the "Evolution" of Birds. Chapter III. details the author's views on the "Differentiation of Species," and Chapter IV. deals with the "Glacial Epoch." Chapters V. to IX. are devoted to the migration of birds, and end with a scheme of classification of the *Charadriidæ*. Here are, in fact, enunciated clearly all the articles of the Seebohm faith!

Evolutionists will probably join issue with Mr. Seebohm on many of his conclusions, and geologists may have something to say as to the possibility of glacial epochs causing all that the author claims for them, but ornithologists are scarcely likely to accept all his conclusions at once. If we are to believe Mr. Seebohm, there is very little progress being made in ornithological work in the Old World, his sympathies being evidently more with the American school of ornithologists, for whose method of nomenclature he has great respect. The non-adoption of trinomial principles Mr. Seebohm attributes to the "conservative views of British ornithologists," though he is mindful to add: "It is, however, only fair to remember that much allowance must be made for the narrow, because insular, views of British ornithologists." Shade of Darwin! The author has singled out the present writer as one of those who seem to have had "no definite idea of what they meant by a sub-species"; but we may assure Mr. Seebohm that in 1874 we did *not* use the term of *Gyps hispaniolensis* as a sub-species of *G. fulvus* "in an absolutely arbitrary manner," and we did not expect to find our nomenclature discussed under the heading of a "vague use of trinomials." Our object was to recognize evident facts, but at the same time to retain a binomial form of name for every bird, and the uncertainty which still surrounds the American method of trinomial names has not yet encouraged us to abandon the simpler and decidedly less clumsy way of expression. Surely Mr. Seebohm himself must admit that to have to speak of an Oyster-catcher as *Hæmatopus niger ater* (p. 311) is not an advantage, and this is only one result of pushing trinomial nomenclature to its extreme. There are not wanting signs that the advocates of the system are beginning to groan under the weight of the burden they have placed on their own shoulders; and when the inevitable return to the old simple path of binomial nomenclature takes place, the only tangible result will have been to have weighted the already frightful list of ornithological synonyms with an additional number of long names. Even Mr. Seebohm tries to modify the task of quotation of books by simplifying some titles; as, for instance, when he speaks of "Coues and Co., Check-List" (p. 427), as if the authors of the admirable A.O.U. "Check-List of North American Birds" had formed themselves into a Limited Liability Company for the manufacture of trinomials.

Another point on which Mr. Seebohm may fairly be called to task is for the number of new names which his book propounds. On the back of the title-page he quotes wise saws from the writings of John Ray (1878), A. R. Wallace (1876), and Henry Seebohm (1883), concerning the necessity of having simple names for birds, and those generally understanded of the people. Here are his own words:—"I have adopted a scheme which appears to me to be the most practical method of any which have been suggested. It may not satisfy the requirements of poetical justice; but it is at least consistent with common-sense. I adopt the name which has been *most used* by previous writers. It is not necessary for me to encumber my nomenclature with a third name, either to denote the species to which it refers, or to flatter the vanity of the author who described it: all my names are *auctorum plurimorum*." Either our author had forgotten that he had nailed this flag to the mast when he began the present book, or the system of *auctorum plurimorum* does not suit the *Charadriidæ*; for the next student of these birds will find that for the 235 species enumerated by Mr. Seebohm, he is responsible for giving to sixty-five of them names not previously in vogue; and the number would have been greater, had not Schlegel worked somewhat on the same line of ideas, while many of the trinomial combinations had been anticipated by "Coues and Co."

The book is profusely illustrated by woodcuts, showing the specific characters of the different species, and these will be invaluable to the student of these difficult birds. In fact, no work has ever been so remarkably treated in this respect, and it will be the book of reference for the *Charadriidæ* for many years to come. The "Keys to the Species" are also excellent, and Mr. Seebohm deserves every credit for having given us such a complete arrangement of some of the most tiresome of all the birds which it falls to the lot of the ornithologist to determine. Every naturalist who works out his facts as completely as the author has done is permitted to account for them by any theory which seems to him good; and Mr. Seebohm's arguments as to the origin of the species and their distribution are not only examples of clever writing, but are plausible enough if once the absolute certainty of the *Charadriidæ* having been driven from the Polar Basin by successive glacial epochs is conceded. Many ornithologists, however, will think that he carries his theory a little too far, as, for instance, when he places the Avocets and Stilts in one genus, *Himantopus*. How they originally came from the north, were split up in bands, became some of them "semi-Stilts" and "semi-Avocets"; how they thought nothing of emigrating (cause not hinted at) from the New World across the Atlantic to the Canary Islands and Spain, or from the Chilian sub-region across the Pacific to New Zealand and Australia—these and many other interesting theories of distribution will reward the student of Mr. Seebohm's book. Most ornithologists will be more grateful for small mercies than Mr. Seebohm is, and thank Dame Nature for having given them characters whereby in a few lines a genus can be written down. Take, for instance, the members of the genus *Esacus*, which Mr. Seebohm unites to *Œdicnemus*, and yet his woodcuts show that the former genus has an enormous bill, longer than the head itself—surely a genuine character of importance. Then, again, *Anarhynchus*, with its asymmetrical bill—confined to New Zealand—need not be united to *Charadrius*;—and so on. With his theory of distribution strong in his mind, the Avocets, with up-turned bill, are united to the Stilts, with their straight bill, because Mr. Seebohm has no doubts as to their common origin in the distant past; but looking at the present almost identical distribution of *Himantopus melanopterus* and *Recurvirostra avocetta*, *H. mexicanus* and *R. americana*, it would seem as if they had long ago been separated as distinct generic forms, as they would have no business to occupy the same areas, if Mr. Seebohm's theory were true. Is it not possible that they were developed as Avocets and Stilts in very remote times, and that similar causes have driven them to occupy the same areas of distribution? And may not both have had a *southern* instead of a northern origin? Thus *Cladorhynchus* in Australia, *Himantopus andinus* in the Andes (apparently, from Mr. Seebohm's illustration, belonging to a distinct genus), and the various species of Stilts in Australia, New Zealand, and Brazil, would remain as isolated species of a former stock, which probably inhabited a continuous area in the South Atlantic and South Pacific Oceans. Where circumstances were favourable to their stay, some may not have migrated northwards, and the differences in some of the southern species could be accounted for by their subsequent isolation, rather than by their inconsequent flight from Chili to New Zealand, as Mr. Seebohm supposes.

Besides the woodcuts of heads, tails, &c., and other specific characters, the present volume is crowded with woodcut illustrations by Mr. John Millais, Mr. Lodge, and Mr. Holding. They are mostly extremely well done, but Mr. Millais seems a little inclined to fashion his Waders on the model of a peg-top.

R. Bowdler Sharpe.

THE MINERALS OF NEW SOUTH WALES.

The Minerals of New South Wales, &c. By A. Liversidge, M.A., F.R.S., Professor of Chemistry and Mineralogy in the University of Sydney. (London: Trübner and Co., Ludgate Hill, 1888)

IT was a very happy thought of Prof. Liversidge to celebrate the centenary of the foundation of the colony of New South Wales by the publication of this handsome and comprehensive volume. Giving, as it does, a very clear account of what is known of the mineral resources of the oldest of the Australian colonies, it brings clearly before the mind of the reader how much has already been accomplished in developing the subterranean resources of an important part of the British Empire, and how large is the promise for the future. The term "mineral," we may mention, is not employed in this work in its narrower scientific sense; coals and oil-shales, and even mineral waters, receiving a due amount of notice in it.

The basis of the present work is found in a paper published by the author in the Transactions of the Royal Society of New South Wales, in 1874, of which memoir a second edition was published by the Mining Department of the colony in 1882. Prof. Liversidge has added very largely to his original memoir; and the numerous analyses of minerals and rocks, made by himself, Mr. W. J. Dixon, F.I.C., and the Government Analyst of the Mining Department, greatly increase the value of the book. Owing to the absence of the author from the colony during the past year, the work has been printed and issued in this country; but, as a proof of the manner in which the book has been brought fully up to date, we may note the statement, on p. 185, of the discovery, by Mr. T. W. Edgworth David, of the Geological Survey of New South Wales, of the sparsely distributed mineral leucite in the Australian colony, the fact having only been announced to the Mineralogical Society so recently as October in last year.

A considerable amount of space is naturally devoted to discussions concerning the occurrence of the precious metals—the account of gold occupying 34 pages, and that of silver 13 pages. The interesting series of assays of New South Wales gold, and an account of the chief nuggets found in the colony, are of much interest. In connection with this subject, we have in the work before us a very clear and concise, but very carefully drawn up, statement concerning the often-disputed question of the original discoverer of gold in Australia. The author states his facts and sources of information, taking great care to give references in all cases, and those interested in the question will have little difficulty in arriving at a decision as to the relative merits of the claims which have been put forward on behalf of Count Strzelecki, the Rev. W. B. Clarke, Sir Roderick Murchison, and other less known individuals, to whom the discovery has been ascribed. One of the most interesting and instructive among the many tabular statements in this work is that which indicates the number of minerals which have yielded, on assay, larger or smaller quantities of gold and silver. This table seems to indicate that, even should the alluvial washings and quartz-reefs be exhausted of their auriferous contents, there still remain in Australia many available and very valuable sources of the precious metals.

Still more important in its bearing on the future welfare of the colony is the account of the common metallic ores, and of the coal, lignite, and oil-shale deposits. There are few, if any, of the metals used in the arts, of which abundant sources of supply are not found within the limits of the colony. The coal-fields are said to cover about one-half the area of those of Great Britain, and numerous analyses and other details enable us to judge of the quality of the fuels which they yield. In the discussion of this important question, Prof. Liversidge's great knowledge and experience as a chemist invest his opinions with the highest value.

Although the book is not a technical mineralogical treatise alone, mineralogists will find very careful descriptions of all the minerals, including the gem stones, which have been found within the colony. Their study of the subject will be much facilitated by the large coloured map which forms a frontispiece to the volume.

In concluding this notice we cannot but congratulate the author upon the enterprise and energy which have enabled him to prepare such a treatise as the present one. The objects aimed at in such books as Zepharovic's "Mineralogisches Lexicon für das Kaiserthum Oesterreich" may seem at first sight incompatible with those to which works like Mr. Albert Williams's "Mineral Resources of the United States" are devoted; but Prof. Liversidge has shown that this is by no means the case, and he has achieved the feat in the case of a young and rising colony, where the difficulties of the undertaking must have been more than usually great. The colony, too, is to be congratulated on its good fortune in having as an occupant of the Chemical Chair in its University, one who has shown himself so successful in attaining practical, while not losing sight of the scientific, results of his researches.

OUR BOOK SHELF.

Elementary Chemistry. By William S. Furneaux, F.R.G.S., Science Demonstrator, London School Board. (London: Longmans, Green, and Co., 1888.)

THE main object of this little work is to assist young students intending to sit for the chemistry examination of the Science and Art Department in the new alternative elementary stage. It appears to be, in fact, an illustrated expansion of the detailed syllabus published by the Department in their Directory.

The want of such a work has possibly been felt by many teachers of this "alternative" or "natural" chemistry, which appears to be rapidly becoming more and more popular with young beginners. There is something truly fascinating in learning these mysteries of common things, and, what is still more important, the knowledge gained has its practical applications in every-day life. In order to afford teachers some idea of the methods recommended of performing the class experiments themselves, the Department have caused to be placed in the western galleries of the South Kensington Museum a complete set of apparatus, as simple and inexpensive as is compatible with the object in view, arranged under the personal direction of the examiners, to illustrate the method of performing each of the experiments indicated in the syllabus. It is to be hoped, therefore, that all who are interested in the teaching of the alternative elementary stage of chemistry, and who can conveniently do so, will avail themselves of this opportunity of comparing the experimental methods there recommended with those which they themselves have previously adopted. One cannot help thinking that many of the methods illustrated by Mr. Furneaux are much too complicated, and it is to be regretted that his book was in the press before the completion of the collection in the western galleries, which was accomplished about two months ago.

The majority of the theoretical explanations leave little to be desired. The ideas of the author, however, as to the nature of the Bunsen flame appear scarcely to accord with more recent investigations, the effect of mixture with an inert gas being entirely overlooked. A. E. T.

Companion to the Weekly Problem Papers. By the Rev. John Milne, M.A. (London: Macmillan and Co 1888.)

THE title of this work gives no adequate idea of its contents. It consists of some 340 pages, which, if about 60 pages be excepted, are devoted entirely to geometry. Besides the author, several other mathematicians are contributors, viz. Mr. R. F. Davis, Prof. Genese, Rev. T. C. Simmons, and Mr. E. M. Langley.

The object of the book seems to be to give prominence to what is here designated "The Modern Geometry of the Triangle." This is seen to consist of a group of pretty theorems which arise from a consideration of the "Brocard points" and the "Lemoine point" of a triangle. The successive chapters bear the titles, "Antiparallels, Isogonals, and Inverse Points," "The Brocard Points and Brocard Ellipse," "The Lemoine Point and Triplicate Ratio Circle," "The Brocard Circle and First Brocard Triangle," "The Tucker Circles," "The Cosine and Taylor Circles," "The Co-Symmedian and Co-Brocardal Triangles," and "Miscellaneous Theorems and Constructions." They comprise a good and almost complete account of the present knowledge of these subjects.

On p. 180 there is a *résumé* of the bibliography, which has evidently been carefully compiled by the knot of enthusiasts in this country who have followed in the footsteps of M. Lemoine M. Brocard, M. Vigarié, Prof Neuberg, M. Catalan, and others. To these investigators on the Continent most of the results here given were known prior to 1881; they were subsequently arrived at independently by mathematicians in England who were unacquainted with the work already accomplished, in the same field of research, abroad. In fact, in the *résumé*, discoveries, and rediscoveries, and rediscoveries of rediscoveries succeed one another in bewildering fashion. The reasons which have led to the nomenclature in certain cases are difficult to fathom. We find, for instance, a circle associated with the name of one mathematician, when, admittedly, the same circle had been examined by a Continental investigator some years previously, whose name, if name be necessary, it ought to bear.

The algebraic portions comprehend sections on "Theory of Maximum and Minimum," "Theory of Elimination," "Summation of Series," "Binomial Series," and "Algebraical and Trigonometrical Identities."

The book will be chiefly useful to those who take an interest in recent triangular geometry; it will enable them to refer to original sources in Continental mathematical publications, and to follow further developments in English magazines. They will also find collected here most of the leading propositions given in a form which is without doubt both judicious and attractive.

Elementary Hydrostatics, with Numerous Examples and University Papers. By S. B. Mukerjee, M.A. (Calcutta: Thacker, Spink, and Co., 1888.)

THE compiler of this handy little work is Assistant Professor of Mathematics in the Lahore College, who, having been, as is the wont of his order, unable to select from the numerous text-books in existence one which seemed fully to meet the wants of his classes, has culled his elegant extracts from them, and so got what he wanted. This proceeding is a good one for his pupils, and saves them the trouble and expense of purchasing and reading many text-books. The selection is well made, and the compiler suitably acknowledges his indebtedness to the English writers (especially to Dr. Besant's classical work). The subjects handled are definitions and first principles, density and specific gravity, equilibrium of fluids, total pressures and resultant pressures on immersed surfaces, floating bodies, on air and gases, determination of specific gravities, and the application of hydrostatical principles in the construction of instruments and machines. Then follow several papers of problems set in the Calcutta University Examinations from 1860 to 1884; and the book closes with an appendix of formulæ to be remembered, and another appendix which gives a short history of the growth of the principles of hydrostatics, taken for the most part from Whewell's "History of the Inductive Sciences." In the body of the work are given numerous illustrative examples, many of which have been carefully worked out. Putting on one side the manufacture of the book—and herein, perhaps, Mr. Mukerjee is only more honest in making known his indebtedness than many are in the writing of text-books—we can congratulate the students on having such a good work in their hands, and can indorse the favourable opinion expressed upon it by Prof. T. C. Lewis, Principal of the College.

Arithmetic for Beginners: a School Class-book of Commercial Arithmetic. By the Rev. J. B. Lock, M.A. (London: Macmillan and Co., 1888.)

IT is not necessary to report upon this little book at any length. It is founded upon the author's larger work, but modifications as to arrangement and treatment of some of the subjects and as to the examples have been introduced. Then, with an eye to the requirements of the examination for commercial certificates, a chapter on exchange and foreign money has been added (in a worked-out example on p. 151 there is an error of some pecuniary magnitude), and the chapter "On Recurring Decimals, not required by Commercials," finds a place at the close of the text. Mr. Lock is generally so careful in his explanations that we are surprised at his omitting all reference to brokerage in his account of the transferment of stock. Numerous examples are given in the text, and six examination-papers and answers to all questions complete a capital hand-book.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Weight and Mass.

PROF. GREENHILL, in his letter which appears in NATURE of May 17 last (p. 54), has again repeated his views on the use of the word *weight*. He has not, however, replied to the criticisms of those who differ from him (see NATURE, vol. xxxvi. pp. 221, 317).

His opponents wish to know how practical engineers who use the word *weight* as synonymous with the physicists' mass, treat a problem involving inertia. Prof. Greenhill has not yet given us an example of such a problem taken from some modern text-book of the practical engineer; nor has he yet given us in simple language a definition of weight. Prof. Greenhill some time ago referred me to Kennedy's "Mechanics of Machinery" for such a definition, but I venture to say that there is no such definition to be found in that standard work.

My own idea is as follows: Matter has many properties—inertia, weight (the force with which the earth pulls it), volume, &c.—and Newton's great discovery consisted partly in seeing clearly that the universal property of matter by which it must be measured is its *inertia*, defined as its capacity for resisting change of velocity.

The *mass* of a body is that which can be ascertained by the operation of *massing;* such an operation, that is, as the following: To a given lump of matter apply some strain or force, and observe the acceleration produced in the matter by that force; then ascertain by experiment to how many lumps of matter called pounds this same force will communicate an equal acceleration.

The *weight* of a body is that which is ascertained by the operation of *weighing*. To weigh a body it is placed on a spring balance, and the force of the earth's attraction is observed by showing the compression of the steel spring of the machine.

It happens, however, that the mass of a body is proportional to its weight; consequently it is sufficient to ascertain whether the weights of two masses are equal in order to ascertain that their masses are equal. The weights of two masses are ascertained to be equal by putting them each on one side of a balance, and observing that the force of the earth's attraction on each is the same. Hence the very difficult operation of *massing* as described above is replaced by the easy operation of weighing.

Prof. Greenhill tells us that "now the invariable unit, the mass, is measured in terms of a variable unit." Is this so? Is it not a fact that those who use exclusively the force of the earth's attraction as the measure of matter, rarely if ever have any conception of the idea of inertia? When the practical engineer has to do with inertia, as in cases of "*centrifugal force,*" he works by formulæ or rule of thumb.

Prof. Greenhill's sentences, "a force equal to the weight of the mass of 10 pound weights," and "the weight of 32 pound weights on the Earth is at the surface of Jupiter a force of 71 pounds' weight," are entirely original.

I believe he means to express "the weight of 10 pounds," and the weight of 32 pounds on the earth is a force equal to the weight of 71 pounds on the surface of Jupiter.

Caius College, May 21. JOHN B. LOCK.

Work and Energy.

WHILE a discussion of the nomenclature of mechanics is going on in NATURE, I would venture to suggest that an effort should be made to get rid of the practice of expressing energy in foot-pounds or foot-poundals. There are certain quantities of work, not of energy. To speak of a foot-pound of energy is quite as incorrect as it would be to speak of a pint of velocity, a yard of acceleration, an acre of momentum, or a pound of duration. There is great need of a short name for the unit of $\frac{1}{2}mv^2$.

Bardsea, May 21. EDWARD GEOGHEGAN.

On the Reappearance of Pallas's Sand Grouse (*Syrrhaptes paradoxus*) in Europe.

I BEG to add the following statements to my communication of May 12 concerning Pallas's sand grouse in Central Europe (see NATURE, May 17, p. 53):—

April 22, Cernozitz, Bohemia.
,, 26, Portitz, near Leipzig, Saxony.
,, 27, Güttmannsdorf, near Reichenbach, Silesia.
,, 27? near Hanover.
,, 27-28, near Hermannstadt, Transylvania.
,, 29, Marmarosch-Comitate, Hungary.
Last days of April: Alsofehér-Comitate, Transylvania.
Gebhardsdorf, Silesia.
Brod, Bohemia.
First days of May: Tullner-field, near Vienna.
Moravia.
Hungary.
Enzersdorf, near Vienna.
Anclam, Pomerania, Prussia.
May 6, Haida, Bohemia.
,, 6, Eidelstedt, near Hamburg.
,, 7? near Schweinitz, Silesia.
,, 7, Oederan, Saxony.
,, 7, 6.30 a.m., near Oederan, Saxony.
,, 8, Wiener Neustadt, Austria.
,, 8? Dalmatia.
,, 8? Grossvoigtsberg, Saxony.
,, 8? near Leipzig, Saxony.
,, 8? near Herrenhut, Saxony.
,, 9, Oederan, Saxony, and nearly every following day there.
,, 13, Selb, Saxony.
,, 13? Grossvoigtsberg, Saxony.
,, 13, Schluckenau, Bohemia.
,, 16, 5 p.m. Oederan, Saxony.

A. B. MEYER.

Royal Zoological Museum, Dresden, May 20.

A FARM in this neighbourhood was visited yesterday by a flight of about forty sand-grouse (pin-tailed). They were first seen about 6 p.m. feeding on a ploughed field. On rising they took a north-westerly course. A pair which were shot by a gamekeeper are in my possession. The presence of these birds in our country is, I believe, of sufficiently rare occurrence to justify me in asking whether they have been noticed in other districts during the last few days. F. M. CAMPBELL.

Rose Hill, Hoddesdon, Herts, May 21.

Tables of Reciprocals.

IN investigating spectral phenomena it is often necessary to convert wave-lengths in frequencies. Can any of your correspondents inform me if there exist in England tables of reciprocals, by which this may be done easily and with sufficient accuracy? V. A. JULIUS.

Delft, Holland, May 19.

On the Veined Structure of the Mueller Glacier, New Zealand.

THE Mueller Glacier, in the Mount Cook district, has a total length of between six and seven miles, with a breadth of one mile in its lower portion. Like most, if not all, of the New Zealand glaciers of the first order, the lower mile or two is so thickly covered with rock debris that the ice can only be seen in the crevasses. All through the lower portion of the glacier the veined or ribboned structure is well marked, running nearly in the direction of the glacier. But at the terminal face there are two systems of veined structure, with the same strike but crossing one another at angles between 15° and 20°. In one system the blue bands are small, from a half to one inch thick, and separated from each other by bands of white ice, with large air-bubbles, about twice the thickness of the blue bands. The blue bands are irregular and sometimes anastomose. This system is similar to the veined structure found higher up the glacier.

The second system is formed by large and regular blue bands from three to six inches broad and from two to six or more feet apart. This coarser system is only occasionally developed. The finer system forms a well-marked synclinal curve on the terminal ice cliffs, which are from 250 to 300 feet high.

The ice here contains in places numerous angular stones, principally of slate, scattered irregularly through it, and these fragments always have their broad, or cleaved, surfaces parallel to the smaller system of veins. These stones have no doubt entered the ice through the numerous moulins and crevasses which are found higher up the glacier, but as they are not found in bands nor in pipes, they must have been moved in position by the flowing of the ice, consequently they must originally have been variously oriented, and their present parallelism to the veins is a decisive proof that the smaller system is due to pressure at right angles to the structure. The origin of the coarser system is not so clear. I did not notice it higher up the glacier, as I ought to have done if it had been an older system than the smaller veins. While, on the other hand, if it is a newer system the rock fragments would probably have been oriented parallel with it instead of with the finer system.

The clear blue ice is generally supposed to resist melting better than the white ice, and to stand out in ridges; but I observed nothing of this on the Mueller Glacier. Both kinds of ice melt here with about equal rapidity. The grooving of the ice, by runlets of water, is certainly parallel to the structure when that structure is vertical or highly inclined; but the grooves are formed in several layers of both kinds of ice, and it seemed to me that the blue ice melted rather more rapidly than the white ice. I cannot suggest any cause for this difference between the ice of the Mueller Glacier and that of the Swiss glaciers. F. W. HUTTON.

Christchurch, New Zealand, March 22.

On the Rainfall and Temperature at Victoria Peak, Hong Kong.

THE first column of the following table shows the month of the year; the second, the mean rainfall at the Observatory (about 100 feet above the sea) from ten years' records; the third, the mean of the past four years' fall; the fourth, same for Victoria Peak (about 1800 feet above the sea); the fifth, the proportion between the figures in the two preceding columns; the sixth, the height of ascent in feet for one Fahrenheit degree of decrease of temperature (mean of the past four years):—

I.	II.	III.	IV.	V.	VI.
January	1·47	2·97	4·63	1·56	288
February	1·66	2·30	3·56	1·55	305
March	3·53	3·41	3·60	1·06	489
April	6·55	7·89	9·19	1·16	407
May	9·82	4·86	6·29	1·29	309
June	12·67	14·42	16·71	1·16	259
July	16·41	16·55	20·29	1·23	274
August	16·93	15·27	17·53	1·15	289
September	9·89	7·98	7·01	0·88	283
October	5·06	2·57	2·06	0·80	281
November	1·04	0·77	1·19	1·54	267
December	0·49	0·97	1·21	1·25	278
Year	85·52	79·96	93·27	1·17	310

The rainfall at the Peak exceeds the record at the Observatory by about one-sixth of the whole amount, and this appears to be due to the circumstance that the mountain presents an obstacle to the wind from whatever side it blows, in consequence of which the air is forced to rise, and being thereby cooled it precipitates more moisture in the form of rain. Even when the air is moderately dry at sea-level its temperature may be decreased below the dew-point in the course of such a rise. The comparatively great rainfall in hilly districts must be attributed to this, for a hill must of course exercise its influence at a distance all round. Our rainfall would therefore be smaller if there were no hills in this neighbourhood. But during the months of September and October less rain is collected at the upper level. This is explained by the circumstance that most of the rain in those months is due to typhoons, when the air is everywhere ascending, even above the open sea; and the defect at the Peak is most noticeable during the raging of a typhoon. The fact that less rain is measured above must, however, be further investigated. It is very doubtful whether it would not be as well to expose the funnels of the gauges 4 feet above the ground, where they would not be so much affected by the rain drifting along the surface of the earth in typhoons, as to have them 1 foot above the grass, as is the case here.

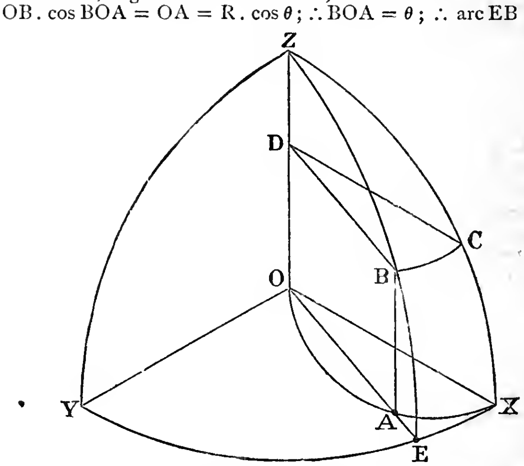
The last column of the table proves the great variability of the fall of temperature with increasing height. It depends upon the humidity of the air. The astronomical refraction near the horizon must be affected by this, but it is rather doubtful whether the effect should be ascertained by comparing observed refractions with meteorological registers kept on mountains on account of the condensation of moisture which tends to raise the temperature on the top of the hill. But it would appear to be time that some astronomer studied the refraction in connection with daily weather-maps, seeing that the variation of temperature with increasing height is so different in cyclones and anticyclones. Of course near the centre of a cyclone it is scarcely possible to make astronomical observations. Bessel's theory of refraction is considered a failure within 5° of the horizon. Ivory's theory might possibly be made to account for the refraction nearly down to the horizon by observing the value of the constant f in connection with the isobars. It, on the whole, represents the variation of temperature high up in the air as estimated by meteorologists. W. C. DOBERCK.

Hong Kong Observatory, February 11.

Problem by Vincentio Viviani.

To pierce in an hemispherical dome four windows such that the remainder of the surface shall be exactly quadrable. It was solved by Leibnitz, J. Bernoulli, and others. Viviani himself, in 1692, published the construction, but without proof. Divide the base of the dome into quadrants; on the four radii as diameters trace semi circles, one in each quadrant; the four right semi-cylinders, of which these are the bases, will pierce the dome in the required windows. The following simple proof, for which I am substantially indebted to Prof. Francis W. Newman, would probably interest many readers of NATURE:—

OXYZ is quarter of dome; AB, generator of cylinder meeting dome in B; BCD, plane parallel to base. Radius of dome $= R =$ OX = OB; angle CDB = XOA $= \theta$; DC = DB = OA $= R\cos\theta$; OB . cos BOA = OA = R . $\cos\theta$; ∴ BOA $= \theta$; ∴ arc EB $= R\theta$;



arc BC $= \theta \,.\, R\cos\theta$. Element of surface of window is BC . d(EB) $= R^2\theta \,.\, \cos \,.\, \theta \,.\, d\theta$; ∴ surface of window is the integral of this from $\theta = 0$ to $\theta = \frac{1}{2}\pi$. Integrating by parts, and taking limits, surface of window $= R^2\left(\frac{1}{2}\pi - 1\right)$; ∴ the remainder of the surface XYZ is R^2, which is exactly quadrable. Q.E.D.

Cor. The quadrable part of the quarter-dome is equal to the surface of the semi-cylinder which is within the dome. For, if AB $= z$, and arc XA $= s = R\theta$, element of surface of the cylinder is $z \,.\, ds = R^2 \,.\, \sin\theta \,.\, d\theta$; ∴ the entire surface within the dome is the integral of this from $\theta = 0$ to $\theta = \frac{1}{2}\pi$, viz. R^2.

A general discussion of Viviani's problem may be seen in Lacroix, "Traité du Calcul Differentiel et du Calcul Intégral," tome ii. pp. 219–22. EDWARD GEOGHEGAN.

Bardsea, May 2.

SUGGESTIONS ON THE CLASSIFICATION OF THE VARIOUS SPECIES OF HEAVENLY BODIES.[1]

VI.

ON THE CAUSE OF VARIATION IN THE LIGHT OF BODIES OF GROUPS I. AND II.

I. GENERAL VIEWS ON VARIABILITY.

IN my former paper I referred to the collision of meteor-swarms as producing "new stars," and to the periastron passage of one swarm through another as producing the more or less regular variability observed in the case of some stars of the class under consideration.

I propose now to consider this question of variability at somewhat greater length, but only that part of it which touches non-condensed swarms; *i.e.* I shall for the present leave the phenomena of new stars, and of those whose variability is caused by eclipses, aside.

It is not necessary that I should pause here to state at length the causes of stellar variability which have been suggested from time to time. It will suffice, perhaps, that I should refer to one of the first suggestions which we owe to Sir I. Newton, and to the last general discussion of the matter, which we owe to Zöllner ("Photometrische Untersuchungen," 76 and 77, p. 252).

Newton ascribed that special class of variability, to which I shall have most to refer in the sequel, as due to the appulse of comets.

"Sic etiam stellæ fixæ, quæ paulatim expirant in lucem et vapores, cometis in ipsas incidentibus refici possunt, et *novo alimento accensæ pro stellis novis haberi.* Hujus generis sunt stellæ fixæ, quæ subito apparent, et sub initio quam maxime splendent, et subinde paulatim evanescunt. Talis fuit stella in cathedra Cassiopeiæ quam Cornelius Gemma octavo Novembris 1572 lustrando illam cœli partem nocte serena minime vidit; at nocte proxima (Novem. 9) vidit fixis omnibus splendidiorem, et luce sua vix cedentem Veneri. Hanc Tycho Brahæus vidit undecimo ejusdem mensis ubi maxime splenduit; et ex eo tempore paulatim decrescentem et spatio mensium sexdecim evanescentem observavit" ("Principia," p. 525, Glasgow, 1871).

With regard to another class of variables he makes a suggestion which has generally been accepted since.

"Sed fixæ, quæ per vices apparent et evanescunt, quæque paulatim crescunt, et luce sua fixas tertiæ magnitudinis vix unquam superant, videntur esse generis alterius, et revolvendo partem lucidam et partem obscuram per vices ostendere. Vapores autem, qui ex sole et stellis fixis et caudis cometarum oriuntur, incidere possunt per gravitatem suam in atmosphæras planetarum et ibi condensari et converti in aquam et spiritus humidos, et subinde per lentum calorem in sales et sulphura et tincturas et limum et lutum et argillam et arenam et lapides et coralla et substantias alias terrestres paulatim migrare."

Zöllner, in point of fact advancing very little beyond the views advocated by Newton and Sir W. Herschel, considers the main causes of variability to be as follows. He lays the greatest stress upon an advanced stage of cooling, and the consequent formation of scoriæ which float about on the molten mass. Those formed at the poles are driven towards the equator by the centrifugal force, and by the increasing rapidity of rotation they are compelled to deviate from their course. These facts, and the meeting which takes place between the molten matter, flowing in an opposite direction, influence the form and position of the cold non-luminous matter, and hence vary the rotational effects, and therefore the luminous or non-luminous appearance of the body to distant observers.

This general theory, however, does not exclude other causes, such as, for instance, the sudden illumination of a star by the heat produced by a collision of two dark bodies, variability produced by the revolution of a dark body, or by the passage of the light through nebulous light-absorbing masses.

If the views I have put forward are true, the objects now under consideration are those in the heavens which are least condensed. In this point, then, they differ essentially from all true stars like the sun.

This fundamental difference of structure should be revealed in the phenomena of variability; that is to say, the variability of the bodies we are now considering should be different *in kind* as well as in degree from that observed in bodies like the sun or α Lyræ, taken as representing highly condensed types. There is also little doubt, I think, that future research will show that, when we get short-period variability in bodies like these, we are really dealing with the variability of a close companion.

II. ON THE VARIABILITY IN GROUP I.

That many of the nebulæ are variable is well known, though so far as I am aware there are no complete records of the spectroscopic result of the variability. But bearing in mind that in some of these bodies we have the olivine line by itself, and in others, which are usually brighter, we have the lines of hydrogen added, it does not seem unreasonable to suppose that any increase of temperature brought about by the increased number of collisions should add the lines of hydrogen to a nebula in which they were not previously visible.

The explanation of the hydrogen in the *variable* stars is not at first so obvious, but a little consideration will show that this must happen if my theory be true.

Since the stars with bright lines are, as I have attempted to show, very akin to nebulæ in their structure, we might, reasoning by analogy, suppose that any marked variability in their case also would be accompanied by the coming out of the bright hydrogen lines.

This is really exactly what happens both in β Lyræ and in γ Cassiopeiæ. In β Lyræ the appearance of the lines of hydrogen has a period of between six and seven days, and in γ Cassiopeiæ they appear from time to time, although the period has not yet been determined.

III. ON THE VARIABILITY IN GROUP II.

This same kind of variability takes place in stars with the bright flutings of carbon indicated in their spectra, *o* Ceti being a marvellous case in point. In α Orionis, one of the most highly-developed of these stars, the hydrogen lines are invisible; the simple and sufficient explanation of this being that, as I have already suggested, the bright lines from the interspaces now at their minimum and containing vapours at a very high temperature—*teste* the line-absorption spectrum now beginning to replace the flutings—balance the absorption of the meteoritic nuclei.

Anything which in this condition of light-equilibrium will increase the amount of incandescent gas and vapour in the interspaces will bring about the appearance of the hydrogen lines as bright ones. The thing above all things most capable of doing this in a most transcendental fashion is the invasion of one part of the swarm by another one moving with a high velocity. This is exactly what I postulate. The wonderful thing under these circumstances then would be that bright hydrogen should *not* add itself to the bright carbon, not only in bright-line stars, but in those the spectrum of which consists of mixed flutings, bright carbon representing the radiation.

[1] The Bakerian Lecture, delivered at the Royal Society on April 12, by J. Norman Lockyer, F.R.S. Continued from p. 60.

I now propose to use this question of variability in Group II. as a further test of my views.

The first test we have of the theory is that there should be more variability in this group than in any of the others. Others are as follows. (2) When the swarm is most spaced, we shall have the least results from collisions, but (3) when it is fairly condensed, the effect at periastron passage (if we take the simplest case of a

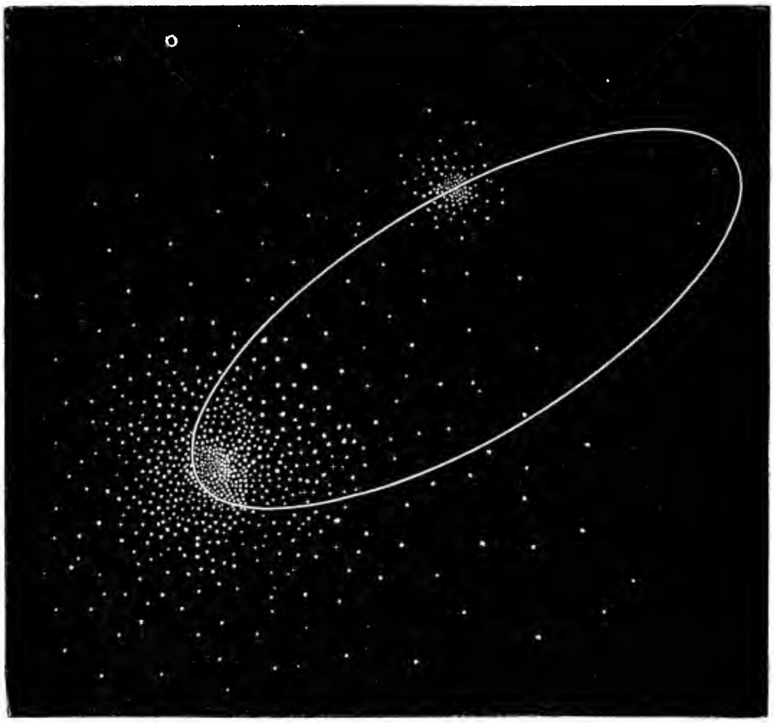


FIG. 11.—Explanation of the variability of bodies of Group II. (1) Maximum variation. The ellipse represents the orbit of the smaller swarm, which revolves round the larger. When the variation is great, the orbit of the revolving swarm is very elliptical, so that at periastron the number of collisions is enormously increased.

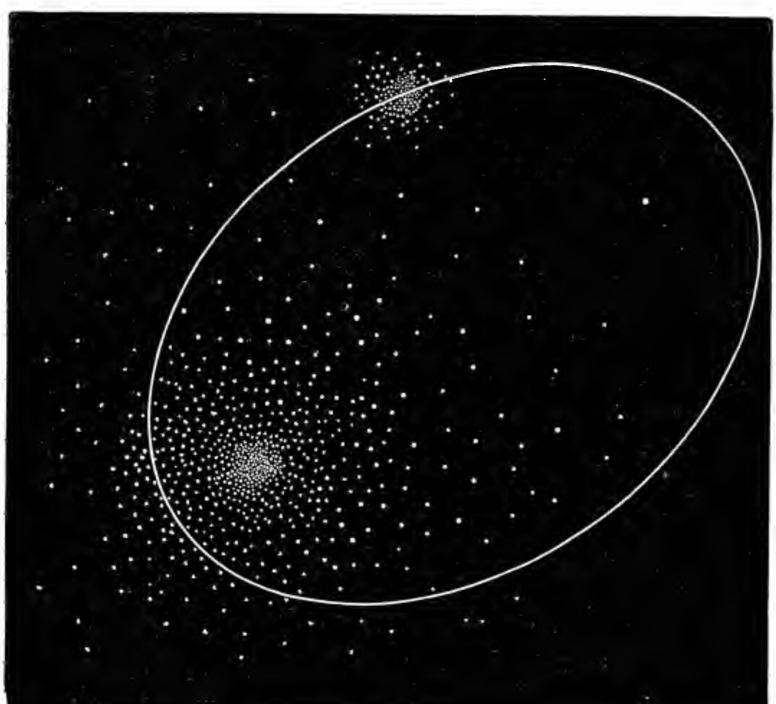


FIG. 12.—Explanation of the variability of bodies of Group II. (2) Medium variation. There will be a greater number of collisions at periastron than at other parts of the orbit, but the variation in the light will not be very great under the conditions represented, as the revolving swarm never gets very near the middle of the primary one.

double star *in posse*) will be greatest of all, because (4) condensation may ultimately bring the central swarm almost entirely within the orbit of the secondary (cometic) body, in which case no collision could happen.

In the light of what has gone before it is as easy to test these points as the former ones.

I will take them in order.

The Frequent Occurrence of Variability in Group II.

The total number of stars included in Argelander's Catalogue, which deals generally with stars down to the ninth magnitude, but in which, however, are many stars between the ninth and tenth, is 324,118. The most complete catalogue of variables (without distinction) that we have has been compiled by Mr. Gore, and published in the Proceedings of the Royal Irish Academy (series ii. vol. iv. No. 2, July 1884, pp. 150–63). I find 191 known variables are given, of which 111 are in the northern hemisphere and 80 in the southern hemisphere.

In the catalogue of *suspected* variable stars given in No. 3 of the same volume (January 1885, pp. 271–310), I find 736 stars, of which 381 are in the northern and 355 in the southern hemisphere.

Taking, then, those in the northern hemisphere, both known and suspected, we have the number 492.

We have then as a rough estimate for the northern heavens one variable to 659 stars taken generally.

The number of objects of Group II. observed by Dunér, and recorded in his admirable memoir, is 297 of these, forty-four are variable.

So that here we pass from 1 in 657 to 1 in 7.

Of the great development of variability-conditions in this group then there can be no question.

To apply the other tests above referred to, I have made a special study of the observations of each variable

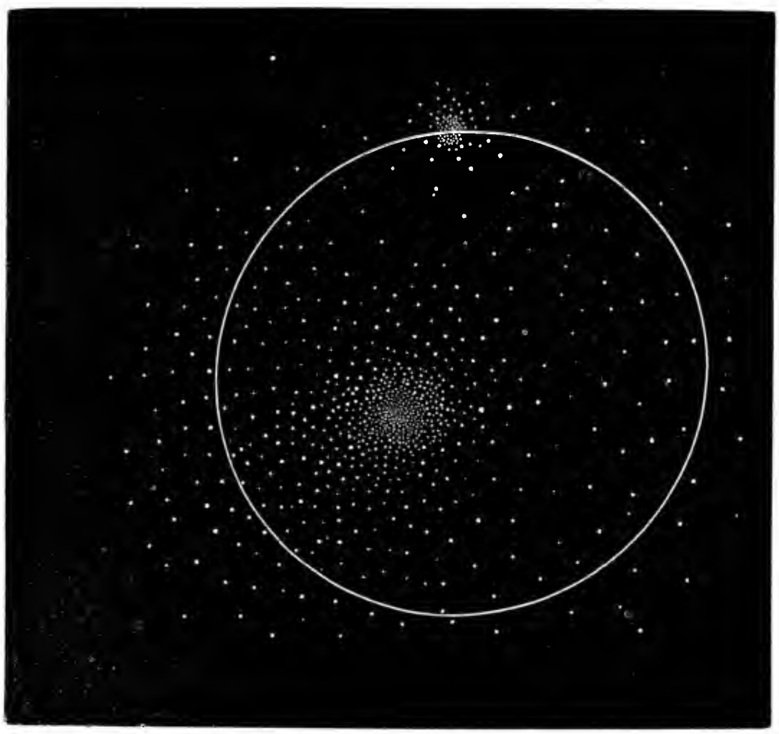


FIG. 13.—Explanation of the variability of the bodies of Group II. (3) Minimum variation. Under the conditions represented, the smaller swarm will never be entirely out of the larger one, and at periastron the number of collisions will not be very greatly increased; consequently the variation in the amount of light given out will be small.

recorded by Dunér. I find they may be grouped as follows:—

1. *All bands visible but narrow.*

No. in Dunér Cat.	Name.	Max.	Min.	Period.
269	μ Cephei ...	4?	5?	irreg.

2. *Bands well marked, but feebler in Red.*

No. in Dunér Cat.	Name.	Max.	Min.	Period.
186	W Herculis (? V)	>8	<12	290?
222	R Sagittarii	7	12	270
81	S Hydræ ...	7·8	<12	256

3. *Bands wide and strong, especially 7 and 8.*

No. in Dunér Cat.	Name.	Max.	Min.	Period.
23	T Arietis ...	8	9–10	324
37	R Tauri......	7·8	<13	326
68	S Canis Min.	7	<11	332
76	R Cancri ...	6	<11–12	360
91	R Leonis Min.	5	10	313
100	R Urs. Maj.	6	12	303
106	R Crateris...	>8	<9	160?
118	R Corvi......	7	<11–13	319
159	R Boötis ...	6	12	223
165	S Libræ......	8	12–13	190?
170	R Serpentis.	5·6	<11	358
181	U Herculis..	6·7	11–12	408
192	S Herculis...	6	12	303
195	R Ophiuchi.	7·8	12	302

4. *All bands markedly wide and strong.*

No. in Dunér Cat.	Name.	Max.	Min.	Period.	
18	*o* Ceti	2–5	8–9	(331)	
20	R Ceti ...	8	<13?	167	
29	*ρ* Persei ...	3·4	4·2	irreg.	Many lines.
92	R Leonis ...	5	10	313	
141	R Hydræ ...	4·5	4·0?	(437)	
158	V Boötis ...	—	—	—	
166	S Coronæ ...	6	12	361	
184	*g* Herculis ...	5	6	irreg.	Nearly circular orbit.
196	*α* Herculis ...	3	4	irreg.	
217	R Lyræ ...	4·3	4·6	46	
221	R Aquilæ ...	6·7	11	345	
239	*χ* Cygni ...	4	13	406	
293	R Aquarii ...	6	11	388	

5. *Bands wide, but pale.*

No. in Dunér Cat.	Name.	Max.	Min.	Period.	
3	T Cassiopeiæ	6·7	11	436	
125	T Urs. Maj.	7	12	256	
127	R Virginis...	6·7	11	146	
157	R Camel ...	8	12?	266	
231	R Cygni ...	6	13	425	
281	*β* Pegasi ...	7	12	382	
210	T Herculis	7	12	165	
4	R Androm.	5·6	<12–13	405	

6. *Bands thin and pale.*

No. in Dunér Cat.	Name.	Max.	Min.	Period.	
50	*α* Orionis ...	1	1 4	irreg.	
128	S Urs. Maj.	7·8	11	225	
187	R Draconis	6·7	11–12	247	
238	S Vulpec. ...				
261	R Vulpec....	7·8	13	137	

A glance at the above tables will show that the kind of variability presented by these objects is a very special one, and is remarkable for its great range. The light may be stated in the most general terms to vary about six magnitudes, from the sixth to the twelfth. This I think is a fair average; the small number of cases with a smaller variation I shall refer to afterwards. A variation of six magnitudes means roughly that the variable at its maximum is somewhere about 250 times brighter than at its minimum.[1]

I have already indicated that, with regard to the various origins of the variability of stars which have been suggested, those which have been always most in vogue consider the maximum luminosity of the star as the normal one; and indeed with regard to the Algol type of stars of short periods, which obviously are not here in question, there can be no reasonable doubt, that the eclipse explanation is a valid one; but in cases such as we are now considering, when we may say that the ordinary period is a year, this, explanation is as much out of place on account of period, as are such suggested causes as stellar rotation and varying amount of spotted area on a stellar surface, on account of range.

[1] Obtained by the formula $L_m = (2\cdot512)^n \cdot L_{m+n}$. For differences of 5, 6, 7 and 8 mag. we get

$$L_m = 100\cdot02 \cdot L_{m+5}$$
$$= 251\cdot24 \cdot L_{m+6}$$
$$= 631\cdot11 \cdot L_{m+7}$$
$$= 1585\cdot35 \cdot L_{m+8}$$

L_m = light of a star of magnitude m.
L_{m+n} = ,, ,, n magnitudes fainter

We are driven, then, to consider a condition of things in which the minimum represents the constant condition, and the maximum a condition imposed by some cause which produces an excess of light; so far as I know the only explanation on such a basis as this that has been previously offered is the one we owe to Newton, who suggested such stellar variability as that we are now considering was due to conflagrations brought about at the maximum by the appulse of comets.

How the Difficulty of Regular Variability on Newton's View is got over in mine.

It will have been noticed that the suggestion put forward by myself is obviously very near akin to the one put forward by Newton, and no doubt his would have been more thoroughly considered than it has been hitherto, if for a moment the true nature of the special class of bodies we are now considering had been *en évidence*. We know that at their minimum they put on a special appearance of their own in that haziness to which I have before referred as having been observed by Mr. Hind. My researches show that they are probably nebulous, if indeed they are not all of them planetary nebulæ in a further stage of condensation, and such a disturbance as the one I have suggested would be certain to be competent to increase the luminous radiations of such a congeries to the extent indicated.

Some writers have objected to Newton's hypothesis on the ground that such a conflagration as he pictured could not occur periodically, but this objection I imagine chiefly depended upon the idea that the conflagration brought about by one impact of this kind would be quite sufficient to destroy one or both bodies, and thus put an end to any possibilities of rhythmically recurrent action. It was understood that the body conflagrated was solid like our earth. However valid this objection might be as urged against Newton's view, it cannot apply to mine, because in such a swarm as I have suggested, an increase of light to the extent required might easily be produced by the incandescence of a few hundred tons of meteorites.

I have already referred to the fact that the initial species of the stars we are now considering have spectra almost cometary, and this leads us to the view that we may have among them in some cases swarms with double nuclei—incipient double stars, a smaller swarm revolving round the larger condensation, or rather round their common centre of gravity. In such a condition of things as this, it is obvious that, as before stated, in the swarms having a mean condensation this action is the more likely to take place, for the reason that the more the outliers of the major swarm are drawn in, the more likely is the orbit of the smaller one to pass clear. The tables show that this view is entirely consistent with the facts observed, for the greater number of instances of variability occur in the case of those stars in which, on other grounds, mean spacing seems probable.

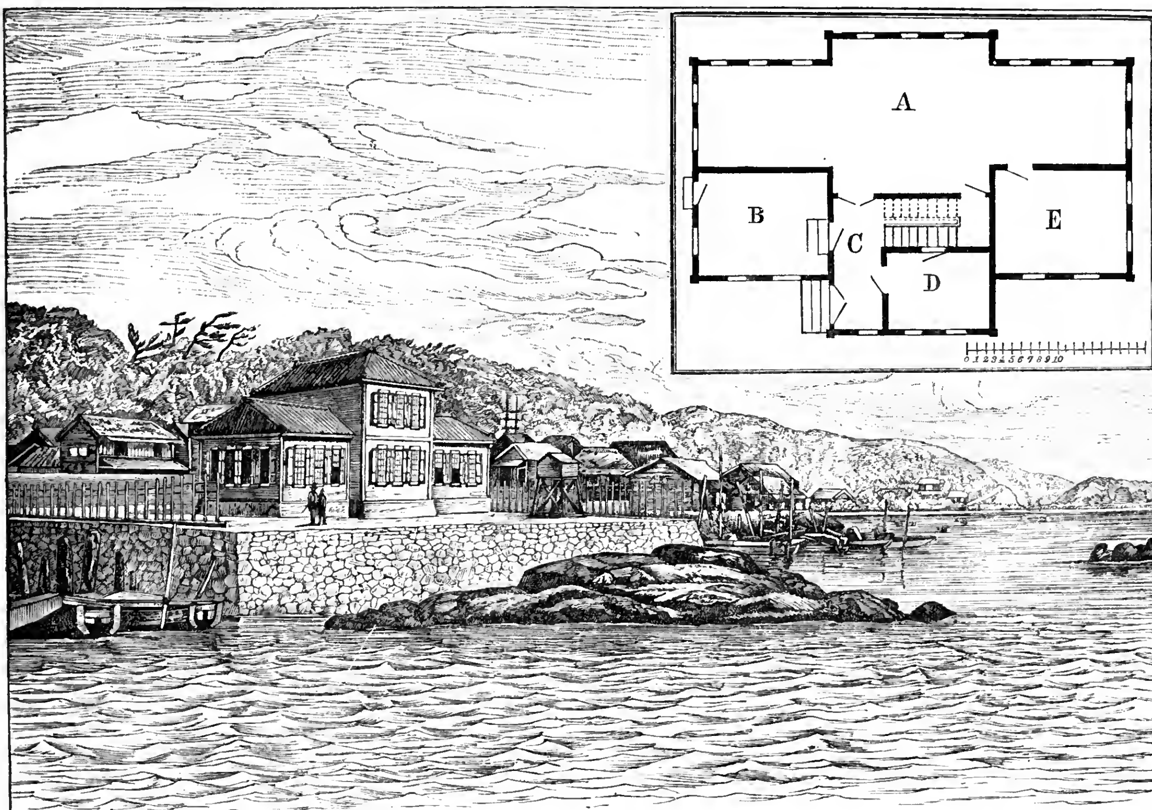
The Cases of Small Range.

So far, to account for the greatest difference in luminosity at periastron passage, we have supposed the minor swarm to be only involved in the larger one during a part of its revolution, but we can easily conceive a condition of things in which its orbit is so nearly circular that it is almost entirely involved in the larger swarm. Under these conditions, collisions would occur in every part of the orbit, and they would only be more numerous at the periastron in the more condensed central part of the swarm, and it is to this that I ascribe the origin of the phenomena in those objects—a very small number—in which the variation of light is very far below the normal range, one or two magnitudes instead of six or seven. Of course, if we imagine two subsidiary swarms, the kind of variability displayed by such objects as *β* Lyræ is easily explained.

NATURAL SCIENCE IN JAPAN.

WITH the close of our eventful Jubilee year there was completed the first volume of a new journal of science which is destined to play a very important part in the advance of knowledge. We refer to the Journal of the College of Science of the Imperial University of Japan, already noted in these pages.

This periodical is issued under the joint editorship of four professors in the College whence it originated. These gentlemen, one only of whom is an Englishman, constitute a publishing committee: they have adopted the wise plan of issuing all communications on all subjects recognized within the one cover, and under their supervision there have already appeared a series of original papers of considerable interest, so far at least as those biological are concerned. The work is being well done: authors, editors, publishers, and craftsmen appear to be working harmoniously in the production of a journal which, while it reflects the utmost credit on all, leaves nothing to be desired. Twenty-one original monographs have been set up, three of them in German, the rest in English. Of these five are biological, while six are devoted to physics, four to chemistry, three to seismology, and two to geology proper. It is to the first-named that we wish now to refer. The first paper published deals with the life-history of a parasite (*Ugimya sericaria*) which works fearful havoc among the silkworms in Japan: this monograph is in itself interesting, apart from its intrinsic merit, as showing that our Eastern friends are fully alive to the so-called practical application of their work. This and other valuable papers which we might name testify most satisfactorily to the thoroughness of, at any rate, one side of the undertaking; others there are which show that



The Marine Biological Station of the Imperial University at Misaki.

these investigators are fully prepared to face some of the most formidable problems now exercising the mind of the zoologist, and in dealing with such problems they display a diligent attention and cautious generalization, such as are occasionally looked for in vain in writings nearer home. If this excellent beginning is, in these respects, indicative of that which is to follow, only results of the greatest value can ensue.

Of the zoological communications two are exceptional—we refer to those contributed by Prof. K. Mitsukuri, of the Imperial University, Tokio. One of these, on the germinal layers in Chelonia (produced in conjunction with his assistant, Ishikawa), has previously appeared in our own *Journal of Microscopical Science*. The other is deserving of especial comment, for it brings tidings of the establishment of a marine biological station of the Imperial University, at Misaki.

Misaki is a fishing settlement to the west of the Bay of Tokio, easily accessible, we are told, from Tokio or Yokohama in a day. Its waters have a direct interest for Western zoologists, in the fact that they have given birth to most of those museum specimens of Hyalonema, with which the skilful Japanese so long duped the rest of the world. Geographically, the relations of Japan to Asia may be appropriately compared with those of Britain to Europe: in their greater climatic variations, however, the Easterns have an advantage, if only by way of variety in the fauna and flora thereby induced. Again, Misaki, says Prof. Mitsukuri, has "long been a favourite collecting ground for naturalists; almost every group of animals is represented in this region in more or less abundance," and he gives it as his opinion that zoologists have by no means "become acquainted with even a small part of all the interesting animals to be found." When we reflect upon this, mindful of the climatic features of the district, and in view of the enumeration given of known inhabitants of its waters, great expectations are conjured up, and the importance of the enterprise upon which our friends have embarked becomes self-evident.

The station has been founded by the Department of Education and the authorities of the Imperial University, as a special adjunct to the biological laboratories of the latter. As it is fair to assume that the governmental body will, like all others, expect "something practical" for its money, we anticipate that attention will early be given to questions of economic importance. The Japanese have a fishing population of more than 1,500,000 active workers, while it is computed that 36,000,000 persons, in all, are more or less dependent upon fish as food. When, in view of the total area and population of our own islands as compared with those of Japan, it is remembered that our own fishing population numbers little over 540,000, it becomes needless to point out that the Japanese are *par excellence* a fishing folk. They moreover appear to possess an ancient but limited literature on the subject.

The establishment, by the Japanese, of this and other similar institutions has been necessitated by the adoption of the products of Western civilization, almost, it would seem, in return for that "devout and learned admiration" so long accorded them by the Western nations. Rapid indeed has been their progress under influences which are bringing their wares into open competition with Western markets, and who shall say but that we proud Europeans may not yet be, perforce, to no small extent dependent upon them for edible produce?

The founding of this marine station is, biologically, a sign of the times. More than this, however. It is a mo ement upon which, in the long run, the intellectual as well as the commercial prosperity of a large section of the community must depend; for in the spread of that true science which seeks to unravel the knowledge of causes, there now lies the only sound basis for national prosperity. Prof. Mitsukuri's association with the undertaking is, in itself, a guarantee that these interests will be upheld. His earlier work was executed under the guidance of, and in fellowship with, American subjects whose names will be for ever memorable in the history of marine zoology: his association with them and with the illustrious Balfour, and his acknowledged indebtedness to Dohrn, are, in themselves, auguries of success. We note with much satisfaction that "arrangements will be made by which students in the biological course of the University will be required to pass at least one term in the station": workers will be thus assured, and we tender them our sincere congratulations and hearty good wishes for a prosperous development of their enterprise. It must not be forgotten that the Japanese waters have lately yielded us the interesting Chlamydoselache, and it would be a most interesting circumstance should the far-famed Hyalonema, to which Prof. Mitsukuri so frequently reverts in his article quoted, receive final consideration at the hands of his countrymen.

The following is a brief description of the station itself, extracted from the original article. "The building is of plain wood, and one story high, except in the middle part, which has a second floor. The main laboratory-room (A), which occupies the whole sea-front, is 48 feet long, 12 feet wide at the two ends, and 18 feet in the middle, and is able to accommodate about ten workers. A number of small aquaria for the use of investigators will be placed in this room. Of the rooms at the back of the main laboratory, one (B) has a cement floor and is for assorting and preserving specimens brought in from the sea. Another (E) is to be used as the library-room, and a third (C) as the store-room. The second floor over the central part of the building is able to give sleeping accommodation for a few persons. From a tank placed outside the building, fresh sea-water is carried into the main laboratory-room and the assorting-room, and is delivered out of many facets." G. B. H.

THE AURORA IN SPITZBERGEN.[1]

THE best observations hitherto made on the aurora borealis are those made at Bossekop, by Bravais, during the expedition of the French corvette *Le Recherche*, 1838–40. Bossekop is also situated in the maximum zone of the auroras, on the coast of Northern Norway. Considering that Spitzbergen lies a little *north* of the same zone, and nearly on the same meridian as Bossekop, it was resolved that the observations of auroras should be made with the greatest possible care at the Swedish International Polar Station at Spitzbergen in 1882–83. This work was confided to Mr. Carlheim-Gyllenskiöld, and the auroral observations are the most complete that have been made by any of the international expeditions during that year. The results are now printed, and form a large quarto volume of 409 pages, with a great number of tables, illustrations, and figures. The results confirm and enlarge those of Bravais, and of other observers of this brilliant phenomenon.

(1) The first question is the determination of the mean co-ordinates of the auroral arch. A mean of 371 measurements gave the azimuth of the culminating point or summit of the auroral arch in S. 24° 12′ E. As early as 1834, Argelander, in Åbo, Finland, found that the azimuth of the culminating point of the auroral arch differs about 10° from the magnetic meridian. At Bossekop the magnetic declination was N. 10° 8′ W., and the declination of the culminating point of the auroral arch N. 22° 4′ W., the anomaly being, of course, about 11° W. The magnetic declination at Cape Thordsen was found to be N. 12° 45′ W., and of course the auroral anomaly from the magnetic meridian was 11° 27′ W.

(2) Eighty-seven measures on the position of the corona borealis were made, and the position of the centre of the corona was found nearly in the magnetic zenith, and *not* in the same vertical as the highest point of the arch. The means are:—

Position of the centre of the corona	H = 79° 55′ ...	Az. = S. 7° 12′ E.
Position of the magnetic zenith	H = 80 35 ...	Az. = S. 12 4 E.
Position of the culminating point of the arch	H = — ...	Az. = S. 24 12 E.

This confirms the measurements made during the past century by Wilcke, Mairan, and others.

(3) The breadth of the auroral arches varies with their elevation above the horizon. The arches consist of rays running in the direction of the breadth of the arch, and converging towards the magnetic zenith. Thus they form a long fringe of rays parallel to the dipping-needle, suspended, like a curtain, from east to west, but with a small extent of breadth from north to south. If this curtain of rays moves from the horizon to the zenith, the breadth varies according to the laws of perspective. The formula gives the greatest breadth at a height of 45°. In the neighbourhood of the zenith the arches are very narrow, stretching as a luminous band across the heavens.

(4) Besides the arches and rays, the auroral light sometimes formed a true spherical zone parallel with the earth's surface, thus floating in space as a horizontal layer of light, often crossed by several arches. This form is seldom to be seen in lower latitudes. These auroral zones were apparently much broader in the zenith than at their extremities nearer to the horizon. When such an auroral zone was lying wholly over the heavens, with the excep-

[1] "Observations faites au Cap Thordsen, Spitzberg, par l'Expédition Suédoise." Tome II. (1) Aurores boréales. Par Carlheim-Gyllenskiöld.

tion of a low segment near the horizon, a dark segment was produced by contrast. Sometimes the luminous zone was broken, and then dark spots or irregular spaces were produced in the same way. These dark spaces were frequently seen tinted with a faint rosy light.

(5) The movement of the arches is ordinarily reported to be from north to south, at places situated to the south of the maximum zone, and, from the opposite direction, at places within the maximum zone. Thus, at different stations between the latitude of Rome and the latitude of Bossekop, 69·6 per cent. of the auroral arches have moved from the north; at Mossel Bay, Franz-Josef Land, and Discovery Bay, on the contrary, 62·5 per cent. have moved from the south. At Cape Thordsen it was of course expected that the most part of the auroral arches would move from the south. Yet this was not the case. On the contrary, 57·6 per cent. moved from the north. The movements were, of course, almost the same in both directions.

(6) The anomalous forms of arches were very frequent, and were made a matter of accurate investigation. Sometimes an auroral arch presents the form of a sinuous band, or resembles a brilliant curtain with deep folds. At other times the arches appeared as regular spirals. Seen from the outside of the earth, or from above, the spirals were almost all contorted in a direction contrary to the motion of the hands of a watch, and the undulations folded as an S. The motion was, in 80 per cent., from west to east. The folds of the auroral draperies had very different dimensions on different occasions. Sometimes a regular arch showed only a slight undulation; at other times, only a part of an immense auroral drapery was seen above the horizon, as a pseudo-arch.

(7) Often, waves of light are running along the arches, and then the rays or beams are apparently in vivid motion. This appearance of the aurora is known in England as "the merry dancers." In 103 cases the waves were running from west to east, and in 101 cases from east to west. The mean angular velocity per second was 38′·6. For a mean vertical height of the aurora of 100 kilometres above the earth's surface, or 222 kilometres from the observer's eye, this gives the immense velocity of about 2·5 kilometres per second. The light of the aurora was often suddenly changing as to the distribution and intensity of light, but the geometrical form of the whole phenomenon was only slowly varying. The rays were sometimes observed to have a slow proper motion from west to east, or *vice versâ*.

(8) As to the classification of the auroral forms, the author rejects that of Weyprecht. The different forms of the aurora in the classification of Weyprecht are, in fact, only different views or projections, as, for instance, the forms III. = beams or rays, and IV. = corona. The corona results, according to the rules of perspective, when a large number of separate beams parallel to each other and to the direction of the dipping-needle seem to converge to one point, viz. the magnetic zenith. A regular and fully-developed arch consists, as we have said before, of a long fringe of rays, and so on. The author considers only two different forms of auroral light, viz. zones, or horizontal layers of light; and arches, composed more or less of distinct rays parallel to the dipping-needle. The arches present themselves in four different conditions: (1) arch, or a regular band; (2) band, or drapery; (3) spiral; and (4) pseudo-arch.

(9) The light of the aurora is, according to the author, of two kinds: (1) the yellow light, entirely monochromatic, and showing in the spectroscope the well-known yellow line of Ångström; (2) the crimson or violet light, resolved in the spectroscope into several rays and bands, spread over all parts of the spectrum. In the following table we give (I.) the lines observed by the author, (II.) the lines observed by several authors before the year 1884, and (III.) the spectrum of lightning, according to the observations of Herschel, Vogel, Schuster, and the author. The unity for wave-length is, as usual, the 0·0000001 of the millimetre.

I.	II.	III.
6306 ± 7·3	6294 ± 6·4	6300
5776 ± 3·0	5776 ± 3·0	—
5664 ± 3·0	5664 ± 3·0	5685
5568 ± 1·6	5570 ± 0·9	—
5353 ± 3·0	5353 ± 3·3	5338
5264 ± 2·5	5280 ± 1·8	5260
5228 ± 2·7	5226 ± 3·2	—
5001 ± 4·2	5003 ± 2·7	5004
4837 ± 10·7	4862 ± 1·5	4860
4707 ± 5·1	4702 ± 2·9	—
4642 ± 3·3	4636 ± 2·4	4632
4236 ± 6·7	4286 ± 4·4	—

There were twelve other extremely faint auroral rays to be seen occasionally, but their position could not be exactly observed.

As to the further discussion of the different auroral spectra and their supposed connection with different auroral forms, we must refer to the original paper.

(10) No sound was ever heard from the auroral light. The feeble rustling noise sometimes heard was observed to come from the loose agile surface-layer of snow driven to and fro by the lightest wind over the underlying layers. Nor was a "smell of sulphur" observed.

(11) As to the height of the aurora, it may first be mentioned that the aurora was never seen to descend below the mountains or the lower clouds. Only two or three times it is possible that the light was seen below the upper clouds. Yet sometimes the auroral light was seen to be reflected from the surface of the snow. Direct measures of the parallax from the end of a short base (573 metres), by means of auroral theodolites of Mohn's construction, gave an average height of 55·1 kilometres; from observations of the corresponding amplitudes and heights of the arches, according to Bravais' method, 57·7 kilometres; and by several other observations and calculations, about 60 kilometres was found to be the probable mean height of the aurora.

(12) As to the annual and diurnal periods of the aurora, no annual variation in the frequency could be proved. The apparent daily period gave a maximum at 8h. 50m. Göttingen time, or 9h. 13m. local time, in the evening; and a minimum at exactly the same hour in the morning. This apparent period must be corrected for the influence of the quantity of clouds and for the influence of the twilight. If F represents the apparent frequency of the aurora, and Q the quantity of clouds in tenth parts of the whole sky, there was found $F = 1 - 0·0730\,Q$, in taking for unity the apparent frequency when the heavens were totally clear.

Further, the apparent frequency when the sun was 10° 47′ below the horizon was the half of the true frequency, and the influence of the sun's light was sensible as far as to a depth of the sun of 17° 45′ below the horizon. Once only the aurora was seen when the sun was not more than 5° 25′ below the horizon.

Taking into account these sources of error, the true daily range has a maximum at 3h. 3m. p.m., and a minimum at 8h. 3m. a.m. local time.

Finally, there was also a well-marked daily range in the form of the aurora. The most brilliant phase of the phenomenon occurred at 4h. p.m.; the aurora then appeared as a complete regular arch. On the other hand, the minimum brilliancy took place at 9h. a.m.; the arches then were resolved into whirling fragments.

Upsala, April. H. HILDEBRANDSSON.

NOTES.

THE general arrangements for the Bath meeting of the British Association have now been made. The first meeting will be held on Wednesday, September 5, at 8 p.m. precisely, when

Sir H. E. Roscoe will resign the chair, and Sir F. J. Bramwell, President-elect, will assume the Presidency, and deliver an address. On Thursday evening, September 6, at 8 p.m., there will be a *soirée;* on Friday evening, September 7, at 8.30 p.m., a discourse on "The Electrical Transmission of Power," by Prof. W. E. Ayrton; on Monday evening, September 10, at 8.30 p.m., a discourse on "The Foundation Stones of the Earth's Crust," by Prof. T. G. Bonney; on Tuesday evening, September 11, at 8 p.m., a *soirée*. On Wednesday evening, September 12, the concluding general meeting will be held at 2.30 p.m. Excursions to places of interest in the neighbourhood of Bath will be made on the afternoon of Saturday, September 8, and on Thursday, September 13.

THE fourth session of the International Geological Congress will be opened on Monday evening, September 17, and will last during the whole of the week. The meetings will be held in the rooms of the University of London, Burlington Gardens. The Honorary President of the Congress will be Prof. Huxley; the President, Prof. Prestwich; the Vice-Presidents, the Director-General of the Geological Survey, the President of the Geological Society, and Prof. McK. Hughes; Treasurer, Mr. F. W. Rudler; and General Secretaries, Mr. J. W. Hulke and Mr. W. Topley. Up to the present date 395 geologists have signified their intention of being present. Of these 210 are British, and 185 foreign. The number of countries represented is 22.

THE Linnean Society holds its centenary celebration to-day. The following is the programme of the proceedings:—At 2.30 p.m. the President will receive the visitors. At 3 p.m. the President will take the chair. After introductory remarks by the President, and the formal business of the anniversary meeting, the Treasurer will lay before the meeting an account of the financial condition of the Society during the last century; the Secretaries will lay before the meeting a history of the Linnean books, herbarium, and other collections; the President will deliver the annual address. The following Eulogia will be pronounced: On Linnæus, by Prof. Thöre Fries, the present occupant of the Chair of Botany at Upsala; on Robert Brown, by Sir Joseph Hooker; on Charles Darwin, by Prof. Flower; on George Bentham, by Mr. W. T. Thiselton Dyer. The Linnean Gold Medal, instituted by the Society on the occasion of its centenary, will be presented to Sir Joseph Hooker (botanist), and Sir Richard Owen (zoologist). (In subsequent years the presentation will be alternately to a botanist and zoologist.) At 6.30 p.m. the annual dinner will be held at the Hotel Victoria, Northumberland Avenue, the President in the chair. To-morrow (May 25th), at 8.30 p.m., the President and Officers will hold a reception of the members and visitors in the Rooms of the Society, when the Linnean collections and relics will be exhibited.

THE late Mr. Cooper Foster, of Grosvenor Street, for many years senior surgeon to Guy's Hospital, was famous among horticulturists as a collector and grower of Hymenophyllums, Trichomanes, and Todias, popularly known as Filmy Ferns. With very few exceptions, the whole of these plants are extremely difficult to cultivate. The conditions under which they grow naturally are not easily imitated. Mr. Foster, however, contrived to keep a very rich collection of species, many of them unknown in gardens except at Kew, where the collection of Filmy Ferns is perhaps unique; and even Kew did not possess several kinds which Mr. Foster possessed. When it is remembered that these extremely delicate plants were cultivated in one or two small greenhouses at the back of a house in Grosvenor Street, Mr. Foster's success appears still more remarkable. After his death the Filmy Ferns were removed to his son's residence at Binfield, Berks. Recently, however, Mrs. Foster offered the whole collection to Kew, and it has lately been transferred to these Gardens, special accommodation having been provided for it in the house (No. 3) where the bulk of the Kew collection is grown. Among the most noteworthy of the plants comprised in the Cooper Foster collection are *Trichomanes reniforme*, a magnificent specimen a yard across, bearing hundreds of fine healthy leaves; *T. parvulum*, which has a compact cushion-like mass of tiny palmate leaves; *T. alabamense*, *Hymenophyllum æruginosum*, *H. chiloense*, *H. cruentum*, *H. flexuosum*, *H. Fosterianum*, *H. pectinatum*, *H. pulcherrimum*, and some grand masses of *H. demissum*. This magnificent gift to the national gardens at Kew will no doubt receive the appreciation from the public which its intrinsic beauty, scientific interest, and actual pecuniary value deserve.

MRS. EMMA W. HAYDEN has given to the Academy of Natural Sciences of Philadelphia in trust the sum of $2500.00, to be known as the Hayden Memorial Geological Fund, in commemoration of her husband, the late Prof. Ferdinand V. Hayden. According to the terms of the trust, a bronze medal and the balance of the interest arising from the fund are to be awarded annually for the best publication, exploration, discovery, or research in the sciences of geology and palæontology, or in such particular branches thereof as may be designated. The award and all matters connected therewith are to be determined by a Committee, to be selected in an appropriate manner by the Academy. The recognition is not to be confined to American naturalists.

ACCORDING to the *Colonies and India*, the appointment of Superintendent of the Botanical Gardens, Singapore, has become vacant owing to the death of Mr. Cautley in Tasmania.

M. HERVÉ MANGON, Member of the Paris Academy of Sciences, and President of the French Meteorological Councils died on the 16th inst., at the age of sixty-seven. He was Minister of Agriculture in the Brisson Cabinet, and was a high authority on drainage and agricultural improvements.

THE Pilot Chart of the North Atlantic Ocean for May show, that, generally, fine weather prevailed over that ocean during April. Storms accompanied by electric phenomena occurred about once a week north of the 40th parallel. A cyclonic storm of great strength was generated on April 15 in about 35° N. and 60° W., moving across the Banks from the 16th to the 18th, in which the wind reached force 11. There was also a gale of considerable strength to the north-eastward of the Azores during the second week of April, and a "norther" was felt in the western part of the Gulf of Mexico on the 13th. Considerable fog was met with off the Grand Banks, and southwards. The amount of ice encountered was unusually small, and was confined for the most part to the south-east coast of Newfoundland. Although it has been delayed in its southward movement by the unusual prevalence of south-easterly winds, it is now liable to appear in quantity, and to constitute a source of danger for several months. Careful observations of the Gulf Stream and the equatorial current are now being made at certain points by the United States steamer *Blake*.

A SODIUM salt of zincic acid has at last been obtained in the crystalline state by Messrs. Comey and Loring Jackson, of Harvard University (*Berichte*, 1888, 1589). Every analyst is aware that zinc hydrate is soluble in potash or soda, and although it has been presumed that zincates of the alkalies or compounds of the alkaline oxides with zinc oxide are formed under these circumstances by replacement of the hydrogen of the hydrate by potassium or sodium, no such compounds have hitherto been isolated. Messrs. Comey and Jackson, however, find that when a concentrated solution of zinc or zinc oxide in soda is shaken with alcohol the mixture separates on standing into two layers a heavier aqueous and a lighter alcoholic layer. When the treatment of the heavier layer with alcohol is repeated once or twice, it eventually solidifies to a mass of white crystals which melt below 100° C. Moreover, on allowing the alcoholic washings to stand, long brilliant white needles, often more than a centimetre in length, are deposited. These latter crystals differ very markedly in melting-point from those obtained from the aqueous portion, as they remain unfused even at 300°. They were finally purified and subjected to analysis, the results of which point very clearly to the composition $2NaHZnO_2 + 7H_2O$, or $2Zn(OH)(ONa) + 7H_2O$. Hence this new salt may be regarded as hydrogen sodium zincate. It is soluble in water and alcohol holding soda in solution, but is decomposed both by pure water and alcohol. The crystals obtained from the aqueous solution above mentioned appear to differ from those just described only in containing more water of crystallization, the amount of which has not yet been fixed with certainty. The fact that zinc oxide behaves so negatively towards the more positive alkalies, playing as it evidently does the *rôle* of an acid, is now happily a proved one, and it is to be hoped that the American chemists will continue their researches until they have been as fortunate in preparing the normal salt of zincic acid.

AT the last meeting of the Asiatic Society of Japan, the Rev. J. Batchelor read a paper on "Some Specimens of Aino Folk-Lore." There were seven of these taken down as they were sung, chanted, or recited by the Aino bard or story-teller. After telling these stories, Mr. Batchelor observed that among the Ainos there are still prophets and prophetesses, but they limit their powers now to telling the cause of illness, prescribing medicine, using charms, and the like. A person when prophesying is supposed to sleep or otherwise lose consciousness, and to become, so to speak, the mouthpiece of the gods. The prophet is not even supposed to know what he himself utters, and often listeners cannot understand the meaning of the utterances. The burden of the prophecy sometimes comes out in jerks, but more often in a kind of sing-song monotone. Mr. Batchelor described one scene of Aino prophesying at which he was present. "Absolute silence reigned around, old men with gray beards sat with eyes full of tears, in rapt attention; the prophet himself was apparently quite carried away with his subject; he trembled, perspired profusely, and beat himself with his hands. At length he finished exhausted, and as he opened his eyes for a moment, they shone with a wild light." During the discussion which followed, it was stated that the author of the paper was engaged in the preparation of an Aino dictionary, for which seven or eight thousand words had already been collected. "Such a dictionary," said Prof. Chamberlain, "would in all likelihood be a kind of tomb in which the rapidly dying language would remain enshrined for ages. Even now it was striking to observe how all except the oldest men and women were really bi-lingual, speaking Japanese as easily as Aino."

MR. BRUCE FOOTE, Superintendent of the Geological Survey of India, lately contributed to the Asiatic Society of Bengal some most interesting "notes" on recent Neolithic and Palæolithic finds in Southern India. These notes have now been reprinted from the Society's Journal. One of the facts to which he calls attention is that "the old Stone-folk" of the Bellary-Anantapur country, where great numbers of Neolithic settlements have been found, selected granite-gneiss hills as the sites of their settlements. Four considerations may, he thinks, have influenced them in this choice:—(1) The more perfect isolation of the granite-gneiss hills, which mostly rise singly out of the plains, or, if in clusters, are yet individually detached, and therefore more suitable for defence than posts on continuous ridges, such as are generally formed by the schistose rocks. Some of the granite-gneiss hills are nearly perfectly castellated by the disposition of the rock-masses. (2) Rock-shelters of great efficiency and comfortable terraces are to be found in numbers on many of the granitoid hills, but hardly ever on the schistose hills. (3) The collection of rain water and its storage would, from the nature of the ground, be much easier on the average granitoid rock than on the average schistose hill. (4) The schistose hills are, in very many cases, generally, in fact, surrounded by a heavy and broad talus most detrimental to easy agricultural work. The granitoid hills, on the contrary, form, as a rule, no great talus, but rise up straight out of the great cotton-soil plains, so that the Neolithic field labourers could have been quite close to places of refuge in cases of attack from other tribes, and yet have been able to carry on their agricultural work.

AT the last meeting of the Archæological Society of Sweden, Herr N. F. Sander read a paper on the wholly or partly undeciphered runic inscriptions in Sweden, which he divided into three classes: (1) those composed of ordinary runic letters, but in which the runic "staf" or sign I, when signifying *i* or *e* had purposely been left out, in one inscription even twenty-five times; (2) the conventional runic signs, which were really runic cipher; and (3) the so-called Sudermania "qvist" (sprig or faggot) runes, as well as the "ice" runes. Here the secret lay in the circumstance that the three "sets" of letters had been purposely misplaced, so that in the inscriptions the third set (*h, b, l, m, r*) came first; first set (*f, u, t, h, o, r, k*) second; and second set (*u, i, a, s*) third. Referring to seven of the first-named order of inscriptions which had recently been deciphered, Herr Sander stated that five of them, all situated in the province of Upland, had the same contents, and contained some curious objurgations. In four of them appeared the word *Pim* or *Piment* (*i.e.* a strong drink composed of wine, honey, and spice), which, as well as *Klaret*, was mentioned in the *Saga* of Rollo the Ganger and the Normans. All these inscriptions were referred to the close of the pagan age. One of them read as follows: "Reksessr, only Thynne's son (son of), assigned (to himself)—*i.e.* wedded—asa-Askra; (she) is daughter of Thynne-Signil and the giant." At the mouth of the River Åby, close to which this stone was found, is a little island called Thynne or Tönnö.

IN an interesting article in a recent number of the *Naturwissenschaftliche Wochenschrift*, Prof. Nehring discusses the question as to the origin of the dog. He expresses his belief that it is descended from various still-surviving species of wolves and jackals. The taming of jackals, he says, presents no particular difficulty, and many attempts to domesticate wolves have been successfully made in recent times. Herr Rongé has so completely tamed a young wolf that it follows him exactly as a dog might do.

THE United States Consul at Auckland, in a recent report, says that rabbits have so eaten out the ranges in New Zealand, that the capacity for maintaining sheep has greatly lessened, and the flocks have fallen off in numbers. At the Stock Conference of 1886 it was stated that rabbits reduced by a third the feeding capacity of land, and that the weight of fleeces had decreased by 1 lb. to 1½ lb. each. The number of lambs decreased from 30 to 40 per cent., while the death rate increased from 3 to 13 per cent. Since 1882, when the Rabbit Act became law, Government has expended £7000 on Crown lands alone, and it is estimated that during the last eight years private persons have spent £2,400,000 in extirpating rabbits. The methods generally in favour are fencing, poisoned grain (generally phosphorized oats), and ferrets, weasels, and stoats.

THE Canadian Minister of Agriculture in his report for the past year refers to various measures taken by the Government for the advance of scientific agriculture in the Dominion. Five experimental farms in various parts of the country were provided by the Legislature, a botanist and entomologist were appointed, and a large number of experiments to ascertain the roots and cereals most suited for the circumstances of Canada—especially its short summer—were carried out under scientific supervision.

A RICH gold-field has been discovered between the two rivers, Lava and Papanahoni, in Surinam. It is an open question whether this district of 20,000–25,000 square kilometres belongs to France or Holland. M. Condreau, the French traveller, who has been closely investigating the district, considers that it will be as productive as the gold-fields of Australia and California.

THE University of Christiania has despatched a zoologist, Herr J. Jversen, to Sumatra, for the purpose of collecting natural history objects for that institution.

A SUM of £550 has been granted by the Danish Government towards the expenses of publishing the zoological and botanical results of Lieut. Hovgaard's Arctic expedition in the *Dijmphna* in 1880–81. The work will soon be issued.

IN addition to a sum already granted, the Norwegian Government has given £300 towards the publication of Prof. Friis's ethnographical chart of the provinces of Tromsö and Finmarken.

THE number of visitors to the Natural History Museum, recorded by aid of Benton's "O" register up to 6 o'clock on Whit Monday, was 4567, and the Museum was open for two hours longer. This number compares with 6010 and 6589 admissions on the Whit Mondays of the two preceling years. During the week ending Saturday last, 149,583 persons visited the Museum in the present year, being an increase of 8000 on last year.

THE honorary degree of LL.D. has been conferred by the McGill University, Montreal, upon Prof. W. Fream, B.Sc. Lond., of the College of Agriculture, Downton, Salisbury, in recognition of his contributions to agricultural science and of his services to Canadian agriculture.

MUCH interest has been excited by the successful transplantation of nerve from a rabbit to man. The operation was performed by Dr. Gersung, of Vienna, and the patient was Dr. von Fleischl, Professor of Physiology in the University of that city. Sixteen years ago Dr. von Fleischl accidentally wounded himself while conducting a *post-mortem* examination, and the consequent severe inflammatinn of his right arm and hand led ultimately to the loss of the terminal joint of his thumb. The end of the stump having become painful, amputation somewhat further back was performed. This was followed by the formation of "neuromata." In the hope of obtaining relief he underwent several fruitless operations. Ultimately, Dr. Gersung suggested that the nerves might be repaired, and the missing portions replaced, by means of fresh nerve taken from a rabbit. The *Times* of Tuesday gives the following account of the operation:—"Just as there is nothing special in any individual human nerve, and as any one of them would be capable of discharging the duty of any other, so, it may be assumed, there is no difference between the endowments of the nerves of man and those of the lower animals, which fulfil identical functions in an identical manner. It was, therefore, inherently probable that the nerve of an animal, if a piece could be obtained of the proper size and length, and if transplantation and union could be successfully effected, would suffice to make good any loss of nerve in man; and, in the present instance, which is, we believe, the first of the kind on record, not only have the transplantation and union been successful, but the new piece of nerve seems to have overcome the tendency of the old to undergo degeneration of structure at its divided extremity. A portion, six centimetres in length, of the great nerve of a rabbit's thigh was selected, and was so removed from the freshly killed animal as to include the natural bifurcation of the main trunk into two branches. The divided stem was secured by stitches to the stump of the nerve in Prof. von Fleischl's arm, and the ends of the branches were secured in like manner to the nerve terminations which remained in his fingers, and which were rendered useless by their separation from the trunk to which they belonged. The whole operation, as a matter of course, was conducted with strict adherence to those principles of antiseptic surgery without which failure would have been more than likely; but, by the observance of which, union, almost anywhere or of any thing, can with a near approach to certainty be secured. The wound healed kindly, the transplanted nerve soon became at home in its new position; and already, after the lapse of a little more than two months, it is reported that sensation is returning to the fingers. At the same time there has been no return of pain, and no fresh indication of the development of neuromata, so that hope of an absolutely successful issue may now with some confidence be entertained."

THE additions to the Zoological Society's Gardens during the past week include three Cape Crowned Cranes (*Balearica chrysopelargus*) from Zanzibar, presented by Colonel E. Smith; two Peregrine Falcons (*Falco peregrinus*) from India, presented by Mr. J. Davidson; a Gannet (*Sula bassana*), British, presented by the Baroness de Taintegnies; a Three-toed Chalcis (*Chalcides tridactylus*) from France, presented by Mr. J. C. Warburg; an Indian Python (*Python molurus*) from India, received in exchange; an Elliot's Pheasant (*Phasianus ellioti* ♀) from China, purchased; an American Bison (*Bison americanus*), a Great Kangaroo (*Macropus giganteus* ♂), seven Suricates (*Suricata tetradactyla*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

COMET 1888 *a* (SAWERTHAL).—Several computers having shown that the Cape observation of this object made on February 18 cannot be harmonized with those made since perihelion by means of parabolic elements, Prof. Lewis Boss has computed elliptic elements for it, based on the above-mentioned Cape observation, and observations obtained at Albany on March 17 and April 18. His first effort was to find a parabolic orbit from the last two observations, and another, also made at Albany, on March 30; but the resulting parabola not only entirely failed to satisfy the Cape observation, but also left residuals too large to be admitted, for other observations at his disposal which had been made since perihelion. The ellipse, on the contrary, which he obtained from the places of February 18, March 17, and April 18 satisfied these other observations very fairly, the largest differences being given by the observation of March 30, viz. (C – O)—

$$\Delta\alpha = -8''\cdot5. \qquad \Delta\delta = -7''\cdot2.$$

The residuals point to a somewhat larger eccentricity than that given below, but are probably due in great part to comparatively small errors in the first and last observations used.

The elements are as follow:—

$$T = 1888 \text{ March } 16\cdot9987 \text{ G.M.T.}$$

$$\left.\begin{aligned} \omega &= 359^\circ\ 54'\ 58''\cdot4 \\ \Omega &= 245\ \ 22\ \ 46\cdot6 \\ i &= 42\ \ 15\ \ 23\cdot1 \end{aligned}\right\} 1888\cdot0$$

$$\log e = 9\cdot997790$$
$$\log q = 9\cdot844329$$
$$\text{Period} = 1615 \text{ years.}$$

Prof. Boss suspects, however, that the true period will be found decidedly greater than 2000 years.

$$x = r\,[9\cdot898389]\sin(v + 328^\circ\ 9'\ 7''\cdot6).$$
$$y = r\,[9\cdot999694]\sin(v + 236\ \ 29\ \ 13\cdot9).$$
$$z = r\,[9\cdot787085]\sin(v + 323\ \ 42\ \ 17\cdot9).$$

In the same number of *Gould's Astronomical Journal* in which the above elements appear, the Rev. G. M. Searle gives an independent elliptic orbit very closely resembling that computed by Prof. Boss. The first two places used are the same as those Prof. Boss employed; the third is one obtained on April 16 at Harvard College. Prof. Boss gives the following ephemeris for Greenwich midnight:—

1888.	R.A. h. m. s.	Decl. ° ′ ″	Log r.	Log Δ.
May 26	0 17 19·4	38 1 45 N.	0·17274	0·26595
28	0 20 45·6	38 43 8	0·18109	0·27036
30	0 24 5·5	39 23 19	0·18928	0·27459
June 1	0 27 19·0	40 2 24	0·19730	0·27864
3	0 30 26·1	40 40 25	0·20516	0·28252
5	0 33 26·6	41 17 26	0·21287	0·28623
7	0 36 20·5	41 53 30	0·22042	0·28978
9	0 39 7·6	42 28 39	0·22783	0·29317
11	0 41 47·8	43 2 56	0·23509	0·29639
13	0 44 21·1	43 36 23	0·24222	0·29947
15	0 46 47·5	44 9 1 N.	0·24921	0·30240

The light ratio on June 15 is $\frac{1}{20}$ of that at discovery.

New Minor Planet.—A new minor planet, No. 278, was discovered by Herr Palisa at Vienna on May 16.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *MAY* 27—*JUNE* 2.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on May 27

Sun rises, 3h. 54m.; souths, 11h. 56m. 57·8s.; sets, 19h. 59m.: right asc. on meridian, 4h. 18·8m.; decl. 21° 25′ N. Sidereal Time at Sunset, 12h. 22m.

Moon (at Last Quarter June 1, 13h.) rises, 21h. 10m.*; souths, 1h. 27m.; sets, 5h. 41m.: right asc. on meridian, 17h. 47·6m.; decl. 20° 31′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	4 42	13 14	21 46	5 35·8	25 29 N.
Venus.....	3 26	11 6	18 46	3 28·2	17 56 N.
Mars......	14 42	20 22	2 2*	12 45·1	4 31 S.
Jupiter....	19 12	23 31	3 50*	15 55·3	19 22 S.
Saturn....	8 2	15 57	23 52	8 19·3	20 14 N.
Uranus...	14 47	20 27	2 7*	12 50·2	4 39 S.
Neptune..	3 47	11 31	19 15	3 52·8	18 35 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

May.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
27	31 Sagittarii	6	22 17†	23 23	49 240
30	20 Capricorni	6	2 45	3 38	40 322

† In horizon at Greenwich.

Variable Stars.

Star	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	May 27,	0 57	*m*
			June 1,	0 36	*m*
U Cancri	8 29·4	19 17 N.	May 28,		*M*
S Leonis	11 5·1	6 4 N.	,, 27,		*M*
R Corvi	12 13·8	18 38 S.	June 2,		*M*
R Boötis	14 32·3	27 13 N.	May 29,		*M*
U Coronæ	15 13·6	32 3 N.	June 1,	0 37	*m*
S Libræ	15 15·0	19 59 S.	,, 2,		*m*
R Scorpii	16 11·0	22 40 S.	May 30,		*M*
U Ophiuchi	17 10·9	1 20 N.	,, 28,	2 8	*m*
			and at intervals of	20 8	
W Sagittarii	17 57·9	29 35 S.	May 27,	21 0	*M*
Z Sagittarii	18 14·8	18 55 S.	,, 27,	1 0	*M*
			,, 31,	0 0	*m*
β Lyræ	18 46·0	33 14 N.	,, 30,	21 0	*M*
R Lyræ	18 51·9	43 48 N.	June 2,		*M*
S Vulpeculæ	19 43·8	27 1 N.	May 29,		*m*
S Sagittæ	19 50·9	16 20 N.	,, 28,	3 0	*M*
T Vulpeculæ	20 46·7	27 50 N.	,, 31,	23 0	*M*
			June 2,	1 0	*m*
δ Cephei	22 25·0	57 51 N.	,, 1,	1 0	*M*
S Pegasi	23 14·9	8 18 N.	May 29,		*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near χ Boötis	227	30 N.	June 2.
,, 54 Draconis	290	60 N.	Slow, short. May 30.
From Vulpecula	303	24 N.	Swift.
Near ι Pegasi	332	27 N.	Swift. Very long paths. Streaks.

GEOGRAPHICAL NOTES.

Referring to the ethnology of the Himalayan hill region o Sikkim, where a small British force is at present in occupation, the *Madras Mail* says that the population may, broadly speaking, be divided into three nationalities: the Lepchas, who are the aborigines; the Nepalese immigrants, now forming nearly half the entire population; and the Bhuteas, or Bhutanese, who very closely resemble the Tibetans, and are pure Tartars. It is remarkable that the last-named are like the Chinese in the make of their hats, clothes, and boots, and in their pig-tails, but their language is somewhat like Turkish in its sound. It is supposed that this people originally came from Tibet, though they apparently derive their name from Bhutan, which lies to the east of Sikkim. They are tall, strong, and hardy, though they are accused of being lazy. They have their Buddhist temples, and erect long poles round their houses, with paper streamers on which are printed prayers in Chinese-looking characters. One may often meet them on the roads twirling their praying-machines, which are cylinders of brass or copper, with a printed roll of prayer inside, and small weights attached to it to make it revolve when once it is set going. It is thought that amongst them, like the Tibetans, polyandry prevails. The women are large and coarse-featured; they wear thick woollen clothes of bright colours, and numerous massive gold and silver ornaments. Some of them smear themselves with a brownish ointment which makes their faces appear as if a coating of French polish had been put on. With regard to the aborigines of Sikkim, they are a Mongolian race, short and stout. In appearance they resemble closely the Nepalese, though, far different from the latter, who are brave soldiers, they are the most arrant cowards. They live by cultivating small tracts of the forests, which they clear by setting fire to the trees and brushwood, and move to a fresh spot each year. As may be supposed, their agriculture is of the most primitive description, and in their language they have no word for a plough. They worship the forces of Nature under the form of demons; the Bhuteas also, though professed Buddhists, propitiate evil demons, the same sort of imaginary beings as the Nats of the Burmese. The Lepchas are monogamous. The race is gradually dying out. The Limboos are a race of half-breeds between the Nepalese and the Lepchas, but resembling the former more than the latter. There are several similar mongrel races to be found in Sikkim, for the Nepalese immigrate in vast numbers, being driven out of their own country by press of over-population. Few ever return to their own country, and great numbers of them work as coolies on the tea estates. Their religion is a mixture of Buddhism and Brahminism, and they boast of their caste distinctions. Many of them carry curved weapons in their belts, while the Bhuteas and Lepchas use straight-bladed weapons. The Bhutea sword is like that of the old Roman legionary, but the hilt has no guard, after the Mongolian fashion. Amongst the jungles of Perai there are some curious aboriginal tribes, who do not appear to suffer from the malaria which attacks everybody else who sets foot in their territory; but it is said that if they leave their jungles they are immediately attacked themselves by fever, the malarial poison with which they have become inoculated thus finding an exit when they quit their own locality. All the natives of the plain call these races indiscriminately "Pahariyas," or "hill-men," who, though they differ from each other, differ still more from the inhabitants of the plain in their language and mode of life. They are all mountaineers and Mongolians, and have all great physical strength. A story is told of a Bhutea woman who once carried a grand piano up the Ghaut from Punkabari to Darjeeling in three days, and arrived on the third day quite fresh and unexhausted at her destination with her burden on her back.

A recent number of the *China Review* (vol. xvi. No. 3) contains a long paper by Mr. Taylor, whose publications on

Formosa and its people have frequently been noticed in these columns, entitled "A Ramble through Southern Formosa." It really describes a long journey along the almost wholly unknown east coast, and has much information respecting the various tribes, their relations to each other and to the Chinese Government—the Tipuns, Paiwans, Diaramocks, Amias, and others. Mr. Taylor refuses to discuss gravely the theory of a cataclysm put forward to account for the aborigines in Formosa. "One might just as well introduce the mythical convulsion which lost Atlanta to Europe, and detached Great Britain from the neighbouring continent, to account for the painted savages Cæsar found in England." The Tipuns are probably descended from emigrants from some northern islands, probably Japan; the Paiwans as a rule inhabit the mountains of the interior, and are head-hunters, a cruel, predatory, and passionate race, probably of Malay origin, and the very earliest settlers in Southern Formosa. The Pepohoans probably came from Loochoo; they have no language of their own, speaking only Chinese, while all the other tribes have their own tongue. The Diaramocks are the most dreaded tribe of the south part of the island; they are reputed cannibals, but Mr. Taylor doubts whether they are not accused without cause. The paper concludes with some vigorous engravings of representatives of the different tribes, including a Diaramock, a Tipun chief, an Amia, a Paiwan, a Tipun warrior, a Nicka, and Tipun weapons.

THE *Bollettino* of the Italian Geographical Society for April publishes the results of some preliminary studies, by Prof. Giulio Beloch, of the Roman University, on the vital statistics of Italy during the last three centuries. According to these studies, the total population of the peninsula has increased from a little over 11,000,000 in 1550 to 13,000,000 in 1700, 16,500,000 in 1775, over 18,000,000 in 1880, and nearly 30,000,000 in 1887. The growth of the population for some of the larger States is given as under:—

States.	Year.	Pop. in millions.	Year.	Pop. in millions.	Year.	Pop. in millions.
Naples ...	1511	2·7	1700	3·0	1770	4·09
Sicily	1570	1·07	1714	1·12	1770	1·48
States of the Church ...	1560	1·6	1701	1·98	1769	2·17
Tuscany ...	1562	0·8	1738	0·89	1766	0·94
Venetia ...	1548	1·6	1700	1·8	1766	2·24
Milanese ...	1542	1·0	1724	1·1	1773	1·1
Piedmont ...	1569	1·05	1723	1·55	1773	2·3
Sardinia ...	1575	0·15	1728	0·3	1775	0·42

A GEOLOGICAL Expedition, under the leadership of MM. Ivanoff and Konshin—the two well-known investigators of the geology of Turkistan—is to be sent out this summer for the exploration of the littoral region of Russian Mantchuria. The orography of this region is hardly yet known, and the Expedition will certainly throw some light on the structure of the chains of mountains which are still hypothetically represented on our maps.

THE IRON AND STEEL INSTITUTE.

THE annual meeting of the Iron and Steel Institute took place last week at the theatre of the Institution of Civil Engineers, under the presidency of Mr. Daniel Adamson. On the motion of the President, His Royal Highness the Prince of Wales was unanimously elected an honorary member. Sir Henry Bessemer presented the Bessemer medal to the President, and referred in the course of his remarks to the circumstance that whereas in Sheffield, the stronghold of steel-making, he could find no one to investigate his process when he first brought it out, fortunately for him—and he might add, fortunately for the world—their President, Mr. Daniel Adamson, did so, and having satisfied himself as to its applicability determined to employ it. The President, whose investigations with regard to steel are well known, thanked Sir Henry Bessemer and the Council of the Institution for the award, and referred to his early connection with Bessemer steel, which metal he had continued to use ever since.

The President then delivered the annual address, which was mainly statistical in character. The Iron and Steel Institute had been nineteen years in existence, during which period 2116 members had been elected, including seventy-two elected at the present meeting. He drew attention to the falling off which had taken place in the production of manufactured iron in this country since 1884, and the large increase in the production of steel during the same period. Thus in 1884 about one and a quarter million tons of Bessemer steel ingots were produced, and in 1887 about two million tons, being an increase of about 60 per cent.; in 1884 nearly half a million of tons of Siemens open-hearth steel ingots were cast, and nearly a million tons last year, the actual increase during the period being over 106 per cent., besides which plant is at present in course of erection estimated to produce another quarter of a million tons annually. During the same period there has been an enormous increase in the application of steel to shipbuilding purposes. Thus from a table supplied to the President by Mr. William Parker, Chief Engineer to Lloyd's Registry of British and Foreign Shipping, it is found that whereas in 1878, under 3000 tons of steel were employed in the manufacture of steamers and sailing-vessels built under Lloyd's survey, and over 300,000 tons of iron, last year over 210,000 tons of steel were employed and about 52,000 tons of iron. The proportional increase in the use of steel in the last three years has been about cent. per cent., and the falling off in the use of iron during the same period 350 per cent. Before leaving the subject of steel, the President referred to the papers read at the Institution of Civil Engineers on "Manganese in its Application to Metallurgy," and on "Some Novel Properties of Iron and Manganese," wherein it was shown that whereas 2·5 to 7·5 per cent. of manganese in steel makes it as brittle as glass, breaking under a much less transverse load than cast iron, 12 to 14 per cent. of manganese in the metal secures high carrying power with great elongation. Thus a bar of the composition—carbon 0·85 per cent., silicon 0·23 per cent., sulphur 0·08 per cent., phosphorus 0·09 per cent., and manganese 13·5 per cent., carried a load of 57·02 tons to the square inch, and took a permanent set at 29½ tons, with an elongation of 39·8 per cent. This metal is toughened by heating it to a high temperature, and plunging it into water at a temperature of 72° F. It is difficult to machine, which would militate against its practical application for many purposes, unless cooling in water whilst developing strength and toughness should also have a softening tendency. The President concluded his address by drawing attention to the influence of the alloys they contain on the various applications of pig metal, as outside of high-class hæmatites that are used for the manufacture of Bessemer and open-hearth steel, selections may be made giving the highest results without using some of the higher-priced irons that are now considered necessary for given purposes.

Mr. Carbutt, President of the Institution of Mechanical Engineers, in proposing a vote of thanks to the President for his address, drew attention to the interesting circumstance noted by Mr. Parker that 100 tons can now be carried one mile by steamships at the rate of thirteen miles an hour, at a total cost, including fuel, insurance, &c., of seven-eighths of a penny.

The papers read and discussed at this meeting ranged over a large variety of subjects. Mr. T. Turner's paper on "Silicon and Sulphur in Cast Iron," read at a previous meeting, was discussed. The conclusions at which the author arrives are that in the blast furnace three chief agencies are at work tending to eliminate sulphur, of which in Cleveland practice not more than one-twentieth passes into the iron: (1) a high temperature tends to prevent the absorption of sulphur by iron; (2) a slag rich in lime readily combines with sulphur; and (3) the amount of sulphur actually retained by the metal is influenced by the proportion of silicon and probably certain other elements present in the iron—the more silicon the less sulphur. This paper was discussed by Messrs. Snelin, Gautier, Riley, Bauerman, and Sir Lowthian Bell; but the author, in his reply on the discussion, considered that nothing had been brought forward to disprove what he maintained, viz. that if they put silicon and sulphur together in iron, they would not combine there, the sulphur would pass off and the silicon remain.

Mr. Gautier read a paper on the melting in cupola furnaces of wrought iron or steel scrap mixed with ferro-silicon, the conclusion at which he arrived being that ordinary wrought-iron scrap so heated may yield results as good as those obtained from castings made with ordinary steel scrap. This conclusion was contested, however, by various speakers in discussion.

A paper read at the last meeting of the Institute by Mr. A. Wilson, on "The Use of Water Gas for Metallurgical

Purposes," was discussed. The author had found water gas and producer gas practically equal both as regarded cost of production and heating values.

Mr. H. Eccles drew attention, in a paper on "An Imperfection in Mild Steel Plates considered chemically," to want of care in sampling steel before casting, whereby defects in the ingots were rolled out into the plates; and it appeared in the discussion, as well as in a paper by Major Cubillo, on "The Manufacture and Treatment of Ordnance at Trubia," that the ingot was much improved when the steel was made and heated in a radiation furnace. Another paper by Major Cubillo, on "Steel Castings for the Manufacture of Guns," gave rise to a highly technical discussion; as did also papers on "The Behaviour of Arsenic in Ore and Metal during Smelting and Purification Processes," by Messrs. Pattinson and Stead, and on "The Effect of Arsenic on Mild Steel," by Messrs. Harbord and Tucker.

The last paper read was on "A New Instrument for the Measurement of Colour, more especially as applied to the Estimation of Carbon in Steel," by Mr. H. Le Neve Foster. In the instrument are two fields of view under similar monocular conditions, freed from any errors which may arise from the introduction of unequal side lights, and also the different power of distinguishing colour that often exists in the eye of the observer; in conjunction with the instrument is a standard set of coloured glasses, each set being the same colour, but regularly graded for depth of tint. The instrument consists of a tube, divided by a central partition terminating at the eye-piece in a knife-edge, which, being inside the range of vision, is not seen when the instrument is in use. At the other end of the instrument are two apertures of equal size, and alterable in size or shape by means of diaphragms. The two apertures are divided by the thick end of the central partition, which, together with the sides, is recessed so as to hide the edges of the standard glasses, as well as the sides of the gauged glass vessels, which are used to contain the liquid that requires to be matched or compared. The only light coming to the eye must pass in equal quantity through the gauged glass vessel and the standard glasses respectively.

The instrument has been used by dyers, brewers, and sugar, and various other manufacturers. It forms a ready means of measuring the depth of colour in water, and is also applicable for Nessler's ammonia test as used in water analysis. For the estimation of carbon, the author finds the best results are obtained by dissolving 0·5 gramme of steel in 10 c.c. nitric acid, sp. 1·20, and boiling for twenty minutes, and then diluting to 50 c.c. and placing the liquid in a 1-inch cell. For mild steel this gives an easy colour to match, the results obtained agreeing well with those found by the Eggerty method.

SCIENTIFIC SERIALS.

THE *Quarterly Journal of Microscopical Science* for April, 1888 (vol. xxviii. part 4) contains:—A monograph on the species and distribution of the genus Peripatus, Guilding (plates 34 to 40), by Adam Sedgwick, F.R.S., gives an account of all the known species of the genus, with a bibliography of most of the literature relating to them. Many of the figures are coloured from nature.—Notes on the anatomy of *Peripatus capensis* and *P. novæ-zealandiæ*, by Lilian Sheldon, Bathurst Student, Newnham College, Cambridge. Gives details about the crural glands, the segmental organs, the accessory glands of the male, and the vas deferens.—On the construction and purpose of the so-called labyrinthine apparatus of the labyrinthine fishes (plate 41), by Dr. Nicholas Zograff.—Studies on the comparative anatomy of sponges; (1) on the genera Ridleia, n. gen., and Quasillina, Norman (plate 42), by Arthur Dendy.—Kleinenberg on the development of Lopadorhynchus, by G. C. Bourne. This paper gives a *résumé* of Prof. Kleinenberg's very detailed account of the development of the Polychæte Annelid Lopadorhynchus, which extends over 225 pages of the *Zeitschrift für Wissenschaftliche Zoologie.*

American Journal of Science, May.—The absolute wave-length of light, Part 2, by Louis Bell. In continuation of his previous communication the author here gives the angular measurements, and the details of the measurement and calibration of the gratings, together with the final results. He also inquires into the probable sources of error in some recent determinations of wave-length. His own final determination of the mean value of the absolute wave-length for the line D_1 is 5896·18 in air at 760 mm. pressure and 20° C. temperature, or *in vacuo* 5897·90, which he considers not likely to be in error by an amount as great as one part in two hundred thousand.—Three formations of the Middle Atlantic slope (continued), by W. J. McGee. In this paper the author deals with the Columbian formation alone, describing in detail the general characters of its fluvial and interfluvial phases. By the *fluvial* phase he understands the thicker and more conspicuous formations commonly occurring along the great rivers at and for some miles below the fall line, while the *interfluvial* comprises the thinner deposits forming the surface over the remainder of the coastal plain. These interfluvial deposits are shown to corroborate and extend the testimony of the deltas, all the phenomena conjointly recording a brief period of submergence of the entire coastal plain in the Middle Atlantic slope reaching 100 feet in the south and over 400 in the north, with coæval cold, long anterior to the terminal moraine period.—On some peculiarly-spotted rocks from Pigeon Point, Minnesota, by W. S. Bailey. The character and origin are discussed of some curious circular spots occurring here and there on the quartzites of Pigeon Point, a district projecting about 3½ miles into Lake Superior, and consisting mainly of a great dyke of coarse olivine gabbro or diabase.—The Taconic system of Emmons, and the use of the name Taconic in geological nomenclature (continued), by Chas. D. Walcott. In this paper the author deals with the subject of nomenclature, discussing the use of the names Taconic and Cambrian, and concluding with a classification of the North American Cambrian rocks.—Prof. R. D. Salisbury has some remarks on the terminal moraines of North Germany, and Carl Barus communicates a short note on the viscosity of gases at high temperature, and on the pyrometric use of the principle of viscosity.

Bulletin de l'Académie Royale de Belgique, March.—Remarks on some stone implements found in Spain by MM. H. and L. Siret, by A. F. Renard. Amongst the rich archæological finds recently made by MM. Siret in the Carthagena and Almeria districts are some polished stone hatchets of small size and beautiful workmanship. With a view to determining the material of which these implements were made, the author has subjected them to a careful analysis, and finds that this material is fibrolite, which occurs in many parts of Spain. In appearance it somewhat resembles jade, but its chemical composition and general properties show that it is quite distinct from that substance.—Determination of the variations in the specific heat of fluids near the critical point, by P. de Heen. It is suggested as a working hypothesis, that fluids are formed of molecular groups which may be called liquidogenic molecules. These groups and their constituent elements, presenting the aspect of little vortices, would appear simply to be the molecules as regarded in the gaseous state (gasogenic molecules). The transition from the liquid to the gaseous state at the critical temperature might then be thus interpreted. It may be admitted that at a given temperature the *gasogenic molecules* cease to move in closed curves, and describe the rectilinear trajectories of M. Clausius. The author's researches, as here described, tend mainly to confirm this view.

Rendiconti del Reale Istituto, Lombardo, April.—On the importance of the phagociti in the morphology of the Metazoi, by Prof. Leopoldo Magi. The author's researches generally tend to confirm the conclusions of Metschnikoff regarding the physiological functions of the phagociti. He considers that "phagocitism"—that is, the intracellular digestive process—is a function which attests in the morphology of the Metazoi, or pluricellular organisms, their genetic descent from the Protozoi. Thus physiology, as well as embryology and palæontology, confirms the now commonly accepted views regarding biological evolution in animal organisms.

Rivista Scientifico-Industriale, April 30.—On some recent discoveries in electro-optics, by Prof. Ercole Fossati. In connection with the recent researches of Hertz and Hallewachs on the influence of light on electrified conductors, attention is directed to the analogous experiments made by Morichini at the beginning of the present century. Reference is made more particularly to this physicist's observations on the magnetization of steel by the effect of light alone, independently altogether of any action caused by heat or terrestrial magnetism.—Researches on magnetic thermogenesis, by Prof. Giuseppe Martinotti. The experiments described in this and previous papers lead to the general conclusion that heat is developed when soft iron, or any other magnetic body, is successively magnetized; and that the heat is increased by reversing or even simply interrupting the current, which is in accordance with the modern theories on thermodynamics and molecular polarization. But all these experiments are merely preliminary studies in a field of vast and increasing importance, the cultivation of which may ultimately lead to the greatest discovery of modern times, the determination and application of the laws by which the material universe is governed even in phenomena of a psychic order.

SOCIETIES AND ACADEMIES.

London.

Royal Society, March 8.—"Contributions to the Anatomy of the Central Nervous System of Vertebrate Animals: Anatomy of the Brain of *Ceratodus forsteri*." By Alfred Sanders, M.R.C.S., F.L.S.

The brain of Ceratodus has the following general arrangement: the membrane which represents the pia mater is of great thickness and toughness; there are two regions where a tela choroidea is developed; one where it covers in the fourth ventricle, and the other where it penetrates through the third ventricle and separates the lateral ventricles from each other.

The thalamencephalon and the mesencephalon are narrow, and the medulla oblongata is wide. The ventricles are all of large size, and the walls of the lateral ventricles are not completed by nervous tissue. All the cranial nerves are to be seen except the abducens and the hypoglossal. There is a large communicating branch between the trifacial and the vagus. The glossopharyngeal has no separate root, but is a branch of the vagus; the ganglion of the vagus is not the termination of the main trunk, but is an off-shoot from the ramus lateralis; the ganglion gives off the branchial nerves and the ramus intestinalis, the ramus lateralis passing on without entering it.

The minute structure of the dorsal part of the cerebrum presents four layers: externally a layer of finely granular neuroglia, with slight indications of radial striation; next a layer of larger-sized cells; then another layer of neuroglia, with fibrillæ having a tendency to a longitudinal direction; and internally a layer of rounded cells closely crowded together. The ventral part of the cerebrum has only two layers, the external of neuroglia, and the internal of rounded cells.

The olfactory lobes resemble the cerebrum in structure, there is an internal layer of cells continuous with those of the cerebrum, and an external layer of glomeruli olfactori, which seem as if they were the external layer of the cerebrum condensed; and between the two, a layer of longitudinal fibres, on which fusiform cells are developed.

The optic lobes also consist of four layers: externally there is a layer of longitudinal fibrils derived from the optic tract; then a layer of smoothly granular neuroglia; then a layer of transverse fibrillæ which collect into a commissure in the central line at the dorsal surface. This layer also contains fusiform and rounded cells sparsely scattered through it; and internally there is a layer of cells mostly rounded. At the central line on the dorsal surface there is a ganglion of large cells resembling those of the optic lobe of the Plagiostomata.

The cerebellum is a mere bridge over the fourth ventricle. Its structure presents the usual number of layers: internally the fibrous layer, which ultimately forms the crura cerebelli ad medullam; then the granular layer, the cells of which are of large size compared to those of the same layer in Teleostei and Plagiostomata; then a layer of Purkinje cells, of which the form and the number of processes are not uniform; externally is the molecular layer, which consists of a coarsely granular network derived from the processes of the Purkinje cells, also a network of finer fibrils and many rounded cells.

In the spinal cord there are three columns of longitudinal fibres on each side in the white substance: viz. the ventral columns between the two ventral roots of the spinal nerves; the lateral columns between the dorsal and ventral roots; and the dorsal columns between the two dorsal roots. Fibres of large size are scattered throughout the two former columns, but collected principally in the ventral. The dorsal consists entirely of minute fibres.

The principal feature in the white substance is a fibre of gigantic size, which is situated on the summit of the ventral columns, one on each side; it consists of a common medullary sheath; inclosing (where the fibre is largest) about 40 to 50 axis-cylinders; these have the character of the axis-cylinders of the ordinary fibres of the white substance, but have no separate medullary sheaths; this fibre is traceable throughout the spinal cord; commencing opposite the posterior end of the abdomen, it extends to a short distance behind the exit of the facial nerve; it varies in size, and becomes of the greatest diameter near the posterior end of the medulla oblongata; its axes escape through the medullary sheath, and join the longitudinal fibres of the ventral columns. Near its anterior termination all the axes have escaped except one; at this point it bears a great resemblance to Mauthner's fibre in the Teleostei. This remaining fibre decussates with that of the other side a short distance behind the exit of the facial nerve, and enters the root of that nerve on the opposite side.

In the gray substance of the spinal cord, there are two series of ganglia, one in the ventral horn, which consists of multipolar cells often of very large size. They send processes into the ventral and lateral columns, which often become the smaller-sized longitudinal fibres. The cells of the other series of ganglia are of smaller size, and are situated in the substantia gelatinosa centralis; they are smooth in outline, and give off one or two processes; they probably have to do with the dorsal roots of the spinal nerves. Cells also of this kind occur at other places, as in the fibræ rectæ, and in the field of the ventral columns.

The transverse commissures are: one in the spinal cord, which passes through the substantia gelatinosa centralis over the central canal; another on the ventral side of the anterior part of the medulla oblongata, which corresponds to the commissura ansulata of the Teleostei, and is connected with the commissure in the dorsal part of the optic lobes; then there is the posterior commissure at the posterior part of the third ventricle; and a commissure at the posterior end of the cerebrum which is the anterior commissure.

There is no chiasma of the optic nerves visible externally; what there is of it, is situated in the substance of the thalamencephalon. The anterior root of the fifth nerve arises from a ganglion occupying a broad swelling at the lateral part of the gray matter of the floor of the fourth ventricle. The posterior root arises from the summit of the restiform bodies.

The facial passes backward in a small tubercle at the junction of the floor of the fourth ventricle with the restiform bodies.

The acusticus arises from a bundle of fibres which are situated on the summit of the ventral column, and appear to be a continuation forward of part of the multi-axial fibre which has not decussated.

The five roots of the vagus pass backward, and enter in succession the same tubercle as, and to the outside of, the facial nerve; the three posterior roots are double, so that the vagus is equivalent to eight nerves, and consists entirely of dorsal roots.

Two nerves are given from the ventral side of the medulla oblongata, each of which has two roots; they do not join the vagus, and pass back some distance within the vertebral canal, and emerge on a level with the exit of the dorsal roots of the spinal nerves.

The second and third spinal nerves supply the pectoral fin, and follow the course usually pursued by the hypoglossal when that nerve is present in Teleostei.

The fibres of the ventral roots of the spinal nerves enter in a direction upward and forward toward the inner edge of the multi-axial fibre, between it and the central canal, and then passing over the dorsal edge of the same are either lost in the gray substance of the ventral horn, join a process of one of the multipolar cells, or become one of the longitudinal fibres of the ventral columns of the cord.

The brain of Ceratodus presents an embryonic condition in three points: viz. first, in the extreme size of the ventricles and the tenuity of the substance of their walls; second, in the alternating origins of the dorsal and ventral roots; third, in the origin of the dorsal roots close to the central line.

Compared to Protopterus, it differs in the shape and imperfection of the cerebral lobes, and in the fact of its having a well-developed rhinencephalon; but it agrees in the narrowness of the mesencephalon, and breadth of the medulla oblongata, and in the rudimentary character of the cerebellum.

Ceratodus agrees also with the Ganoids in the comparative narrowness of the mesencephalon, and in the proportions of the cerebellum.

With the Plagiostomata it agrees in the structure of the optic lobes, both orders presenting a ganglion of large cells in the dorsal part.

With the Teleostei it agrees in the multi-axial fibre, which anterior to its termination resembles the Mauthner's fibre, also in the position and fact of its decussation.

With the Petromyzon it agrees in the structure of the tela choroidea which covers the fourth ventricle.

April 19.—"On the Heating Effects of Electric Currents, No. III." By W. H. Preece, F.R.S.

I have taken a great deal of pains to verify the dimensions of the currents as detailed in my paper read on December 22, 1887, required to fuse different wires of such thicknesses that the law

$$C = ad^{3/2}$$

is strictly followed; and I submit the following as the final values of the constant "a" for the different metals:—

	Inches.	Centimetres.	Millimetres.
Copper	10244	2530	80·0
Aluminium	7585	1873	59·2
Platinum	5172	1277	40·4
German silver	5230	1292	40·8
Platinoid	4750	1173	37·1
Iron	3148	777·4	24·6
Tin	1642	405·5	12·8
Alloy (lead and tin 2 to 1)	1318	325·5	10·3
Lead	1379	340·6	10·8

With these constants I have calculated the two following tables, which I hope will be found of some use and value:—

Table showing the Current in Amperes required to Fuse Wires of Various Sizes and Materials.

$$C = ad^{3/2}.$$

No. S.W.G.	Diameter. Inches.	$d^{3/2}$.	Copper. $a = 10244$.	Aluminium. $a = 7585$.	Platinum. $a = 5172$.	Ger. Silver. $a = 5230$.	Platinoid. $a = 4750$.	Iron. $a = 3148$.	Tin. $a = 1642$.	Tin-Lead Alloy. $a = 1318$.	Lead. $a = 1379$.
14	0·080	0·022627	231·8	171·6	117·0	118·3	107·5	71·22	37·15	29·82	31·20
16	0·064	0·016191	165·8	122·8	83·73	84·68	76·90	50·96	26·58	21·34	22·32
18	0·048	0·010516	107·7	79·75	54·37	54·99	49·95	33·10	17·27	13·86	14·50
20	0·036	0·006831	69·97	51·81	35·33	35·72	32·44	21·50	11·22	9·002	9·419
22	0·028	0·004685	48·00	35·53	24·23	24·50	22·25	14·75	7·692	6·175	6·461
24	0·022	0·003263	33·43	24·75	16·88	17·06	15·50	10·27	5·357	4·300	4·499
26	0·018	0·002415	24·74	18·32	12·49	12·63	11·47	7·602	3·965	3·183	3·330
28	0·0148	0·001801	18·44	13·66	9·311	9·416	8·552	5·667	2·956	2·373	2·483
30	0·0124	0·001381	14·15	10·47	7·142	7·222	6·559	4·347	2·267	1·820	1·904
32	0·0108	0·001122	11·50	8·512	5·805	5·870	5·330	3·533	1·843	1·479	1·548

Table giving the Diameters of Wires of Various Materials which will be Fused by a Current of Given Strength.

$$d = \left(\frac{C}{a}\right)^{2/3}.$$

Currents in amperes.	Diameter in inches.								
	Copper. $a = 10244$.	Aluminium. $a = 7585$.	Platinum. $a = 5172$.	German Silver. $a = 5230$.	Platinoid. $a = 4750$.	Iron. $a = 3148$.	Tin. $a = 1642$.	Tin-Lead alloy. $a = 1318$.	Lead. $a = 1379$.
1	0·0021	0·0026	0·0033	0·0033	0·0035	0·0047	0·0072	0·0083	0·0081
2	0·0034	0·0041	0·0053	0·0053	0·0056	0·0074	0·0113	0·0132	0·0128
3	0·0044	0·0054	0·0070	0·0069	0·0074	0·0097	0·0149	0·0173	0·0168
4	0·0053	0·0065	0·0084	0·0084	0·0089	0·0117	0·0181	0·0210	0·0203
5	0·0062	0·0076	0·0098	0·0097	0·0104	0·0136	0·0210	0·0243	0·0236
10	0·0098	0·0120	0·0155	0·0154	0·0164	0·0216	0·0334	0·0386	0·0375
15	0·0129	0·0158	0·0203	0·0202	0·0215	0·0283	0·0437	0·0506	0·0491
20	0·0156	0·0191	0·0246	0·0245	0·0261	0·0343	0·0529	0·0613	0·0595
25	0·0181	0·0222	0·0286	0·0284	0·0303	0·0398	0·0614	0·0711	0·0690
30	0·0205	0·0250	0·0323	0·0320	0·0342	0·0450	0·0694	0·0803	0·0779
35	0·0227	0·0277	0·0358	0·0356	0·0379	0·0498	0·0769	0·0890	0·0864
40	0·0248	0·0303	0·0391	0·0388	0·0414	0·0545	0·0840	0·0973	0·0944
45	0·0268	0·0328	0·0423	0·0420	0·0448	0·0589	0·0909	0·1052	0·1021
50	0·0288	0·0352	0·0454	0·0450	0·0480	0·0632	0·0975	0·1129	0·1095
60	0·0325	0·0397	0·0513	0·0509	0·0542	0·0714	0·1101	0·1275	0·1237
70	0·0360	0·0440	0·0568	0·0564	0·0601	0·0791	0·1220	0·1413	0·1371
80	0·0394	0·0481	0·0621	0·0616	0·0657	0·0864	0·1334	0·1544	0·1499
90	0·0426	0·0520	0·0672	0·0667	0·0711	0·0935	0·1443	0·1671	0·1621
100	0·0457	0·0558	0·0720	0·0715	0·0762	0·1003	0·1548	0·1792	0·1739
120	0·0516	0·0630	0·0814	0·0808	0·0861	0·1133	0·1748	0·2024	0·1964
140	0·0572	0·0698	0·0902	0·0895	0·0954	0·1255	0·1937	0·2243	0·2176
160	0·0625	0·0763	0·0986	0·0978	0·1043	0·1372	0·2118	0·2452	0·2379
180	0·0676	0·0826	0·1066	0·1058	0·1128	0·1484	0·2291	0·2652	0·2573
200	0·0725	0·0886	0·1144	0·1135	0·1210	0·1592	0·2457	0·2845	0·2760
225	0·0784	0·0958	0·1237	0·1228	0·1309	0·1722	0·2658	0·3077	0·2986
250	0·0841	0·1028	0·1327	0·1317	0·1404	0·1848	0·2851	0·3301	0·3203
275	0·0897	0·1095	0·1414	0·1404	0·1497	0·1969	0·3038	0·3518	0·3413
300	0·0950	0·1161	0·1498	0·1487	0·1586	0·2086	0·3220	0·3728	0·3617

May 17.—"On the Structure of the Electric Organ of *Raia circularis.*" By J. C. Ewart, M.D., Regius Professor of Natural History, University of Edinburgh. Communicated by Prof. J. Burdon Sanderson, F.R.S.

This paper gives an account of the structure of the cup-shaped bodies, which, as mentioned in a previous paper read on April 26, 1888, make up the electric organs of certain members of the skate family. The structure of these electric cups has been already studied in three species of skate, viz. *Raia fullonia*, *R. radiata*, and *R. circularis*. The present paper only deals with the electric organ of *R. circularis*. It shows that the cups in this species are large, well-defined bodies, each resembling somewhat the cup of the familiar "cup and ball." The cup proper, like the disks of *R. batis*, consists of three distinct layers, (1) the lining, which is almost identical with the electric plate of *R. batis*; (2) a thick median striated layer; and (3) an outer or cortical layer. The lining or electric plate is inseparably connected with the terminal branches of the numerous nerve-fibres, which, entering by the wide mouth in front, all but fill the entire cavity of the cup, and ramify over its inner surface, the intervening spaces being occupied by gelatinous tissue. This electric layer, which is richly nucleated, presents nearly as large a surface for the terminations of the electric nerves as the electric plate which covers the disk in *R. batis* and *R. clavata*. The striated layer, as in *R. batis*, consists of numerous lamellæ, which have an extremely contorted appearance, but it differs from the corresponding layer in *R. batis*, in retaining a few corpuscles. The cortical layer very decidedly differs in appearance from the alveolar layer in *R. batis*. It is of considerable thickness, contains large nuclei, and sometimes has short blunt processes projecting from its outer surface. These short processes apparently correspond to the long complex projections which in *R. batis* give rise to an irregular network, and they seem to indicate that the cortical layer of *R. circularis* essentially agrees with the alveolar layer of *R. batis*, differing chiefly in the amount of complexity. Surrounding the cortex there is a thin layer of gelatinous tissue in which capillaries ramify. This tissue evidently represents the thick gelatinous cushion which lies behind the disk in *R. batis*, and fills up the alveoli.

The stem of the cup is usually, if not always, longer than the diameter of the cup. It consists of a core of altered muscular substance, which is surrounded by a thick layer of nucleated protoplasm continuous with the cortical layer of the cup, and apparently also identical with it.

The cups are arranged in oblique rows to form a long, slightly-flattened spindle, which occupies the posterior two-thirds of the tail, being in a skate measuring 27 inches from tip to tip, slightly over 8 inches in length, and nearly a quarter of an inch in width at the widest central portion, but only about 2 lines in thickness.

The posterior three-fifths of the organ lies immediately beneath the skin, and has in contact with its outer surface the nerve of the lateral line. The anterior two-fifths is surrounded by fibres of the outer caudal muscles. It is pointed out that while the organ in *R. circularis* is larger than in *R. radiata*, it is relatively very much smaller than the organ of *R. batis*.

Linnean Society, April 19.—Mr. Carruthers, F.R.S., President, in the chair.—Prof. Martin Duncan exhibited a specimen of *Heterocentrotus mamillatus*, showing the apertures of three of the genital ducts to be in the median interradial sutures, the corresponding basal plates being imperforate. A discussion followed, in which Mr. W. Percy Sladen and Dr. C. Stewart took part.—Mr. George Murray exhibited some specimens of *Spongocladia*, with explanatory coloured diagrams, and made some interesting remarks on the presence of sponge-spicules on Algæ at present unaccounted for.—Mr. D. Morris, of Kew, exhibited, and made remarks upon, the bird-catching sedge, *Uncinia jamaicensis*.—Mr. John R. Jackson, of Kew, exhibited some table mats from Canada made of the highly scented grass *Hierochloa borealis*, and a sample of the so-called pine wool prepared from the leaves of the American long-leaved or turpentine-yielding pine, *Pinus australis*, with a mat made from the wool, an industry which has recently been started on a large scale at Wilmington, North Carolina.—Mr. J. E. Harting exhibited a living specimen of Natterer's bat, which had been captured the previous day at Christchurch, Hants, together with a water-colour drawing from life of Daubenton's bat recently taken at the same place.—The first paper of the evening was by the Rev. George Post (communicated by Mr. Thiselton Dyer), and contained descriptions of new plants from Palestine. In the absence of the author, the salient points in the paper were admirably demonstrated by Mr. J. G. Baker, F.R.S., who exhibited specimens of the plants alluded to.—A paper was then read by the Botanical Secretary, Mr. B. Daydon Jackson, on behalf of Prof. Fream, on the flora of water meadows. An interesting discussion followed, and the meeting adjourned.

May 3.—Dr. John Anderson, F.R.S., Vice-President, in the chair.—The Chairman announced a resolution of the Council to found a gold medal, to be called the "Linnean Medal," to be awarded at the forthcoming anniversary meeting to a botanist and zoologist, and in future years to a botanist and zoologist alternately, commencing with a botanist.—Dr. Francis Day exhibited some specimens of Loch-leven and sea trout raised at Howietoun to illustrate his observation that the markings usually relied upon to distinguish the species are not constant, and therefore, taken alone, of no value for the purpose of identification. He also exhibited specimens of trout from Otago, New Zealand, descendants of some which had been introduced there, presenting some curious modifications of structure. A discussion followed, in which some interesting remarks were made by Prof. Howes and Mr. Willis Bund.—On behalf of Mr. Miller Christy, the Botanical Secretary (Mr. B. Daydon Jackson) exhibited some specimens of the Bardfield oxlip (*Primula elatior*, Jacquin), gathered near Dunmow, and occurring only in this part of England (cf. Trans. Essex Field Club, iii. p. 148).—Mr. A. D. Michael read a paper on the life-histories of the Acari *Glyciphagus domesticus* and *G. spinipes*. After describing in detail observations and dissections extending over three years, the author concludes that there is a hypopial stage in the life-history of *Glyciphagus*, but far less developed than in *Tyroglyphus*, and not an active stage in the species observed; that it does not occur to all individuals of a species, and it has not been ascertained whether it occurs in all species; that the stage is not the result of desiccation or unfavourable conditions; and that it occupies the period between the penultimate ecdysis and that immediately previous. Dr. C. Stewart criticized Mr. Michael's researches in favourable terms.—A communication was then made by Mr. C. B. Clarke on root-pressure. He contested the view of A. Sachs (and his followers) that root-pressure is sufficient to sustain the weight of a column of water of the height of 100 (or even 30) feet, and to force out drops at particular points of the leaves. He maintained that it was a mathematical error to apply the equation $p = g\rho z$ to the case of water in plants, and that in a collection of cells and longitudinal tubes of varying size (all very small) the only mechanical ideas that could be applied were those of capillary attraction and motion. In the discussion which followed, Prof. Marshall Ward thought root-pressure necessary to explain the admitted results of manometer experiments. Mr. A. W. Bennett, on the other hand, regarded the assumption of a high fluid tension in the cells of roots to drive moisture to the summits as nothing more than an expression of our ignorance as to what the water does move.—A paper on the ovicells of some Lichenoporæ was read by the Zoological Secretary (Mr. W. Percy Sladen in the absence of the author, Mr. A. W. Waters.

Physical Society, April 28.—Prof. Reinold, F.R.S., President, in the chair.—The following communications were read:—On electromotive force by contact, by Mr. C. V. Burton. The object of the paper is to discuss the seats of the electromotive forces developed by the contact of conductors. By considering the distribution of electricity on the surfaces of the conductors, and from the fact that the potentials throughout their masses are constant, except about a thin layer near the junction, the author deduces that "*the molecular action which gives rise to a contact E.M.F. between two conductors is confined to the immediate neighbourhood of the junction.*" If E be the contact E.M.F., and M the quantity of electricity which passes across the junction when two metals originally at the same potential are placed in contact, it is shown that the work done is EM, half of which is spent in producing heat and half in raising the potential energy of the system. Since the conductors are supposed to be kept at constant temperature, and the action which gives rise to the E.M.F. is confined to the immediate neighbourhood of the junction, the molecular energy must be absorbed at the junction. By supposing the surface of contact very small, and the capacity of the system large, it is shown that *heat* and *chemical action* are the only kinds of energy which fulfil the required conditions of supplying an indefinite amount of energy. Hence, for substances chemically inactive, "*the true contact E.M.F. is equal to their coefficient of the Peltier effect* *expressed in absolute measure*"; and for substances chemically active, but devoid of Peltier effect, "*the E.M.F. is equal to the energy of combination of one electro-chemical equivalent.*" Since metal-metal contacts can only be the seats of Peltier E.M.F.'s it is inferred that the apparent contact E.M.F. (measured inductively) must be due chiefly to air-metal contacts. A list of analogous properties of Peltier and chemical E.M.F.'s is given in parallel columns. The results of some experiments on the contact E.M.F. of glass and ebonite with mercury are tabulated, but they are very irregular, and the author concludes that there is no true and definite contact E.M.F. between conductors and non-conductors. Profs. Ayrton, Schuster, Thompson, and Perry discussed the points raised, and it was considered that direct experiment on contact E.M.F. in a very perfect vacuum could alone decide the questions.—On a theory concerning the sudden loss of magnetic properties of iron and nickel, by Mr. H. Tomlinson. Experiments by himself and other observers have shown that the temperatures at which iron and nickel lose their magnetic properties depend on the specimens used, and the magnetizing forces employed; but the temperature at which they *begin to lose* these properties are definite—for nickel about 300° C., and iron about 680° C. The author's own experiments on "Recalescence of iron" show two critical temperatures; and Pinchon has shown by calorimetric measurements that between 660° and 720° C., and between 1000° and 1050° C., heat becomes latent. All these facts seem to indicate a molecular rearrangement about these temperatures. In his proposed theory, he assumes that the molecules of iron (say) contain magnetic atoms capable of motions of translation and of rotation. These tend to form closed magnetic circuits, but at ordinary temperatures are unable to do so on account of the close proximity of their centres. On raising the temperature, their centres are further separated till at about 680° C. their polar extremities rush together, forming complete circuits and exhibiting no external magnetic properties. On cooling down, the centres approach until the gravitation attraction overcomes the magnetic attraction of their poles, when the magnetic properties reappear. Prof. Ayrton asked whether the author had made experiments on the reappearance of magnetic properties when raised to a white heat, and Prof. Thompson inquired whether cobalt had been tested. Both questions were answered negatively.—Note on the graphic treatment of the Lamont-Frolich formula for induced magnetism, by Prof. S. P. Thompson. The formula referred to is $N = \bar{N}\,\frac{Si}{Si+b}$; where $\bar{N}$ = total induction when saturated, N = induction due to Si ampere turns, and b = value of Si which makes $N = \frac{1}{2}\bar{N}$. Simple geometrical constructions are given for plotting the curve when $\bar{N}$ and b are known, and for finding $\bar{N}$ and b when two pairs of values of N and Si have been determined. The use of the formula is shown to be justified in practice, for, as pointed out to the author by Prof. Perry, the curves connecting permeability, μ, and induction, B, are straight lines from B = 7000 to B = 16,000, between which dynamos are usually worked. A method of predetermining $\bar{N}$ and b is given for magnetic circuits of known form and materials, thus removing the objection often urged against the above formula, viz. that it involves two constants which had to be determined after the magnet was made.

Mathematical Society, May 10.—Sir J. Cockle, F.R.S., President, in the chair.—Mr. E. B. Elliott communicated a fourth paper on cyclicants or ternary reciprocants and allied functions.—Mr. Cook Wilson gave a sketch of some theorems on parallel straight lines, together with some attempts to prove Euclid's twelfth axiom. Messrs. Elliott, Buchheim, and Prof. Henrici, F.R.S., took part in a lengthened discussion of the paper.—The following were taken as read:—On the flexure and the vibrations of a curved bar, by Prof. H. Lamb, F.R.S.—On the figures formed by the intercepts of a system of straight lines in a plane and on analogous relations in space of three dimensions, by S. Roberts, F.R.S.—On Lamé's differential equation; and stability of orbits, by Prof. Greenhill.

Entomological Society, May 2.—Dr. D. Sharp, President, in the chair.—Dr. P. B. Mason exhibited an hermaphrodite specimen of *Saturnia carpini*, from Lincoln, and another specimen of the same species with five wings, bred at Tenby.—Herr Jacoby exhibited female specimens of *Chrysomela japana*, collected by Mr. J. H. Leech in Japan, and called attention to a sexual structure in the middle of the abdominal segment.—Mr. Adkin exhibited a variety of *Eubolia bipunctaria*, taken at Box Hill.—Mr. W. F. Kirby exhibited, for Dr. Livett, a curious discoloured female specimen of *Ornithoptera minos*, Cramer.—Mr. H. Goss exhibited, for Mr. W. Denison-Roebuck, a number of specimens of an exotic species of bee obtained by the Rev. W. Fowler, of Liversedge, from split logwood. The cells or pouches were very irregular and rough, and altogether unlike those of any known British species.—Dr. J. W. Ellis read a paper entitled "Remarks on the British Specimens of *Aphodius melanostictus*, Schmidt," and he exhibited a number of specimens and drawings of this species and of *Aphodius inquinatus*, F. A discussion ensued, in which Dr. P. B. Mason, Dr. Sharp, Mr. Champion, and Dr. Ellis took part.—Mr. E. Meyrick communicated a paper on the Pyralidina of the Hawaiian Islands, the material for which paper consisted principally of the collection of Lepidoptera Heterocera formed by the Rev. T. Blackburn during six years' residence in the Hawaiian Islands. Mr. Meyrick pointed out that the exceptional position of these islands renders an accurate knowledge of their fauna a subject of great interest. He stated that, of the fifty-six known species of Hawaiian Pyralidina, nine had probably been introduced through the agency of man in recent times; but he believed the remaining forty-seven to be wholly endemic: of these latter the author referred twenty-six species to the *Botydidæ*, twelve to the *Scopariadæ*, four to the *Pterophoridæ*, three to the *Crambidæ*, and two to the *Phycitidæ*. Dr. Sharp, Mr. McLachlan, Dr. Mason, and Mr. E. B. Poulton took part in the discussion which ensued.

PARIS.

Academy of Sciences, May 14.—M. Janssen, President, in the chair.—On diamagnetism, by M. Mascart. In connection with M. Blondlot's recent communication describing an experiment on the apparent diamagnetism of a solution of the perchloride of iron in a more concentrated solution of the same substance, it is pointed out that in 1845 Faraday showed that the action of the magnetic forces on a body depends on the medium in which it is plunged, as it results from the difference of their coefficients of magnetic induction. If the intensity of magnetization remains proportional to the magnetizing force, which is the case with all diamagnetic and slightly magnetic bodies, the theory then shows that the magnetizm on the surface of the body in question changes its sign when the outer medium has a high coefficient.—Remarks accompanying the presentation of a map of Massaya in Abyssinia, by M. d'Abbadie. Attention was drawn to some cartographic improvements introduced into this map by the author with the view of rendering the nomenclature more distinct, and more in accordance with the local pronunciation of geographical names. In all cases such foreign descriptive terms as *Ras*, *Jebel*, &c., give place to their equivalents *Cape*, *Mount*, &c.—Fluorescence of cupriferous lime, by M. Lecoq de Boisbaudran. After calcination in the air, carbonate of lime containing a little oxide of copper yields a substances which gives in vacuum an extremely bright green fluorescence. No spectral rays have been observed. When calcination takes place in hydrogen, instead of the green fluorescence, a more or less pink or reddish light is obtained, at times somewhat intense, but always greatly inferior to the green fluorescence.—Observations of the new planet 277, discovered on May 3, at the Observatory of Nice, by M. Charlois. The observations extend over the period from May 3 to May 9, when the planet appeared to be of the thirteenth magnitude.—Observations of the same planet are recorded for the period May 5–12, taken by M. Trépied, at the Observatory of Algiers.—Observations of the channels in Mars, by M. Perrotin. Some important modifications are described, that have taken place in these appearances since they were first observed by the author in 1886. The triangular continent, somewhat larger than France (the Lybia of Schiaparelli's map), which at that time stretched along both sides of the equator, and which was bounded south and west by a sea, north and east by channels, has disappeared. The place where it stood, as indicated by the reddish-white tint of land, now shows the black, or rather deep blue colour of the seas of Mars. The Lake Mœris, situated on one of the channels, has also vanished, and a new channel, about 20° long and 1° or 1°·5 broad, is now visible, running parallel with the equator to the north of the vanished continent. This channel forms a direct continuation of a previously existing double channel, which it now connects with the sea. Another change is the unexpected appearance about the north pole of another passage, which seems to connect two neighbouring seas through the polar ice.—Action of hydrochloric acid on the solubility of stannous chloride; hydrochlorate of stannous chloride, by M.

Engel. It is generally assumed that the solubility of stannous chloride in water increases in the presence of hydrochloric acid. But the experiments here described show that this is the case only when the quantity of acid added to the saturated solution of the chloride attains a certain value. The hydrochlorate of stannous chloride, here also described, has for formula, $SnCl_2 + HCl_2 + 3H_2O$. It crystallizes and melts at about $-27°$.—On the existence of a pyrophosphorous acid, by M. L. Amat. The existence here demonstrated of this body is a brilliant verification of the theory of Wurtz regarding the constitution of the phosphorous and hypophosphorous acids.—Essay on the equivalents of the simple bodies, by M. Delauney. Taking as unity the equivalent of hydrogen, the equivalents of the simple bodies may be obtained by the expression, $\frac{N}{3}\sqrt{5^2 - n^2}$, where N and n are integers, the values of n being obviously restricted to 0, 1, 2, 3, or 4. According to these several values of n the elementary bodies are disposed in so many family groups, from which chlorine alone is excluded, while its neighbour, bromine, appears to belong to as many as three of the groups. This classification seems natural, the first family supplying the true metals—copper, gold, lead—below which, in the descending scale, the fifth family corresponds to the alkaline metals and metalloids. From all this is deduced a curious molecular theory based on the assumption of a primitive molecule formed of six atoms. Around one of these the other five describe circles with radii 1, 2, 3, 4, 5, all moving in the same plane, and the central atom revolving round its own axis perpendicular to the plane. The atoms at the distances 1, 2, 3, 4 revolve in the same direction as the central, the outer in the contrary direction, the molecule thus constituting a sort of astronomic system, infinitely small, but analogous to the stellar groups. All these considerations go to confirm in principle, if not in fact, the views of those chemists who hold that all the simple bodies are ultimately reducible to one—that is, hydrogen.—Researches on the synthesis of the albuminoid and proteic substances, by M. P. Schutzenberger. Having completed his analytical studies of albumen, fibrine, caseine, gelatine, and other proteic substances, the author has now begun the study of their synthesis. In this paper the first results are given, showing that the leuceine obtained by the synthetic process is identical with that yielded by decomposition.

BERLIN.

Physiological Society, April 27.—Prof. du Bois-Reymond, President, in the chair.—Dr. Blaschko spoke on the development of horny tissue. Between the rete Malpighii and the corneous layer (stratum corneum) of the epidermis two layers are found—the stratum granulosum and the stratum lucidum—in which the cells of the rete, produced karyokinetically, must undergo their conversion into the epidermal cells of the stratum corneum. The speaker confined himself first to a consideration of the granules of the stratum granulosum, about which most widely different views have been advanced by various writers. They have been regarded as consisting of fat, cholesterin, amyloid substance, proteid, keratin, and hyalin; and further as fluid, semi-fluid, or solid. Dr. Blaschko has satisfied himself that the granules are not fluid, but that they contain more water than the cells of the epidermis. He has further proved by employing all the chemical reactions which are characteristic of such different substances as fat, cholesterin, proteid, &c., that the granules cannot be regarded as composed of any of the above. The curious colour they assume when stained with hæmatoxylin, and their behaviour with chemical reagents, shows that their proper place is one intermediate between albumen and keratin; the speaker hence proposed to give the name of prokeratin to the material of which the granules are composed.—Dr. Klaatsch had made a series of preparations from the skin of monkeys, by which he shows that it is possible, by using various colouring-matters, to give different colours to the stratum lucidum and stratum corneum in one and the same specimen, thus making it easy to distinguish these layers each from the other and from the stratum granulosum. He shows further that in the skin of monkeys, as in that of man, alternating elevations and depressions are met with; the former, or gland-hillocks, cover the glands of the skin, while the latter, or folds, are joined by tense bundles of connective-tissue passing through the rete, and thus forming an attachment for the skin. Finally, and in the third place, the preparations showed that the nuclei of the cells in the rete are still here and there recognizable in the stratum corneum as spaces which are probably formed by a disappearance of the nuclear substance, the nuclear membrane being persistent.—Dr. R. Schneider has carried on a series of researches, extending over nearly every class of animals, on the absorption of iron and on its occurrence as oxide in the organs and tissues of the animals. Up to the present time all the animals examined, whether living in water, mud, or underground, have contained oxide of iron; which was detected, using all due precautions, by employing ferrocyanide of potassium and dilute hydrochloric acid. The speaker gave an account of the behaviour of single animals taken from the Protozoa, Cœlenterates, Worms, Arthropods, Gasteropods, Fishes, and Amphibia. Among Vertebrates, oxide of iron was found in the cells of the alimentary canal, in the liver and spleen, occasionally in the kidneys and teeth, and in Proteus it occurred throughout the whole skeleton. Among the Invertebrates oxide of iron was found to occur in the cells of the liver and intestine, in the respiratory organs, the shells and chitinous envelopes. The oxide occurred chiefly in the protoplasm of the cells, but also frequently in the nuclei. It is impossible here to enlarge further upon the interesting details of which Dr. Schneider supplied an extended series.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Theoretische Geologie: Dr. E. Reyer (E. Schweizerbachsche).—Practical Lessons in the Use of English: M. F. Hyde (Heath, Boston).—The Origin of Floral Structures: Rev. G. Henslow (Kegan Paul).—The Baths and Wells of Europe, 3rd edition, revised: J. Macpherson (Stanford).—Jahrbuch der Naturwissenschaften, 1887–88: Max Wildermann (Herder, Freiburg).—A Manual of General Pathology: J. F. Payne (Smith, Elder).—Practical Zoology, 2nd edition: Marshall and Hurst (Smith, Elder).—Tropical Africa: H. Drummond (Hodder and Stoughton).—A Manual of Practical Assaying, 6th edition: J. Mitchell; edited by W. Crookes (Longmans).—Hand-book for the Stars, 4th edition: H. W. Jeans; revised by W. R. Martin (Longmans).—Descriptions of New Indian Lepidopterous Insects from the Collection of the late Mr. W. S. Atkinson; Part 3, Heterocera (continued): F. Moore (Calcutta).—Memoirs of the Geological Survey of India, Palæontologia Indica, ser. xiii., Salt Range Fossils, vol. i. Part 7: W. Waagen (Trübner).—Beiträge zur Kenntniss der Nagelfluh der Schweiz: Dr. J. J. Früh (Williams and Norgate).—Plotting, or Graphic Mathematics: Dr. R. Wormell (Waterlow).—Hylomorphism of Tnought—Being Part 1 Theory of Thought: Rev. T. Q. Fleming (Williams and Norgate).—Transactions of the Society of Naturalists of Cracow University, 1887.—Mémoires de la Société de Physique et d'Histoire Naturelle de Genève, tome xxix. seconde partie (Genève).—Brain, April (Macmillan).

CONTENTS.

THURSDAY, MAY 31, 1888.

AL-BÎRÛNÎ.

Al-Bîrûnî's India: an Account of the Religion, Philosophy, Literature, Chronology, Astronomy, Customs, Law, and Astrology of India about A.D. 1030. Edited in the Arabian original by Dr. Edward Sachau. (London: Trübner and Co., 1887.)

IT has often been said that India has no history and no historians. We look in vain through the ancient Sanskrit literature for any Herodotus or Thucydides. The very idea of chronicling the events of the day or gathering the recollections of the past seems never to have entered the Hindu mind, and their ancient chronology is hardly more than astronomical mythology. The historical growth of Indian literature, religion, and philosophy would indeed have remained a perfect riddle but for the few glimpses which we are able to catch of the real history of the country through other nations which were brought in contact with it. These are the Greeks, the Chinese, and the Arabs, whose successive accounts run like three broad bands of longitude across the ill-defined map of ancient India.

The Greeks do not tell us very much of what they saw of India, either before or after Alexander's invasion. We may indeed gather from Hecatæus (B.C. 549-486) that India existed, and that its chief river, the Indus, had a name of Sanskrit origin. We know, therefore, that Sanskrit was the spoken language of India in the sixth century B.C. But even that name had clearly passed through Persian channels before it reached Hecatæus, for it is only in Persian that the initial *s* of *Sindhu*, the river, could have been changed into *h*, and afterwards been dropped. Herodotus also mentions some Indian names—such as the *Gandarii*, the *Gandhâras* of the Veda—which clearly show that at his time the peoples and rivers and mountains of India had names which find their explanation in Sanskrit only. With Alexander's expedition we might have hoped that the full light of history would have burst upon India. But most of the works written by Alexander's companions have been lost, and even the work of Megasthenes, who stayed as ambassador at Palimbothra, the modern Patna, at the court of King Sandracottus, has been preserved to us in fragments only. Still the date of Sandracottus, in Sanskrit Chandragupta, has proved the sheet-anchor of ancient Indian chronology, and has once for all fixed the date of Chandragupta and of his grandson, the great Buddhist monarch Asoka, in the fourth and third centuries B.C.

The next witnesses to the actual state of political, social, and religious life in India are the Chinese. Buddhism had been adopted as a third State religion in China in the first century A.D. From that time the religious intercourse between China and India was never entirely interrupted. Buddhist priests travelled from India to China, and pious pilgrims went from China to India as the holy land of their religion. Some of these pilgrims have left very full descriptions of what they saw and did in India, the most important being those by Fa-hian (399-414 A.D.), Hiouen-thsang (629-645), I-tsing (673-695), and Khi-nie, who visited India in 964, at the head of 300 pilgrims. Most of these travels and diaries have been translated into French and English by Remusat, Stanislas Julien, Beal, and Legge; and they give us a picture of Indian life during the Middle Ages of which we should have had no idea if we had been restricted to Indian sources alone.

More important, however, than the descriptions of these Greek and Chinese authors, is the work to which we wish to call attention—namely, the account of India written by Al-Bîrûnî in the year 1030 A.D., and now published for the first time by Prof. Sachau, of Berlin. Al-Bîrûnî was a native of Khwârizm, the modern Khiva, born in 973. He had devoted himself to the study of astronomy and philosophy, and when Khiva was taken by Sultân Mahmûd of Ghazna in 1017, Al-Bîrûnî was induced to accompany him to India. The famous Avicenna, *i.e.* Abu Ali Ibn Sina, declined the same honour, and remained at home. During the thirteen years that Al-Bîrûnî spent in India, he devoted himself sedulously to the study of Sanskrit, and Sanskrit literature. He does not use the name of Sanskrit, but calls the language of India, both literary and vernacular, Hindi, *i.e.* Indian; the fact being that Sanskrit was not yet used as a proper name of the ancient literary idiom, but only as an *epitheton ornans*. What progress Al-Bîrûnî made in his studies seems somewhat doubtful. It was formerly supposed that he translated not only from Sanskrit into Arabic and Persian, but likewise from Arabic and Persian into Sanskrit. But Dr. Sachau has clearly proved that his knowledge of Sanskrit was far too elementary to enable him to perform such tasks by himself. He shows that he depended chiefly on the assistance of his pandits, like many Sanskrit scholars of more recent times, and that all we can assert with safety is that he was able to direct and to check their labours. With all that, Al-Bîrûnî was a most exceptional man for his time, a man of wide sympathies, a true philosopher, and acute observer. The very idea of learning a foreign language, except perhaps Persian and Turkish, never entered the head of a Muhammedan. His weapon was the sword, not the pen. Al-Bîrûnî, however, to quote Prof. Sachau's words, "convinced that those who want to meet the Hindus on the battle-ground of intellectual warfare, and to deal with them in the spirit of justice and equity, must first learn all that is peculiar to them in manners and customs as well as in their general modes of thought, produced a comprehensive description of Indian civilization, always struggling to grasp its very essence, and depicting it with due lights and shades, as an impartial spectator." The title of the book tells its own story: "*An accurate description of all the categories of Indian thought, as well those which are admissible, as those which must be rejected.*"

The existence of this work of Al-Bîrûnî's has been known for many years, and Sanskrit scholars have long clamoured for its publication and translation. Their appetite was first whetted by the specimens which Reinaud published in 1845 in his "Fragments Arabes et Persans relatifs à l'Inde," and some years later in his invaluable "Mémoire sur l'Inde" (1849). When Reinaud declined to undertake the editing of the whole text of Al-Bîrûnî's "Indica," Woepcke and MacGuckin de Slane undertook the difficult task. The former, however, died;

the latter began to feel the approach of old age, and the prospect of a speedy termination of this important undertaking became more and more doubtful, when, in the year 1872, a young German scholar, Dr. Sachau, boldly stepped into the breach, and promised to devote all his time to this great enterprise. After fifteen years of hard work he has redeemed his pledge. He has given us the Arabic text of Al-Bîrûnî, and he is now engaged in printing an English translation of it. We doubt whether anyone could have been found so well qualified for the task. Dr. Sachau has long been known as a hard-working, honest, and thoroughly sound scholar. He stands in the first rank among the students of Arabic and Persian, and he possesses, at the same time, a fair knowledge of Sanskrit. He is now one of the brightest stars in the University of Berlin, and has lately been appointed there as Director of the newly-founded Imperial School of Oriental Languages. He was well prepared for his task by having previously published another work of Al-Bîrûnî's, the text and English translation of "The Chronology of Ancient Oriental Nations." Few people can appreciate the enormous difficulties of publishing for the first time an Oriental text like that of Al-Bîrûnî. Dr. Sachau was, no doubt, more fortunate than his predecessors in securing a manuscript of Al-Bîrûnî's, belonging to M. Schefer, which professes to have been copied from a copy in the handwriting of the author. But even thus the labour of editing and translating such a text, which had never been edited and translated before, was enormous. When speaking of the difficulties which he had to overcome in editing Al-Bîrûnî's chronological work, Dr. Sachau writes: "I have boldly attacked the sometimes rather enigmatic style of the author, and if I have missed the mark, if the bewildering variety and multiplicity of the subject-matter have prevented my reaching the very bottom of every question, I must do what more or less every Oriental author does at the end of his work —humbly ask the gentle reader to pardon my error and correct it." There is the true ring of the *bonâ fide* scholar in this. No one is nowadays considered a real Oriental scholar who has not won his spurs by an *editio princeps*. After a text has once been constituted by a comparison of manuscripts more or less faulty; after a translation has once been accomplished, however imperfect, it is easy enough to print a new so-called critical edition, or a new so-called improved translation. But the scholars who take the first, and the scholars who take the second, step belong to different races. They differ as Columbus who discovered America differs from the traveller who now crosses the Atlantic in seven days. "Generations of scholars," as Dr. Sachau says, "have toiled to carry the understanding of Herodotus to that point where it now is, and how much is wanting still!" To expect, therefore, that Al-Bîrûnî's text, as edited here for the first time, or its translation, should be free from mistakes would only show a complete ignorance of the conditions under which Oriental scholars have to work. There may be hereafter better editions of Al-Bîrûnî; there never can be one so creditable to its author as this *editio princeps*. We could have wished that a work of such importance to students of Indian history had been carried out by an English scholar. But, failing that, we have at least the satisfaction that the expense of publishing the Arabic original of the "Indica" has been generously defrayed by the Indian Government, following in this respect the noble example set by the patron of Al-Bîrûnî himself, the powerful Sultân Mahmûd of Ghazna.

THE SCIENTIFIC WRITINGS OF JOSEPH HENRY.

The Scientific Writings of Joseph Henry. Two Vols. 8vo, pp. 1082. (Washington: Smithsonian Institution, 1886.)

UNDER the above title, two handsome volumes have recently been published by the Smithsonian Institution, Washington, containing the papers published by its late distinguished Secretary in various scientific serials through the long period of fifty-four years. It is characteristic of the man that, although for thirty-two of those years he had almost unrestricted command of the publishing resources of that great institution, not one of his papers was given to the world through the medium of the "Smithsonian Contributions" or "Miscellaneous Collections," or in any way at the expense of its funds. They range over a great variety of subjects, chiefly in electrical physics and meteorology, and in date from 1824 to 1878.

As may be inferred from the earlier of these dates, when Faraday was still an assistant to Sir Humphry Davy, in the laboratory of the Royal Institution, and Henry a private tutor in a family at Albany, New York, many of these papers are reprinted for their historical interest rather than for their present scientific value; but his fellow-countrymen, in acknowledging Faraday's preeminence, delight to point out in how many particulars Henry walked *pari passu* with him in the then nearly untrodden paths of electro-magnetism, under immense relative disadvantages. As early as 1835, Henry, then a Professor at Princeton, New Jersey, connected his residence with his laboratory in the Philosophic Hall by a telegraph, in which the galvanic circuit was completed through the earth—probably the first realization of that familiar property on which all our telegraph circuits are now dependent. It was a little later (in 1842) that he showed the writer of this short notice, under promise of secrecy, an experiment which at the moment greatly interested him. A long bar of iron was wrapped in a coil or ribbon of copper, half an inch wide; two copper wires, each terminating in a small ball, were soldered to the bar. On holding these balls to the ears, and transmitting a strong current through the coil, a very distinct musical note was heard each time the current was made or broken. He narrowly missed forestalling Faraday in the great discovery of producing electric currents by the rotation of an electro-magnet or movement of its armature. Henry caused an electro-magnet of unusual power to be constructed in August 1831, with a view to realizing his conceptions on this subject. He was at the time accidentally interrupted in pursuing his experiments, and did not resume them until May or June 1832; and in the meantime (in February 1832) Faraday had made his independent discovery.[1] As early as 1843, Henry proposed "a new method of applying the instantaneous transmission of an electrical action to determine the time of the passage of a (cannon) ball between two screens, placed

[1] *Philosophical Magazine*, April 1832.

at a short distance from another in the path of the projectile," and contrived a self-recording apparatus reading to the one-thousandth part of a second. As at that time Hutton and the ballistic pendulum reigned supreme—and this is not an experiment easily made in a laboratory—it does not appear that he carried it out. Perhaps the most elaborate of his numerous researches is that on the transmission of sound in relation to fog-signalling, carried on at the expense of the United States Lighthouse Board for several years from 1865 onwards, concurrently with those on which Prof. Tyndall was at that time engaged for the Trinity Board. That these distinguished men did not always meet with the same effects, or draw the same conclusions from them, is but a natural consequence from the extreme complexity of the phenomena.

The great work of Prof. Joseph Henry's life—in which his strength and calmness of judgment, his high-minded independence and self-effacement, enabled him to achieve the highest results—was the organization of the Smithsonian Institution upon its present liberal basis, in the face of not a little opposition from persons of more contracted views.

"These I's are *egos*, and not *oculi*," is a line from some forgotten squib which he was wont to quote when self-interest seemed to obscure the only interest precious to him—that of science in its widest scope, and the advancement of human knowledge. He lived to see the wisdom of his policy gratefully acknowledged by his countrymen and the scientific world. Although a very fertile inventor, and the author of many ingenious contrivances now in use to facilitate the working of the electric telegraph, he never patented anything. In his own words, he "did not consider it compatible with the dignity of science to confine the benefits which might be derived from it to the exclusive use of any individual." The expression is not carefully chosen; it simply means that he declined to derive selfish advantage from his discoveries. A very brief and modest statement by himself of what these were in relation to the electro-magnetic telegraph is reprinted in vol. ii. from the Smithsonian Annual Report for 1857. In collecting and reprinting these papers, the Institution has raised a worthy monument to Henry's memory, and made a valuable contribution to the history of physical science.

J. H. L.

AN ELEMENTARY TEXT-BOOK OF PHYSIOLOGY.

An Elementary Text-book of Physiology. By J. McGregor Robertson, M.A., M.B., Senior Assistant in the Physiological Department, University of Glasgow. 350 pp. (London: Blackie and Son, 1887.)

IN compiling this volume the author has sought to "present the essential facts and principles of physiology, not in a series of disconnected paragraphs, but woven into a continuous story." This being so, we look for a readable book rather than for the more empirical treatise nowadays predominant; and the the success of the work must consequently depend, in the main, upon the manner in which the narrative is strung together. That the book really is a readable one there can be no doubt, and for style and general accuracy it is very satisfactory. When we consider the method of arrangement adopted, however, we must confess that it is disappointing. The author lays it down as a tenet that "we cannot properly understand the physiology of the human body without reference to the form and build, . . . and thus we shall have to note the main anatomical facts regarding a part of the body before going on to consider the work which that part does." Very proper, and true to the letter. In spite of this, however, the reader is led straight away into a consideration of the chemical constitution of the body as a whole. Surely it would be more logical to treat of the constitution of the several structural elements in order of presentation, deferring the more general statements for a final *résumé*. A similarly dangerous position is approached when the writer deals with structure itself. Chapter II. is devoted to "Elementary Structures," that is to say, the author discusses the structural unit before entering upon a consideration of those organs and tissues which are its aggregates. This is an old grievance, and all experience shows that this method, though at first sight apparently natural, is in reality seductive, if not illogical. It is fair to the author to state that he does not adopt it throughout. In view of it, however, the following statements are the more unfortunate: "cells are little masses of a jelly-like material"; "usually the cell has an outer covering or membrane, called the *cell-wall*"; "from little nucleated masses of protoplasm cells are produced, and then from cells all the other textures of the body are derived."

As the work is of a readable character, we expect, furthermore, to find comparisons and illustrations drawn from the experience of daily life, and in this we are not disappointed. Stock comparisons, like that of the human body with the steam-engine, come in as a matter of course, and in his choice of novel ones the author has been very successful. Nothing can, however, be more easily overdone than this. If, for example, the human eye is compared with the photographer's camera, care ought to be taken to point out in what the two differ, especially when considering the lens in accommodation. This has not been done.

Taking the book as a whole, the author is to be congratulated, and especially so upon his treatment of certain leading topics—notably that of diet. By far the weakest parts of the work are those devoted to histology. The interminable striped muscle question is most feebly treated, and who but the author is to know what is meant by the words "the nerve-tubes end, it has been seen, in the (muscle) fibres"? The description of a secreting gland generally given is so worded as to imply that the "basement membrane" is a leading, if not the chief, constituent thereof. These and other defects referred to in the sequel demand immediate attention, and we would fain see the elimination of such old heresies as the capillary or "hair-like vessel" and the transmission of "messages" along the nerve-fibre. There would appear to be a fatality in the persistency with which teachers of a certain class continue to thrust these and similar stumbling-blocks in the way.

This volume is confessedly designed for the "requirements of candidates for the examinations of the Science and Art Department and of the Local Examination

Boards of the Universities," and the syllabus of the first-named body is appended to it. The book thus finds a place among the legion of cram-books which now threaten to overwhelm us. The majority of these are, as everybody knows, notoriously bad, and readers of NATURE will not need to be reminded that strong measures are being proposed for the purpose of checking the evil consequent upon their multiplication, and that of elementary text-books in general. Conspicuous among these is the recent proposal to establish a Publication Committee, whose members shall sit in judgment on all text-books, with power to suppress or modify at will. With this suggestion we have no sympathy: it is unscientific in principle, while its adoption would tend towards the establishment of a conservatism and narrow cliquism greatly to be dreaded. The introduction of such a measure would, in our opinion, only serve to strengthen that spirit of popery which threatens to invade certain branches of science in our own lands. The evil will assuredly work it's own end, and, so far as professed cram-books are concerned, the publication of works of such relatively general excellence as the one before us cannot fail to be a far more potent remedy—a more natural one it most certainly is.

Chief among the defects referred to above, as standing in need of revision, are the following. Too little importance is throughout attached to the sources and evolution of heat in the animal economy; the parts played by the muscles and liver need especial comment, and we note that in the table of gains and losses given no count is taken thereof. The functional importance of the diaphragm in the mechanism of respiration is over-stated; on the other hand, that of the withdrawal of water by the kidney is under-stated as a fundamental of excretion. The distribution and function of glycogen are insufficiently noted. The references to non-nucleated cells (p. 26), and to the comparative anatomy of the central nervous system, might well be excised; while the long *résumé* (pp. 255–58) of brain-functions given might be advantageously replaced by a more concise description of the actual facts determinable in a typical case. The relegation (p. 48) of the sutures of the cranial bones to the category of "imperfect joints" is groundless.

Numerous illustrations are employed, and of these many are new and highly satisfactory. Figs. 42, 45, 80, and 118, are, however, little short of useless. It is a fact, and not a "*view*," that "the life of the body is the sum of the lives of the individual cells composing it," and it cannot be said that with the study of the anatomy of the lungs we begin our "*view*" of the means of purification of the blood.

OUR BOOK SHELF.

Evolution and its Relation to Religious Thought. By Joseph Le Conte, Professor of Geology and Natural History in the University of California. (New York: D. Appleton; London: Chapman and Hall, 1888.)

THE title of this book is somewhat misleading. The work is in effect a concise account of evolution and its principal evidences, contained in 253 pages, supplemented by 82 pages giving the author's views on the relation of evolution to materialism, which he rejects, and to several religious questions, of which we can only say in these columns that they are dealt with in a candid spirit, on the basis that the law of evolution is thoroughly established, and is indeed "a necessary condition of rational thought." The exposition of evolution is well-planned, the main problems and their significance and the modes of proof being clearly and simply set out, so that the general reader with a modicum of knowledge of natural history may realize them to a considerable extent. These chapters are illustrated by a number of well-selected comparative figures, such as the fore and hind limbs of typical vertebrates, the evolution of the horse family, and the vascular system and brain of vertebrates. Prof. Le Conte cordially accepts Mr. Romanes's "physiological selection" as the most important advance in the theory of evolution since Darwin; and it is significant that this new view should have already found a place in a popular work written by a man of science. It is, however, a little hazardous to apply with so much confidence a theory still requiring proof; and this appears to lead the author to put forward a still less proven idea, not new it is true, that the steps of evolution at certain times become comparatively rapid, so that there may be few generations, or perhaps only one, between successive species. Some of the author's statements are undesirably broad, as when he says, "All vertebrates, and none other, have a number of their anterior vertebral joints enlarged and consolidated into a box to form the skull, in order to inclose and protect a similar enlargement of the nervous centre, viz. the brain." Of course the author is excluding Amphioxus, but he does not say so. Similarly the statement that "by extensive comparison in the taxonomic and ontogenic series the whole vertebrate structure in all its details in different animals may be shown to be modifications one of another" is a little vague. But on the whole the book is sufficiently accurate, and should prove useful.

Outlines of Qualitative Analysis. By George W. Slatter, Science Master at the Salt Schools, Shipley. (London: Thomas Murby, 1888.)

THIS further addition to the already large number of books on elementary analysis is compiled from the author's laboratory notes issued to his students in the Salt Schools. Most teachers appear to have a few particular methods of their own, and the custom of writing a book to embody them seems to be fast gaining ground. With the matter of Mr. Slatter's book one can find very little fault; but at the same time, except for the use of his own students, one can scarcely see any reason why another book should be presented to the public, when all the facts, in a much more complete form, are already given in most of the larger laboratory guides now in vogue.

The majority of the methods recommended are certainly well-tried and convenient ones; and a very good point is the trouble taken in explaining the theory of the analysis tables. Exception, however, may be taken to Mr. Slatter's mode of separating antimony and tin by use of Marsh's apparatus: while theoretically good, experience shows that accidents are liable to happen, and this is especially the case among young students; hence the platinum and zinc electrolytic method is more frequently preferred. The author seems also to have a predilection for the use of nitro-hydrochloric acid in dissolving the sulphides of nickel and cobalt in Group III., while there can be no doubt that potassium chlorate and hydrochloric acid work far better, there being no danger of leaving nitrates in the solution.

The method of analyzing phosphates is one which works very well, and is probably the best known. Similarly the analysis of double cyanides by ignition with ammonium nitrate and sulphate is the one which in the writer's opinion is both the simplest and gives most satisfactory results.

A. E. T.

The Land of the Pink Pearl. By L. D. Powles. (London: Sampson Low, 1888.)

MR. POWLES served for some time as a circuit justice in the Bahama Islands, and in the present volume he communicates the impressions produced upon him both by the islands themselves and by their inhabitants. He makes no profession of an intimate knowledge of any branch of science, so that the work contains few elements of interest that call here for special notice. We may say, however, that the book is written in a lively and agreeable style, and that the author has brought together much useful general information about what he calls "this obscure corner of Her Majesty's dominions." The most valuable passages are those in which he deals with the relations between the white and the coloured population. His statements on this subject are certainly not lacking in vigour, for he speaks of the African race in the Bahamas as being "ground down and oppressed in a manner which is a disgrace to British rule." When Mr. Powles went to the Bahamas, he had an impression that negroes were "intended by Nature to be kept in subjection by the whites." Experience, however, led him to modify this extravagant notion. Referring to the statement, so often made, that "it is impossible to produce anything by free negro labour," he sensibly suggests that "perhaps if the Imperial Government would establish an agricultural college and give the coloured race in the Bahamas a fair chance, we might see a different state of things." The physical deterioration of the coloured people is, he thinks, sufficiently accounted for by their wretched food and by the unhealthy nature of the places in which they are compelled to live. Curiously enough, Africans in the Bahamas retain their original tribal distinctions; and Mr. Powles says that every August some tribes elect a queen whose will on certain matters is accepted as law.

A Treatise on Alcohol, with Tables of Spirit-Gravities. By Thomas Stevenson, M.D. Second Edition. (London: Gurney and Jackson, 1888.)

THE present edition of this useful little work, originally published under the title of "Spirit-Gravities," contains a critical account of the various determinations of the specific gravity of alcohol, and introduces the most recent investigations—those of Messrs. Squibb—on this subject. These investigations do not, however, affect the accuracy of the alcoholometric tables, which are therefore reprinted unchanged.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Dispersal of Seeds by Birds.

IT should be borne in mind by readers of NATURE in various parts of the world that many facts bearing on this matter may be collected with very little trouble. At Mr. Thiselton Dyer's suggestion I take this opportunity of supplementing my letter to Mr. Botting Hemsley (NATURE, vol. xxxviii. p. 40).

The frigate-birds, petrels, gannets, boobies, &c., that frequent in numbers the guano islands of the Pacific, will present opportunities of investigating this subject rarely found elsewhere. Not only the crops, but also the feathers and feet should be examined, since seeds have been sometimes found adhering to sea-birds that have been sitting on broken eggs. The industries connected with the ocean-ranging mutton-bird in Bass's Straits, and with the grebe in South America, may afford other opportunities. The seal-fisher in the Southern Ocean, and the sportsman on some remote coral islet, the voyager around the Cape, and the lighthouse-keeper in southern climes, these and many others might take a practical interest in this subject. It is important that not only should the seeds and fruits be preserved and sent to Kew, but that the species of bird should be known; and for this purpose, where there is any doubt, the wing or head of the bird might be also sent. H. B. GUPPY.

May 27.

Nose-Blackening as Preventive of Snow-Blindness.

As a partial answer to Prof. Ray Lankester's inquiry on nose-blackening as preventive of snow-blindness, may I offer some observations which I have made in my many wanderings in the higher Alps in early summer, when I have necessarily had much experience of the effects of snow on the human body?

But first I should like to draw attention to a letter of the Hon. Ralph Abercromby in NATURE (vol. xxxiii. p. 559), which he was kind enough to send me, relating some experiences on nose and face blackening in Morocco to prevent sand glare, in Fiji to prevent water glare, and in Sikkim to prevent snow glare. It was very curious that the Fijians, who ordinarily painted their faces white and red for ornament, would, before going fishing on the reefs in the full glare of the sun, blacken them. Mr. Abercromby draws attention very naturally to "the strange anomaly of physiological experience apparently contradicting the teachings of pure physics. Charcoal black, which is used in physical experiments as the best absorbent of every kind of heat radiation, is practically used by three races at least, to protect one of the most sensitive human organs from reflected light and heat."

Experience has, I think, sufficiently shown that snow-blindness and snow-burn, or sunburn on snow, own the same causes for their production; and, as nowadays both guides and climbers in the Alps invariably take the precaution of protecting their eyes with coloured spectacles, snow-blindness is rarely heard of. My observations are almost entirely confined to the causes of sunburn.

It will, I think, be readily conceded by Alpine climbers that sun on the snow burns more quickly than on rocks or in the heated valleys at a lower elevation. This increased power of burning appears somewhat singular when one reflects that the heat rays must be occupied in the melting of the snow, and thus rendered latent.

Iron-workers, glass-workers, and others are constantly exposed to a heat of 400° or 500° F., and yet do not become burnt; and there can be little doubt that the enormous radiation from heated rocks and valleys, in addition to the direct rays of the sun, make up an amount of heat far greater than is ever experienced on even a very sunny snow slope, and yet one does not become sunburnt. No doubt the surface of the snow reflects and disperses much heat, but certainly far less than it receives, as heat rays are absorbed and rendered latent by the snow-melting and evaporation. Experience fully corroborates this, for one may often lie on one's back and freely expose the face for long periods to the sun and yet remain unburnt. There must therefore be some other factor in sunburn than heat alone.

In discussing the subject with Prof. Tyndall, he added the very interesting and significant fact that he was never more burnt on snow than whilst experimenting with the electric light at the North Foreland Lighthouse, where there was no heat sufficient to produce such an effect.

I am aware that sometimes, in peculiar conditions of the atmosphere, the direct sun's rays will burn. I have met with some singular instances where several persons have been burnt on the same day, even in England, who had never previously suffered in that way. I am further aware that sometimes (not always) in a dead calm on a ship's deck one may be severely burnt, and that in boating on a river the same may occasionally happen. Masks and veils have been long used as a protection on snow, and are more or less successful; *brown* veils and glasses in my experience being the most efficient. As bearing upon this, I may mention that a friend of mine after an ascent on snow had an enormously swollen face, and I observed that in the general swelling there were many pits or depressions, and that each pit corresponded to a freckle: the irritating rays had been intercepted by the brown colour of the freckle. About the same time, I encountered a paragraph in the *Lancet*, saying that a German *savant* had been experimenting on the effects of sunlight on the retina, and had found that it had destroyed the visual purple of the retina, but that the action was modified by transmitting the sun's rays through various coloured glasses, and that when transmitted through *brown* glass the purple of the retina was unchanged. I have never seen any corroboration of these assertions, but they are worthy of further consideration. Stimulated by these observations, I painted my face brown with water-colour, and spent many hours on the snow of the Gorner Grat on the same day that about eighty out of a hundred people who were staying at the Riffel Alp went up to witness the first ascent of the season of Monte Rosa. In the evening everyone except myself and my daughter, who had carefully protected herself with a brown veil, was more or less severely sunburnt, whilst the remaining visitors, who had spent the day on the rocks and mountain-sides in the full sun, were untouched. Connected with this is the fact that visitors to the Engadine in winter become extremely brown, as though coloured by walnut-juice, whilst in summer, unless they go on the snow, this is not so, although of course the heat is greater. I have been there in winter and summer, and have had many opportunities of confirming this observation. Then again the very brown colour of the *châlets* is only to be seen at high altitudes where snow is, and even those parts of the *châlets* which by their position cannot receive rays reflected from snow do not become brown. And over the doors of these brown *châlets* in which the cows are kept the wood is invariably white and colourless just at that part which would always have, steaming up, the warm moist breath of the cows, and by this moisture the reflected rays would be intercepted. I think that all these observations bear upon and are related to the question raised by Prof. Ray Lankester.

I have made many other experiments and observations, but for brevity's sake I omit them, as I think I have said enough to show that the subject is a large one, and worthy of consideration. In a comment on Mr. Abercromby's letter abovementioned, Petrie says, "We should not look at the surface skin, which is constructed to bear local variations of temperature, &c., but at the delicate tissues beneath. White skin," he adds, "is translucent, but black stops out solar energy." It is possible that sunlight reflected from snow may have an influence in producing the improved health of consumptives who remain in the Engadine in winter, and Mr. Abercromby reminds me that the quality of heat which causes snow-burnt is not that which causes sun-stroke. Sun-stroke is very rare (if known at all) on mountains. Equatorial countries—Ceylon, Borneo, West Indies, &c.—are not the worst for sun-stroke; but sub-tropical and semi-tropical dry countries, such as Scinde, North-West Bengal, United States, Italy, &c.

He also says that photography is much slower in equatorial than in these latter countries. The cause undoubtedly is the absorption of violet and ultra-violet rays by water vapour, which is in excess near the Line. Photography is rapid—except for blue sky—at high altitudes. ROBERT L. BOWLES.

Folkestone, May 23.

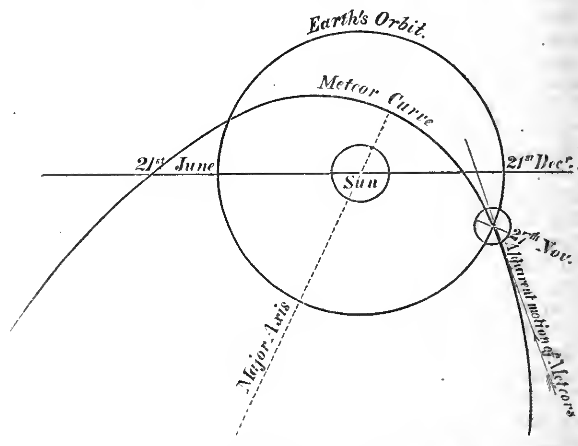
Mysterious Sky Lights.

ON turning over some back volumes of NATURE in search of information concerning the spectrum of the zodiacal light, I have discovered something which appears to be interesting and suggestive, viz. several communications describing what the writers supposed to be abnormal displays of the zodiacal light, displays occurring at the wrong time, *i.e.* near to the periods of the solstices instead of those of the equinoxes, and displays having the wrong shape, lacking the conical outline, but nevertheless nearly in the right place. The most interesting of these letters are from Mr. Maxwell Hall, and dated from Jamaica. He was so much exercised by the heterodoxy of the appearances he observed that he suggested a new theory, and illustrated it by a diagram on p. 204 of vol. vii.

He says in this letter that "for several nights lately the zodiacal light has been exceedingly bright and well-defined, and *more particularly on the nights of November* 24 *and* 27; on the evening of the 24th I found an explanation of what had often perplexed me before, viz. the existence of a faint, isolated band of light across the zenith, but as soon as it was dark in the evening, the zodiacal light was distinctly seen to *stretch across the whole sky*, forming that faint band of light previously observed; I then began to note its position, *but the best observations were made on the night of the 27th, when it was most distinct.*"

The italics in the above are mine. The dates given, November 24 and 27, 1872, are those of the remarkable meteoric shower supposed to be connected with the lost comet, Biela. The grandest display was on the 27th. May not the luminosity stretching "across the sky" have been due to sunlight reflected from the meteoric matter lying outside of the earth's atmosphere? Such a spurious zodiacal light does not demand our actual plunging into the meteoric stream producing it, but should be observable whenever such a stream exists between us and the sun, its apparent breadth varying with its actual breadth and its proximity to the earth.

On p. 85 of the same volume is a diagram of the Biela



meteor-path showing its relation to the earth's orbit and to the sun. From this, reprinted above, it is evident that the meteors should display a spurious zodiacal light at the time named, and on to that of the winter solstice, and later. In vol. xi. of NATURE, p. 115, reference is made to a letter from Mr. Hind, dated December 7, 1874, in which he points out "that the zodiacal light has been conspicuous for the last few evenings; and for several years past this phenomenon has been much more marked in December and January than about the vernal equinox."

The pages of NATURE and those of some of the older volumes of the *Gentleman's Magazine* contain many records of mysterious streaks and bands and "pillars" of light seen after sunset, variously ascribed to zodiacal light, to aurora borealis, celestial phosphorescence, &c. If I am right in assigning the spurious zodiacal light of the period above named to the Biela meteors, careful observations of such celestial luminosity in relation to other well-known meteor-streams may be very frui ful.

The Grange, Neasden. W. MATTIEU WILLIAMS.

Curious Apparent Motion of the Moon seen in Australia.

CAN any of your readers explain the phenomenon described in the following extract from a letter received from my daughter residing in Maryborough, Queensland, Australia:—

"We saw such a curious phenomenon on Sunday night, about 10.30. Miss C——, Miss H——, and I were sitting in the balcony, when we noticed the *moon apparently dancing up and down.* It is on the wane, so looked so extraordinary. The motion was visible only when she was behind a narrow stratum of cloud, and continued at intervals for thirty minutes. I felt quite seasick with watching it, and Miss H. was so frightened; she thought there might be an earthquake coming, so went to bed in her clothes to be ready for an emergency. Our house would soon fall in an earthquake, its walls are thin, and no cellars."

I presume the phenomenon is connected with the varying refrangibility of the atmosphere, perhaps arising from the mixing of hot and cold air; but should be glad of further information.

T. MELLARD READE.

Park Corner, Blundellsands, May 27.

Another Specimen of *Lepidosiren paradoxa.*

IT may interest some of your readers to know that I have lately received another specimen of this rare fish from my friend Dr. J. Barbosa Rodriguez, the energetic Director of the Museu Botanico do Amazonas at Manáos. This is the *fifth* specimen known. A short notice of the fourth specimen, an adult female of large size, caught in the Igarapé do Aterro near Manáos, also presented to me by Dr. Barbosa Rodriguez, appeared in NATURE more than a year ago (vol. xxxv. p. 343). Dr. Rodriguez published a note on that specimen in the *Jornal do Commercio* of Rio de Janeiro for October 15, 1886. I state this as his note might easily be overlooked, not having appeared in a scientific periodical.

The last specimen received was caught at Autaz near the Madeira River in September 1887; it came, Dr. Rodriguez writes, from a mud-pool, whence it issued forth wriggling on the mud during rain-storms. My friend received it dead and in a state of incipient decomposition; he did all he could to insure its preservation, but when it reached me all I could save was the skeleton and portions of the skin and tougher muscles. These I have put in strong alcohol for future study. This specimen is considerably smaller than the one previously received, being, as far as I can judge, about 0m. 400 millim. in length.

At Autaz this fish is called *Trayra-boia*, or *Turum-boia*; the latter name is onomatopœic for *Turum*, which expresses the grunt made by the fish, and *boia* means "snake." On the Rio Mahu, an affluent of the Rio Branco, Dr. Rodriguez tells me that the name of this fish in the Makuchy dialect is *Aramó*.

HENRY H. GIGLIOLI.

Royal Zoological Museum, Florence, May 22.

Dreams.

MR. R. L. STEVENSON, in his "Chapter on Dreams" in *Scribner's Magazine* for January last, brings forward one difficult point that must have puzzled many dreamers besides himself. The point is that the dreamer is often in the position of an ignorant onlooker, who, only when the plot or story is complete, sees the drift and motive of the different incidents that have been enacted before his eyes by what Mr. Stevenson calls "the Little People who manage man's internal theatre."

Perhaps it is one step further on in the puzzle to have the interpretation only vouchsafed to one after awaking; and the following example may be of some interest.

Much of my dreaming goes on in the form of reading; and it once happened to me to awake while looking at the outside of a pamphlet I dreamt I was holding. I saw it vividly enough before me; it had a mud-coloured cover, and the title was printed on it in plain Roman capitals: "Food, or the astrology of every day." "But this is nonsense," I thought; until, still having a vivid view of the title before me, I observed that the rough brown paper had been rubbed up after the word "the," and that there was a wide gap between it and the "astrology." Evidently a letter was missing, and I at once conjectured that the word had been printed "gastrology." But this I did not arrive at till I was wide awake.

I come back to Mr. Stevenson's query, "Who are the Little People?" and how comes their amazing independence of their employers?

E. H.

Strange Rise of Wells in Rainless Season.

A HOUSE near Fareham, standing in its own grounds, is principally supplied with water by two wells, about 16 feet deep. They are usually quite full in winter, and gradually empty before autumn. Owing to the small amount of rain last winter, the beginning of March found the wells with only 3 feet and 2 feet of water respectively: when, after a continuance of north-east wind, *without rain*, but with half a gale blowing, the water in these wells rose 14 feet and 12 feet.

Can you or any of your readers explain this mystery? There is a tradition in the neighbourhood that it is customary with the wells in the district to rise with a heavy gale even without rain; and a similar phenomenon has been observed before by my informant.

E. H.

May 23.

Milk *v.* Lightning.

IN Emin Pasha's letter published in NATURE (vol. xxxvii. p. 583), the Sudan Arabs are said to have a superstition that fire kindled by a flash of lightning cannot be extinguished until a small quantity of milk has been poured upon it. A similar belief seems to have existed formerly in this country. The earliest register-book of this parish contains the following note:—

"In the yeare of our Lord 1601 and uppon ye 14 day of May beinge thursday ther was great thundringe and lightninge and ye fyer descendinge from heaven kindled in a white-thorne bush growinge neere to a mudd-wall in Brook-street westward from Thomas Wake his house, it burned and consumed ye bush and tooke into ye wall about on yeard then by milke brought in tyme it was quenched and it did noe more hurt."

JOHN CYPRIAN RUST.

The Vicarage, Soham, Cambridgeshire, May 23.

The Renewed Irruption of *Syrrhaptes*.

MR. SCLATER having requested me to contribute to *The Ibis* an account of the present visitation of *Syrrhaptes* similar to that which I compiled for that journal in 1864, I would ask for information on the subject to be sent to me, and especially cuttings from foreign newspapers, the name of the publication and the date being always indicated thereon. I must add that I do trust my task will not be the unpleasant one of merely recording senseless slaughter. In 1863 the species bred both in Denmark and in Holland. There is no reason why it should not, if unmolested, breed this year in many parts of Britain. The visitations of 1872 and 1876 were of insignificant proportions, but that of the present year would seem to be of considerable magnitude, and sanguine hopes might be entertained as to the result if the malign influence of the "collector" could be neutralized or withstood.

ALFRED NEWTON.

Magdalene College, Cambridge, May 27.

"The Shell-Collector's Hand-book for the Field."

As your reviewer (NATURE, May 17, p. 51) has shown that the little book which bears the above title is certainly worth a large share of "powder and shot," I may, in all fairness, be allowed to reply to those strictures made by him which are the most unfair, and which I consider warrant a reply from me. In the first place, it is quite apparent that he has never used the "Authenticated British List" published by the Conchological Society, where he would have found *Clausilia parvula*, *C. solida*, and *Zonites draparnaldi* excluded, doubtless, on reliable authority; while *Bulimus Goodallii*, *Vertigo tumida*, and *Planorbis dilatatus* are included, also, doubtless, on reliable authority, as recognized members of the British fauna, even if they be "casuals." He has also, it is quite apparent, never read Prof. Macalister's "Introduction to Animal Morphology," where he will find it stated on p. 286 that "the operculum has always more conchiolin in its composition than the shell whose mouth it closes." He does not know, it is also quite apparent, that Pisidium and Sphærium are British fresh-water mussels, and *siphonated* British fresh-water mussels too, there being one siphon in the former and two in the latter genus (cp. the description of these genera in Westerlund's "Fauna of Sweden and Denmark"). He can scarcely know that the epiphragm has been called by some authors (as instance Macalister) the clausilium; and although recognizing this on p. 5 of my "Hand-book," I have described in a footnote to the genus Clausilium (p. 44) the only structure which we recognize to-day under that name. He does not know, it is evident, that Prof. Milnes Marshall ("Practical Zoology," p. 106) states that "the periostracum or outer layer is horny and uncalcified. To it the colour of the shell is due," and that "the middle layer" "is densely calcified, and has an opaque porcellanous appearance." And he scarcely knows that in Huxley and Martin's "Course of Elementary Instruction in Practical Biology," p. 274, the aperture of the shell is spoken of as the *peritreme* and not as the *peristome*, and that in the majority of works on comparative anatomy it is also solely mentioned under that name. I think it also my duty to tell your reviewer that the teeth-formulæ were not copied from Lankester, as he supposes, but from Woodward, and that upon comparison I find the copy correct (cp. Jeffrey Bell, "Comparative Anatomy and Physiology," p. 136).

In the second place, with regard to those other strictures which I can characterize by no other name than *mere whims*. It is a mere whim, for instance, to consider *Anodonta anatina* as a variety of *A. cygnea*, since such has never yet been generally recognized. It is a mere whim to believe that *Achatina acicula* should be *Cæciliancella acicula*; *Bulimus acutus* should be *Helix* (*Cochlicella*) *acuta*; *Zonites* should be *Hyalinia*," and I had rather remain with my old system of nomenclature than get so inextricably entangled in the medley of new systems made by Continental workers, all of which systems differ the one from the other as "chalk from cheese." It is a mere whim to imagine that chapters on "The Anatomy of a Snail" and "The Anatomy of a Fresh-water Mussel" should have been excluded since the basis of systematic zoology is anatomy. And it is a mere whim to cavil at the inclusion of the vars. *minor*, *maxima*, and "*albida*," (*exalbida*, if you please, Mr. Reviewer, for so was it named by Menke), and the monstrosity *sinistrorsum* of *Helix aspersa*, since Dr. Gwyn Jeffreys and Moquin-Tandon have named varieties and used variety-names, and since Prof. E. von Martens, than whom no better conchologist, expressly mentions that "it is certainly desirable that every local form, well-marked zoologically or geographically, should have a distinct name." And may I turn a reviewer on my own book, and ask myself how it is that I did not, as your reviewer desires, give the localities for every species, and make the book costly, and, by so doing, take it away from the reach of the poorer classes? Why also did I not give the definite localities for the now local species, when I considered rightly that some of them may turn up in other, and, perhaps, far distant spots to those now known? In conclusion, I would point out to your reviewer—for I must not occupy your valuable space to any greater extent—that as there is a virtue in the every-day affairs of this our mundane life which, to quote Seneca, is "the only immortal thing that belongs to mortality," so, as certainly, is there a virtue in right reviewing which is quite as exacting and quite as important to always bear in one's remembrance.

J. W. WILLIAMS.

51 Park Village East, N.W.

IN reviewing Dr. Williams's little book, I wished not merely to point out the author's mistakes, but to guard young conchologists, to whom the book is addressed, from placing too great reliance on the statements it contains.

I felt also convinced that the author was not practically conversant with his subject; indeed, that his knowledge was purely derivative, and this the foregoing letter fully confirms.

I will not occupy space with a detailed criticism on the author's method of compilation, but will simply refer to a single instance, quoted to show his want of care in referring to the original sources of information, so needful in such a task. The method of numeration of the tooth-formula, referred to by me as incorrect at p. 7 of the "Hand-book," is now justified by the author, who quotes Woodward as his authority; but upon referring to my brother's "Manual" I find that my statement was fully justified by the fact that the quotation is *not correct*, it having been taken by Dr. Williams from Prof. Jeffrey Bell's "Comparative Anatomy," where my brother's name is given as the authority for the instances quoted, and not for the whole paragraph to which it is appended, and which does not appear in his book. To the second part of Dr. Williams's *whimsical* letter I feel sure it is needless to reply.

HENRY WOODWARD.

129 Beaufort Street, S.W., May 28.

Freaks of Nature.

I INCLOSE a letter from my grandson Charles, a boy, son of St. Vincent Erskine, the explorer, with whose travels you are probably acquainted.

This singular instance of a change in the habits of birds, consequent on the advance of civilization, is extremely important and interesting, as it evinces almost reasoning powers and adaptation of habits to circumstances. As you are aware, some birds in South Africa build their nests on the pendant boughs of willow-trees as a defence against snakes and iguanas.

These willows, like other trees in Natal, are rapidly becoming scarcer, as they are cut down, whilst the boys who take the nests increase. This is, no doubt, the cause of the birds changing their nests to the telegraph-wires, where they are also safer from their natural enemies.

It would be interesting to know whether similar instances occur elsewhere.

D. ERSKINE.

47 Gratton Road, Kensington, May 25.

P.S.—It is remarkable also that the hole is at the side instead of the bottom, showing that the bird was aware that the situation was snake-proof. Darwin would have been glad of this proof of evolution.

WHILE watching the landscape of Natal between Ladysmith and Pietermaritzburg from a Natal Government Railway carriage, I saw some nests of the "golden weaver" bird. There were four of them hanging in a row, close together. They were the round kind, without the long arm. On one of the nests sat a cock weaver bird, but I saw no hens.

The nests seemed to be one or two years old, except one, which was greener than the others, and most certainly one of this season's. The chief peculiarity seemed to lie in the fact that the birds had woven grass round the wire for some six or eight inches, and two or three inches in circumference, before beginning to make the nest, and that the bird had to deal with a horizontal wire instead of a vertical stick or a branch. The bird always twists the grass round the branch (if he builds on a vertical twig) for some way up among the leaves and stalks, leaving the long ends free, thus forming his foundation. Weavers prefer to build on trees where the long slender twigs droop towards the ground, and so afford a nice vertical slender support. They are especially fond of the weeping-willow, whose slender switches generally branch off into two small shoots at the end: between these the bird loves to build his nest. Besides, the willow has lots of leaves very near together, and so holds the straws very well. On the wire he had no such support, but had to trust to his own ingenuity to overcome the novel situation, which task he seems to have accomplished very well.

The entrance to these nests was not at the bottom, as usual, but by a hole in the side, and all the nests did not look the same way.

I suppose there was only one nest a season or two ago, with a single pair of birds; soon we shall have a long string, or rather wire, of these ingeniously built homes with their happy quarrelsome occupants, making enough noise to stop all the messages ever sent that way. They will hear all the "Government" secrets: then we will be able to say truly, "A little bird told me."

C. H. ERSKINE.

WHIRLWINDS, WATERSPOUTS, STORMS, AND ROTATING SPHERES.[1]

IT is often necessary, in many branches of science, to halt in our steady progress along the beaten roads of induction, and say, "*Fiat experimentum.*" We may not always be able by this means to reproduce exactly all the physical conditions of the phenomenon, we are investigating, or to evolve a test crucial enough to enable us to decide between rival hypotheses. Nevertheless, the power we thus gain, especially in the case of an atmospheric phenomenon, of seeing the entire system of action in a *coup d'œil*, of gauging its relative proportions, and of examining its dependence and effects on its *entourage*, can hardly be over-rated.

Such would appear to have been M. Weyher's object in the delicate and ingenious experiments which he has so skilfully elaborated and described in the pamphlet of 91 pages before us.

The physical theory of atmospheric eddies, including the rotating flat disk or cyclone, and the rotating column which manifests itself as a tornado, waterspout, or dust-whirl, according to variations in its intensity and surrounding circumstances, has lately been developed to an extent not generally known, principally by Ferrel, Sprung, Oberbeck, and Marchi. It is therefore decidedly satisfactory to those who believe in the progress of meteorology by rational theory and deduction, to find that the motions exhibited in M. Weyher's experiments, in which the conditions in Nature are very fairly imitated, agree in every point with those which have been deduced from their physical theory.

Theory, for example, shows that a tornado is due primarily to an unstable condition of saturated air, accompanied by a gyrating motion (which may initially be very small, and which is practically always present to

[1] "Sur les Tourbillons, Trombes, Tempêtes, et Sphères Tournantes: Étude et Expériences." Par C. L. Weyher. (Paris, 1887.)

some extent, owing to the earth's rotation), relative to some central point.[1]

Given these conditions, the rest follow as necessary consequences, viz. (1) a current ascending up the *axis*, combined with rapid rotation round it; (2) a hyperboloidal funnel of rarefied air tapering downwards, and reaching the earth when the action is powerful, round the sides of which a condensed vapour-, or so-called water-spout, should usually prevail, owing to the sudden rarefaction of the air entering the central area through the sides or at the base, with the consequent lowering of the plane of condensation from the cloud-level which it usually occupies. When, therefore, it is said that "a waterspout is simply the cloud brought down to the earth by the rapid gyratory motion of the tornado,"[2] it is not meant that the cloud is actually carried downwards by an aërial current, since by theory the motion is precisely in the opposite direction; but that the *conditions* of *condensation* are propagated downwards from the cloud-stratum where they first commence. Neglect of this consideration, as well as the physical fact that condensation can only occur under most exceptional circumstances in a *downward* current, has led to many false deductions from apparent circumstances.

Theory, moreover, indicates that the current up the axis, together with gyration round it, which, by the conservation of rotational momentum, may become exceedingly rapid as the air approaches it, must combine to give a spiral character to the movement near the axis, while the conditions of continuity equally demand that there should be a compensatory descending current somewhere in the vicinity, gyrating spirally in the same sense, and of only moderate velocity, owing to its greater distance from the axis.

At the base of a tornado, or its milder form of waterspout, there should also be a rising up of the water at sea, or of light objects on land, which are supported by the ascending current until their collision or size carries them outside the central area, when they fall back to the earth, or to points where they are again brought within the influence of the whirl-currents. These and many other

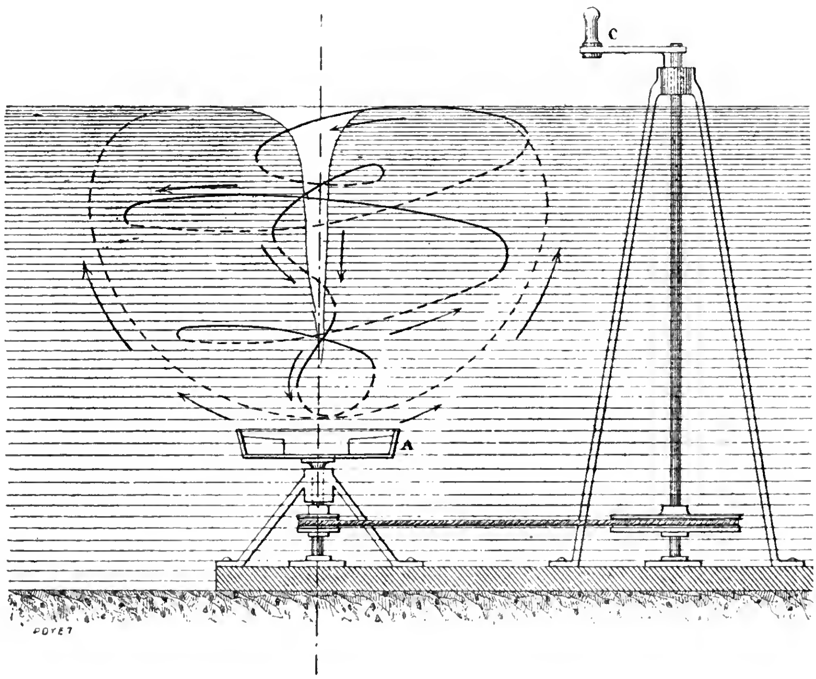


FIG. 1.

minor characteristics of tornadic action are confirmed and illustrated by M. Weyher's experiments.

M. Weyher commences by examining the conditions which prevail in an eddy produced in water, either by an outflow through a sluice, or a momentary rotation imparted by the stroke of an oar. In the former case the motion is well known, but in the latter it is somewhat new to find that besides the rotation round a vertical axis there is an interchanging vertical motion such that each particle describes a descending helix down the axis of the whirl, and ascends in a helix of the same sense to regain the surface.

Fig. 1 shows the same circulation produced by the revolution of a tourniquet, A.

If this figure be looked at upside down, it substantially depicts what is believed to be the motion of the air in a whirlwind, waterspout, or tornado, and is precisely similar to what is found to be the motion round those artificially produced by M. Weyher.

The important point to notice with respect to the water eddies, which are introduced mainly to show their analogy to air-whirls, is that, according to M. Weyher, their source of action must be at some distance *below* the surface. By artificially causing the liquid to rotate at its surface only, he found it impossible to obtain the central descending funnel of a complete water eddy.

A similar condition is found to hold in an *inverse* sense in the case of artificially-produced air-whirls. The action must in their case originate in the *upper* part of the air-column, whence the motion is communicated by degrees to its lower boundary. The analogy, therefore, between the water eddy with a descending motion round its axis and the atmospheric whirlwind is completely *inverse*, and not *direct*, as some have supposed.

[1] Sergeant Finley found, in his review of 600 tornadoes in the United States, that the direction of rotation round the axis was invariably cyclonic, or against watch-hands ("Signal Service Notes," No. xii. p. 10).

[2] "Recent Advances in Meteorology" (p. 301), by W. Ferrel. (Washington, 1885.

M. Weyher next proceeds to discuss the motions which should theoretically occur in an air-whirl. These are shown in vertical section in Fig. 2.

In the annular region bordering the inner rarefied space, and represented by A *a c* C, *b* B D *d*, the air is assumed to be rendered denser than the normal by the centrifugal force of gyration, and according to M. Weyher it is by the *descent* of this denser air upon the depression caused by the air below rushing up to fill the central area, that the rotation system propagates itself from above towards the earth.

We do not think this explanation is either correct or necessary. It is contrary to the physical theory that there should be a sheath of dense air surrounding the rarefied region, and, apart from this, friction, and the transference of air up the axis from its lower end amply account for the downward propagation.

The most interesting of M. Weyher's experiments are those in which he artificially produces the phenomena of the waterspout. By means of a rotating tourniquet placed over cold water, an aërial eddy is caused which draws up the water, in the form of a spout composed of drops, to a considerable height; but when the water is heated, a clearly-defined condensed-vapour-, or, as it is popularly

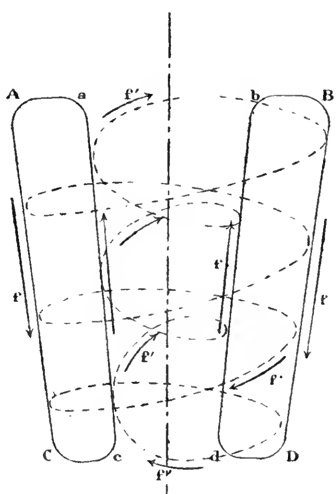


FIG. 2.

termed, water-spout, makes its appearance, like that shown in Fig. 3, which represents a form of the apparatus suitable for a chamber experiment.[1]

With from 1500 to 2000 rotations per minute, the vapour from the heated water is found to condense itself into a visible sheath enveloping a clearly-defined and rarefied central nucleus, conical, and tapering downwards. The diameter of the sheath is from $\frac{3}{4}$ inch to 1 inch. Besides this vapour-spout, water-drops are carried up, as in natural marine spouts, until they are thrown out beyond the influence of the upward current.

Other features of spouts are then imitated, particularly what is called the *hérisson*, which appears to be identical with what the French sailors call the *buisson*, or bush-like ploughing up of the sea, which occurs at their bases, both before and during the period of complete formation. This is effected by placing twenty or thirty small air-balloons in the place of the water, underneath the tourniquet. These are then seen to rise up a short distance, and fall back in graceful interlacing elliptical curves. The entire motion throughout the *hérisson*, as well as the whole system, is further studied by placing underneath the tourniquet a quantity of oatmeal in a glass vessel, and observing its motion by means of eye-pieces fitted into the top of the vessel. The motions are thus seen to be precisely the same as those theoretically inferred, and when the rotation is stopped, the ascending spires of the currents at the lower end, are found engraven in lines on the finer particles, which, in obedience to these currents, lie in a conical heap round the vertical axis of the whirl.

Several other experiments are made with cotton-wool and smoke, each of which exhibits some special feature characterizing the spouts of Nature.

The pressure and temperature conditions in different parts of the area are next investigated.

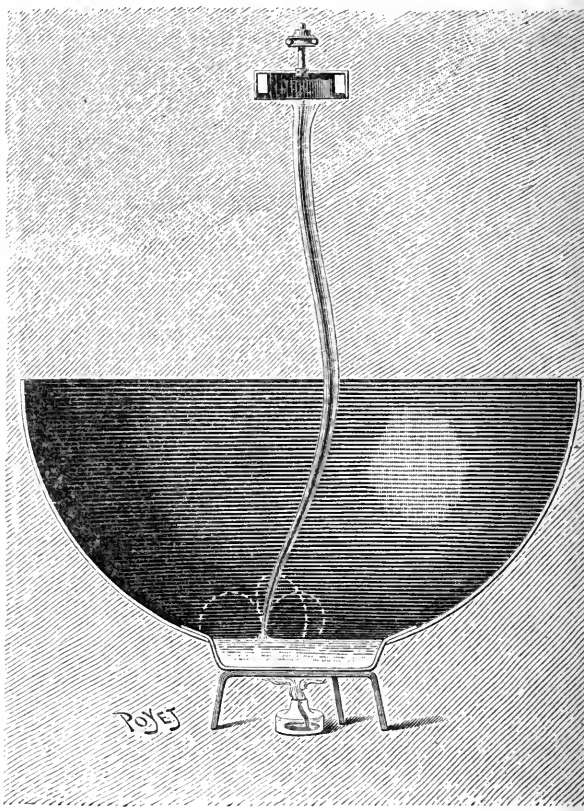


FIG. 3.

By means of a manometer, it is found that the rarefaction at the centre of the rotating tourniquet is transmitted almost unaltered in intensity (probably proportionally diminished in area) to the centre of the whirl on the surface, while the thermometer at the same point, at first shows a fall and then a rise of temperature, the latter evidently due to the friction of the rapidly moving air against the surface.

The analogous phenomenon of a cyclone is very fairly imitated by the apparatus shown in the accompanying diagram (Fig. 4), consisting of a large tourniquet placed over a table covered with a number of pins mounted with movable threads of red wool. The tourniquet is arranged so as to be capable of translation as well as rotation. At the centre, the table is pierced with a small hole at D, communicating by means of a caoutchouc tube with a manometer, which thus registers the changes of pressure

[1] For those desiring to repeat the experiment the dimensions are as follows. The tourniquet is made of tin from 5 to 6 inches in diameter by 1 to $1\frac{1}{2}$ inch in height. There are from 10 to 12 rectangular fans 1 inch by $\frac{3}{4}$ inch. The vessel holding the water is placed 31 to 39 inches from the tourniquet, and is from 7 inches to 1 foot in diameter by $1\frac{1}{2}$ to 2 inches deep. The hemispherical continuation of the vessel, to keep off local air-currents, which disturb the continuity of the spout, is 3 feet in diameter.

as the supposed cyclone passes over it. On rotating the tourniquet and passing it along over the table, the directions and positions of the threads are seen to indicate not only the horizontal, but also the vertical components of the winds thus produced, including the region of calm in the centre, as well as the downward and outward motion at the anticyclonic border. The variations of pressure recorded by the manometer, when plotted out, show a curve similar to that in a symmetrical cyclone, including the rise of pressure at the border where the motion is descending and outwards.

Hail is then explained, as being caused by vapour drawn up into the *hérisson* of what M. Faye terms a *trombe internubaire*, which descends from the upper regions as far as the surface of the cloud, whence the hail proceeds. The rest of the explanation, which mainly involves a continual churning up and down of the frozen particles, is similar to that given by Ferrel and Möller, except that the hailstones impinging upon one another at the focus of the *hérisson* are supposed, by the heat thus engendered, to aid in effecting the temporary melting of their surfaces necessary to account for the concentric coats of snow and ice they usually exhibit.

M. Weyher's experiments do not, of course, fulfil all the conditions which prevail in Nature, since in that case the rotation is doubtless kept up, after it has once been started in the air at some distance above the surface, by the upward movement along the axis, and the consequent aspiration of the surrounding air into the area of gyration. With this exception, however, there seems little wanting.

The position of the source from which the vapour is drawn is not so important as might be thought, since the vapour condensed in the natural waterspout is not the cloud actually brought down to the surface, any more than it is—except for the space of a few feet at its lower extremity—the water bodily carried up, but is the result of the condensation, by rarefaction, of vapour previously contained invisibly, but certainly amply enough for the purpose, right down to the earth's surface. In fact, the origin of the vapour, being at the base, more nearly imitates Nature than if it were only supplied above in the form of a cloud.

M. Weyher's experiments so far, therefore, bear out the hypothesis that a system of rotating air-currents above the earth's surface, causes tornadic, waterspout, and dust-spout phenomena, by an aspiration *towards*, and a flow *up*, its axis, and show that such a system can propagate itself and its accompanying effects downwards without assuming any downward component along the axis.

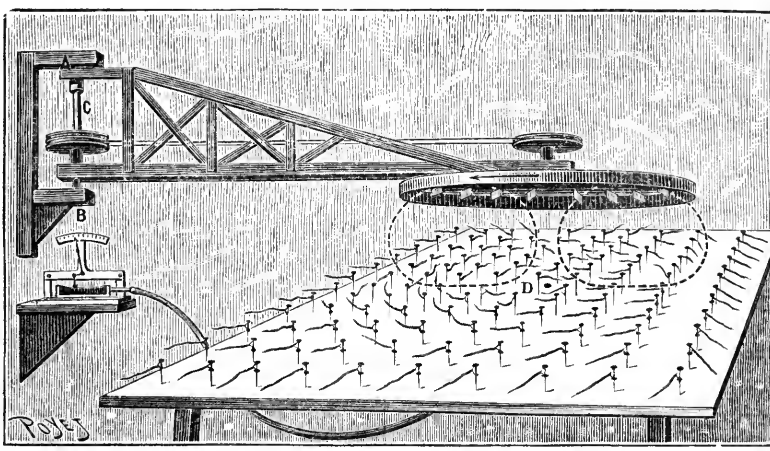


FIG. 4.

The last part of the work is devoted to a description of certain curious effects produced by rotating spherical tourniquets. Fig. 5 shows a convenient form of the apparatus, in which S represents a sphere made of eight or ten circular fans, fixed on an axis passing through two vertical disks whose function it is to keep off disturbing currents, and also to concentrate the action. M is an air-balloon, which, when the tourniquet is set in motion, is found to revolve round it in the plane of its equator, and be attracted instead of repelled.

M. Weyher thus explains this, at first sight, paradoxical motion. A rotating spherical ventilator draws in the air chiefly at its poles, and expels it in the plane of the equator, but, except *in this plane*, there is a general motion of the air all round *towards* the ventilator. The stream of air issuing from the ventilator in the plane of the equator is divided by the balloon, and forms vortices, which, together with the currents centrally directed on its reverse side, tend to urge it towards the ventilator. Whether this explanation be considered satisfactory or not, the balloon certainly revolves like a satellite round the ventilator. By means of floating gold-leaves, the action of the ventilator is seen to cause two dissymmetrical aërial whirls, whose inner gyrations commencing at some distance from the generating sphere, run round the polar axis in opposite directions, and meet on the plane of the equator. From thence the air jointly brought by these inner helices is driven outwards, and returns by similar helices, like the downward return-current of the tornado, to the points at the extremities of the prolonged polar axis. So far well, but we cannot quite admit the validity of the manometer experiment by which, on p. 74, the author attempts to show the existence of the aspiration in the plane of the equator requisite to explain the attraction it exerts on the air-balloon. The effect of velocity in decreasing pressure, as exemplified by Hawksbee's famous experiment, would probably mask any other vortical effects such as those sought by M. Weyher.

It appears to be a recognized custom for an author, after describing his experiments, to indulge in some pet speculations, and even to make the orthodoxy of the former an excuse for the frequently Utopian character of the latter.

M. Weyher certainly treats himself to an ample dessert of this description in his concluding section, in which, assuming the existence of a *ponderable* ether, the phenomena of the *tourbillon* are by analogy transferred to the solar system, which is supposed to be the *hérisson* of a whirl system reaching it from space, the sun being in the focus, and in which the planets, by the mutual influence of the ethereal whirls due to their axial rotation, cause simultaneously spots on the sun and cyclones on the earth.

We fail to follow M. Weyher here, and think it would have been better if he had not only hesitated, as he admits he did, but decided not to publish such wild speculations. His experiments are exceedingly instructive

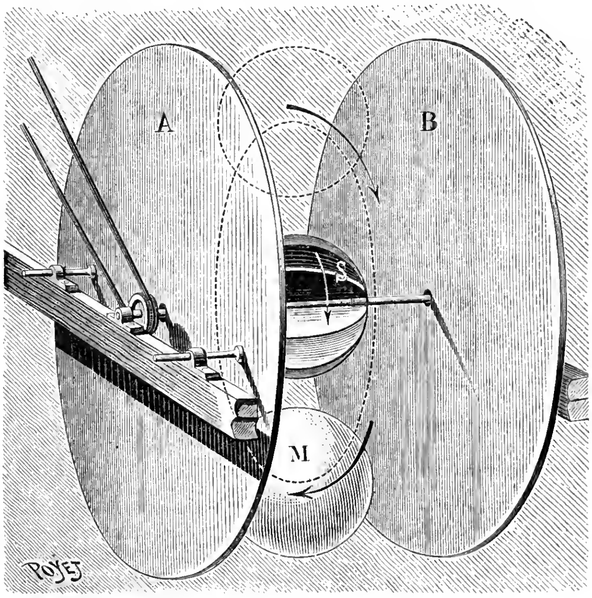


Fig. 5.

and suggestive, and if he can ultimately succeed in imitating the conditions of Nature more closely, we shall doubtless have an end of the theoretical polemics which have hitherto retarded rather than aided the progress of our knowledge of aërial motions and their causes.

E. Douglas Archibald.

TIMBER, AND SOME OF ITS DISEASES.[1]

VII.

If we pass through a forest of oaks, beeches, pines, and other trees, it requires but a glance to see that various natural processes are at work to reduce the number of branches as the trees become older. Every tree bears more buds than develop into twigs and branches, for not only do some of the buds at a very early date divert the food-supplies from others, and thus starve them off, but they are also exposed to the attacks of insects, squirrels, &c., and to dangers arising from inclement weather, and from being struck by falling trees and branches, &c., and many are thus destroyed. Such causes alone will account in part for the irregularity of a tree, especially of a Conifer, in which the buds may be developed so regularly that if all came to maturity the tree would be symmetrical. But that this is not the whole of the case, can be easily seen, and is of course well known to every gardener and forester.

If we remove a small branch of several years' growth from an oak, for instance, it will be noticed that on the twigs last formed there is a bud at the axil of every leaf; but on examining the parts developed two or three years previously it is easy to convince ourselves of the existence of certain small scars, above the nearly obliterated leaf-scars, and to see that if a small twig projected from each of these scars the symmetry of the branching might be completed. Now it is certain that buds or twigs were formed at these places, and we know from careful observations that they have been naturally thrown off by a process analogous to the shedding of the leaves; in other words, the oak sheds some of its young branches naturally every year. And many other trees do the same; for instance, the black poplar, the Scotch pine, *Dammara*, &c.; in some trees, indeed, and notably in the so-called swamp cypress (*Taxodium distichum*) of North America, the habit is so pronounced that it sheds most of its young branches every year.

But apart from these less obvious causes for the suppression of branches, we notice in the forest that the majority of the trees have lost their lower branches at a much later date, and that in many cases the remains of the proximal parts of the dead branches are sticking out from the trunk like unsightly wooden horns. Some of these branches may have been broken off by the fall of neighbouring trees or large limbs; others may have been broken by the weight of snow accumulating during the winter; others, again, may have been broken by hand, or by heavy wind; and yet others have died off, in the first place because the over-bearing shade of the surrounding trees cut off the access of light to their leaves, and secondly because the flow of nutritive materials to them ceased, being diverted into more profitable channels by the flourishing, growing parts of the crown of leaves exposed to sunlight and air above.

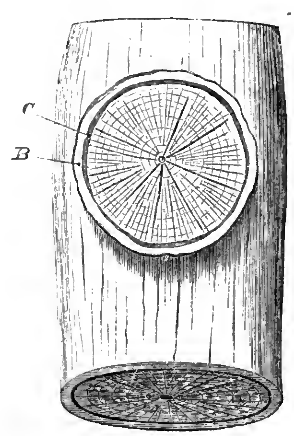


Fig. 21.—Portion of a tree from which a branch has been cut off close to the stem. *C*, the cambium of the branch; *B*, the cortex.

The point I wish to insist upon here is that in these cases of branch-breaking, however brought about, open wounds are left exposed to all the vicissitudes of the forest atmosphere; if we compare the remnant of such a broken branch and the scar left after the natural shedding of a branch or leaf, the latter will be found covered with an impervious layer of cork, a tissue which keeps out damp, fungus-spores, &c., effectually.

It is, in fact—as a matter of observation and experiment—these open wounds which expose the standing timber to so many dangers from the attacks of parasitic fungi; and it will be instructive to look a little more closely into the matter as bearing on the question of the removal of large branches from trees.

If a fairly large branch of a tree, such as the oak, is cut off close to the trunk, a surface of wood is exposed, surrounded by a thin ring of cambium and bark (as in Figs. 21 and 22). We have already seen what the functions of the cambium are, and it will be observed that the cut edge of the cambium (*C*) is suddenly placed under different conditions from the usual ones; the chief change, and the only one we need notice at present, is that the cambium in the neighbourhood of the cut surface is released

[1] Continued from vol. xxxvii. p. 516.

from the compressing influence of the cortex and bark, and owing to this release of pressure it begins to grow out at the edges into a cushion or "callus," as shown in Figs. 23 and 24. A very similar "callus" is formed in the operation of multiplying plants by "cuttings," so well

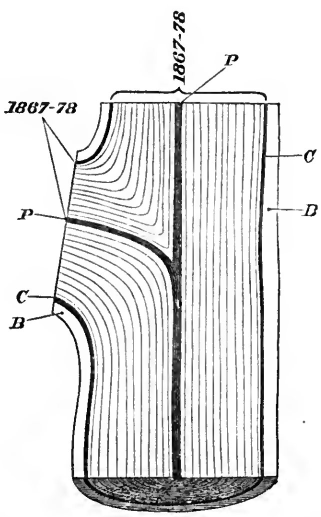


FIG. 22.—The same in longitudinal section. *P*, the pith of stem and branch; on either side of this are the twelve annual zones of wood produced during the years 1867–78, as marked. The cambium, *C*, separates these from the cortex, *B*.

known to all: the cambium at the cut surface of the "slip" or "cutting," is released from the pressure of the cortex, and begins to grow out more rapidly in the directions of less pressure, and forms the callus.

Now this callus (Fig. 23, *Cal*) is in all cases something more

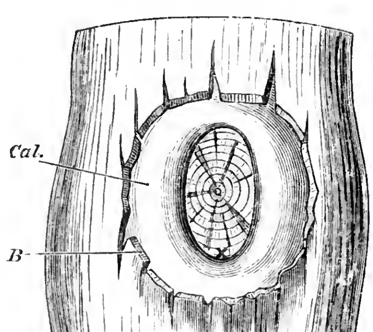


FIG. 23.—The same piece of stem four years later. The cushion-like development, *Cal*, resulting from the overgrowth of the cambium and cortical tissues of the cut branch, has extended some distance from the edges, and is covering in the exposed wood. *B* is the dead outer corky tissue, incapable of growth, and partially cracked under the pressures exerted by the thickening of the stem. The latter is somewhat swollen transversely, owing to the release of pressure in this region enabling the cambium to develop a little more actively here; the quicker growth of the occluding cushion in the horizontal direction is due to the same cause.

than mere cambium—or rather, as the cambium extends by cell-divisions from the cut edge of the wound, its outer parts develop into cortex, and its inner parts into wood, as in the normal case. The consequence is that we have in the callus, slowly creeping out from the margins of the wound, new layers of wood and cortex with cambium between them (Fig. 24); and it will be noticed that each year the layer of wood extends a little further over the surface of the wound, and towards the centre of the cut branch; and in course of time, provided the wound is not too large, and the tree is full of vigour, the margins of the callus will meet near the middle, and what was the exposed cut surface of the branch will be buried beneath layers of wood and cortex, between which lies the cambium, now once more continuous over the whole trunk of the tree (Figs. 25 and 26).

It is not here to the purpose to enter into the very interesting histological questions connected with this callus-formation, or with the mechanical relations of the various parts one to another. It is sufficient for our present object to point out that this process of covering up, or *occlusion*, as I propose to term it, requires some time for its completion. For the sake of illustration, I have numbered the various phases in the diagram, with the years during which the annual rings have been formed; and it

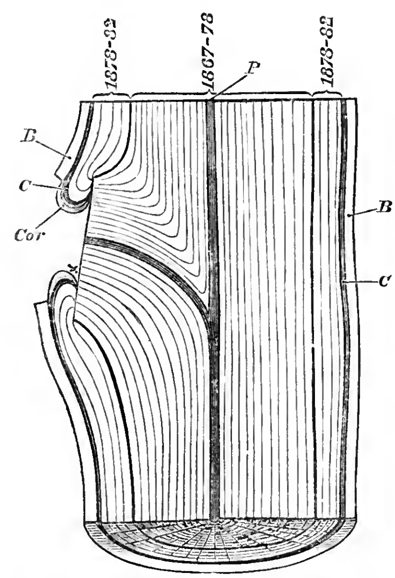


FIG. 24.—The same in longitudinal section: *P*, *B*, and *C* as before. The four new layers of wood formed during 1879–82 are artificially separated from the preceding by a stronger line. On the left side of the figure it will be noticed that the cambium (and therefore the wood developed from it) projected a little further over the cut end of the branch each year, carrying the cortical layers (*Cor*) with it. At ×, in both figures, there is necessarily a depression in which rain-water, &c., is apt to lodge, and this is a particularly dangerous place, since fungus-spores may here settle and develop.

will be seen at a glance that, in the case selected, it required seven years to cover up the surface of the cut branch (cf. Figs. 21–26). During these seven years more or less of the cut surface was exposed (Fig. 24) to all the exigencies of the forest, and it will easily be understood that abundant opportunities were thus afforded for the spores of fungi to fall on the naked wood, and for moisture to condense and penetrate into the interior; moreover, in the ledge formed at × in Figs. 23 and 24, by the lower part of the callus, as it slowly creeps up, there will always be water in wet weather; and a sodden condition of the wood at this part is insured. All this is, of course, peculiarly adapted for the germination of spores; and, since the water will soak out nutritive materials, nothing could be more favourable for the growth and development of the mycelium of a fungus. These circumstances, favourable as they are for the fungi, are usually rendered even more so in practice, because the sawyers often allow such a branch to fall, and tear and crush the cambium and cortex at the lower edge of the wound. These and other details must be passed over, however, and our attention be confined to the fact that here are ample chances for the spores of parasitic and other fungi to fall on a surface admirably suited for their development.

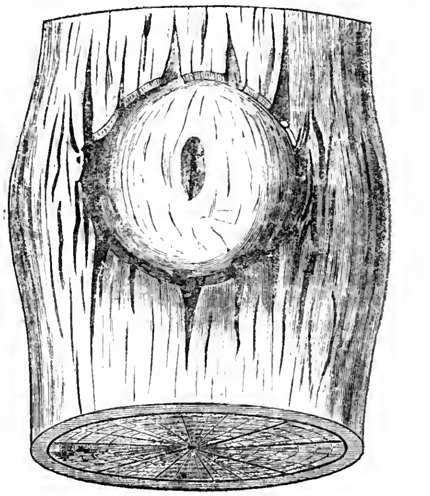


FIG. 25.—The same piece of stem six years later still: the surface of the cut branch has now been covered in for some time, and only a boss-like projection marks where the previous cut surface was. This projection is protected by cork layers, like ordinary outer cortex, the old outer cortex cracking more and more as the stem expands.

The further fact must be insisted upon that numerous fungus-spores do fall and develop upon these wounds, and that by the time the exposed surface is covered in (as in Fig. 25) the timber is frequently already rotten, usually for

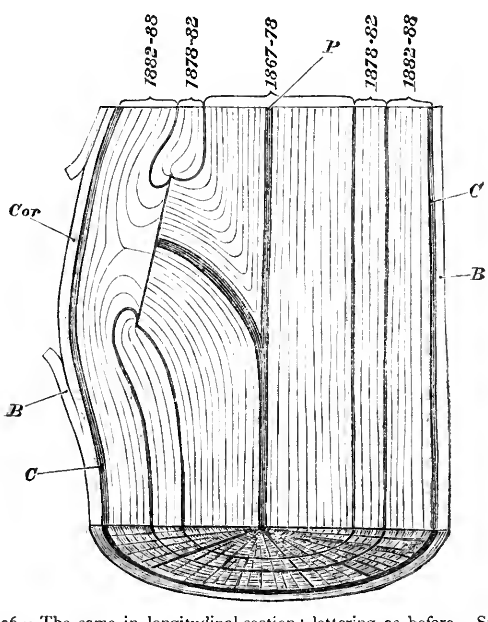


FIG. 26.—The same in longitudinal section: lettering as before. Six new layers of wood have been developed, and the cut end of the branch was completely occluded before the last three were formed—*i.e.* at the end of 1885. After that the cambium became once more continuous round the whole stem, and, beyond a slight protuberance over the occluded wound and the ragged edges of the dead corky outer layers, *B*, there are no signs of a breach.

some distance down. In the event of fungi, such as have been described above—parasites and wound-parasites—gaining a hold on such wounds, the ravages of the mycelium will continue after the occlusion is complete, and I have seen scores of trees, apparently sound and whole, the interior of which is a mere mass of rottenness: when a heavy gale at length blows them down, such trees are found to be mere hollow shells, the ravages of the mycelium having extended from the point of entry into every part of the older timber.

In a state of nature the processes above referred to do not go on so smoothly and easily as just described, and it will be profitable to glance at such a case as the following.

A fairly strong branch dies off, from any cause whatever—*e.g.* from being overshadowed by other trees. All its tissues dry up, and its cortex, &c., are rapidly destroyed by saprophytic fungi, and in a short time we find only a hard, dry, branched stick projecting from the tree. At the extreme base, where it joins the tree, the tissues do not at once perish, but for a length of from half an inch to an inch or so the base is still nourished by the trunk. After a time, the wind, or a falling branch, or the weight of accumulated snow, &c., breaks off the dead branch, leaving the projecting basal portion: if the branch broke off quite close to the stem, the wound

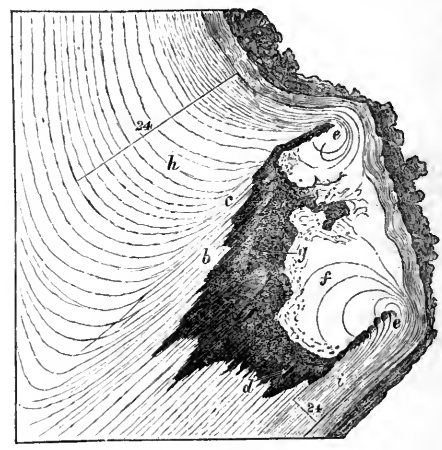


FIG. 27.—Base of a strong branch which had perished naturally twenty-four years previously to the stage figured. The branch decayed, and the base was gradually occluded by the thickening layers of the stem: the fall of the rotting branch did not occur till six years ago, however, as can be determined from the layers at *e* and *f*, which then began to turn inwards over the stump. Meanwhile, the base had become hollow and full of rotten wood, *g*. It is interesting to note how slight the growth is on the lower side of the branch base, *i*, as compared with that at *h* above: the line numbered 24 refers to the annual zones in each case. As seen at *b* and *d*, the rotting of the wood passes backwards, and may invade the previously healthy wood for some distance. (After Hartig.)

would, or at least might, soon be occluded; but, as it is, the projecting piece not only takes longer to close in, but it tends to rot very badly (Fig. 27), and at the best forms a bad "knot" or hole in the timber when sawn up. Of course what has already been stated of cut branches applies here: the wounds are always sources of danger so long as they are exposed.

It is beyond the scope of these articles to set forth the *pros* and *cons* as to the advisability of adopting any proposed treatment on a large scale: the simple question of cost will always have to be decided by those concerned. But whether it is practicable or not on a large scale, there is no question as to the desirability of adopting some such treatment as the following to preserve valuable trees and timber from the ravages of these wound-parasites. Branches which break off should be cut close down to the stem, if possible in winter, and the clean cut made so that no tearing or crushing of the cambium and cortex occur; the surface should then be painted with a thorough coating of tar, and the wound left to be occluded. If the cutting is accomplished in spring or summer, trouble will be caused by the tar not sticking to the damp surface. Although this is not an absolute safeguard against the attacks of fungi—simply because the germinal tubes from spores can find their way through small cracks at the margin of the wound, &c.—still it reduces the danger to a minimum, and it is certain that valuable old trees have been preserved in this way.

Before passing to treat of the chief diseases known to start from such wounds as the above, it should be remarked that it is not inevitable that the exposed surface becomes attacked by fungi capable of entering the timber. It happens not unfrequently that a good closure is effected over the cut base of a small branch in a few years, and that the timber of the base is sound everywhere but at the surface: this happy result may sometimes be attained in pines and other Conifers, for instance, by the exudation of resin or its infiltration into the wood; but in rarer cases it occurs even in non-resinous trees, and recent investigations go to show that the wood formed in these healing processes possesses the properties of true heart-wood. At the same time there is always danger, as stated, and we will now proceed to give a brief account of the chief classes of diseases to which such wounds render the tree liable.

The first and most common action is the decay which sets in on the exposure of the wood surface to the alternate wetting and drying in contact with the atmosphere: it is known that wood oxidizes under such circumstances, and we may be sure that wounds are no exception to this rule. The surface of the wood gradually turns brown, and the structure of the timber is destroyed as the process extends.

The difficulty always arises in Nature, however, that mould-fungi and bacteria of various kinds soon co-operate in and hurry these processes, and it is impossible to say how much of the decay is due to merely physical and chemical actions, and how much to the fermentative action of these organisms. We ought not to shut our eyes to this rich field for investigation, although for the present purpose it suffices to recognize that the combined action of the wet, the oxygen of the air, and the fermenting action of the moulds and bacteria, &c., soon converts the outer parts of the wood into a mixture of acid substances resembling the humus of black leaf-mould.

Now as the rain soaks into this, it dissolves and carries down into the wood below certain bodies which are poisonous in their action on the living parts of the timber, and a great deal of damage may be caused by this means alone. But this is not all: as soon as the decaying surface of the wound provides these mixtures of decomposed organic matter, it becomes a suitable soil for the development of fungi which are not parasitic—*i.e.* which cannot live on and in the normal and living parts of the tree—but which can and do thrive on partially decomposed wood. The spores of such fungi are particularly abundant, and most of the holes found in trees are due to their action. They follow up the poisonous action of the juices referred to above, living on the dead tissues; and it will be intelligible that the drainage from their action aids the poisonous action as it soaks into the trunk. It is quite a common event to see a short stump, projecting from the trunk of a beech, for instance, the edges of the stump neatly rounded over by the action of a callus which was unable to close up in the middle, and to find that the hollow extends from the stump into the heart of the trunk for several feet or even yards. The hollow is lined by the decayed humus-like remains of the timber, caused by the action of such saprophytes as I have referred to. Similar phenomena occur in wounded or broken roots, and need not be described at length after what has been stated.

But, in addition to such decay as this, it is found that if the spores of true wound-parasites alight on the damp surface of the cut or broken branch, their mycelium can extend comparatively rapidly into the still healthy and living tissues, bringing about the destructive influences described in Articles III. and IV., and then it matters not whether the wound closes over quickly or slowly—the tree is doomed.

H. MARSHALL WARD.

(*To be continued.*)

HERVÉ MANGON.

IN the current number of *La Nature* there is an interesting article, by M. Gaston Tissandier, on Charles François Hervé Mangon, whose death we announced last week. The following are the essential facts noted by M. Tissandier.

Hervé Mangon was born in Paris on July 31, 1821, and was trained by his father, a military surgeon, who devoted himself almost entirely to the education of his son. At the age of nineteen the young man entered l'École Polytechnique, and two years later l'École des Ponts et Chaussées. He afterwards acted as engineer for several railways, but his chief interest at that time was in science as applied to agriculture.

In 1850 he published his "Études sur les Irrigations de la Campine Belge," and on the "Travaux Analogues de la Sologne." This work attracted great attention, and brought about important improvements in the French laws relating to agriculture. Drainage was then scarcely known, even by name, in France. In 1851, M. Hervé Mangon published a work on the subject, which was considered so valuable that he received from the Academy of Sciences the decennial prize for the most useful work on agriculture issued during the previous ten years. His practical instructions on drainage, of a little later date, were widely circulated, and it is estimated that the results of his researches have led to an increase, in the French revenue, of fourteen millions of francs yearly. Irrigation, manures, chemical refuse, and everything by which land might be fertilized, were made by him subjects of prolonged and careful study. He visited the principal agricultural works and irrigations in France, Belgium, Scotland, Spain, and Algiers, and summed the knowledge thus acquired in his "Traité de Génie Rural."

These researches were followed by meteorological studies, in which he took the deepest interest. He invented or improved many meteorological instruments, and on his estate at Brécourt in Normandy he organized a model meteorological station, provided with the latest scientific improvements. Towards the end of his career he played a most important part in the reorganization of the French meteorological service, and he became the President of the Meteorological Council. He contributed also to the organization of the scientific mission to Cape Horn, and to many other enterprises useful to science.

As a Professor, he created at the École des Ponts et Chaussées the course on "Hydraulique Agricole" (1849); at the Conservatoire des Arts et Métiers the course on "Travaux Agricoles et de Génie Rural" (1864); and at the new Institut National the course on "Génie Rural" (1876), a science of which he may be considered one of the founders. He lectured with ease, and his expositions were always clear and methodical.

He possessed an extraordinary power of work. He rose early, carried on his own correspondence, and did all his literary work without assistance. His personal tastes were simple, and the activity of his body seemed to keep pace with that of his mind. He welcomed fellow-workers cordially, and readily offered them counsel and help, his disposition being one of rare generosity. He was skilful in working in wood and metal, and always kept in his library a quantity of apparatus made by himself. With this he was constantly experimenting, sometimes even getting up during the night to carry on some research of special interest.

In 1872 he was elected a member of the Academy of Sciences; in 1880, Director of the Conservatoire des Arts et Métiers; and in 1887, Vice-President of the Academy of Sciences. Notwithstanding the manifold calls on his time, he worked hard to secure the success of the Exhibition of 1867, and of all the succeeding Paris Exhibitions.

Believing it to be important that men of science should take part in politics, he entered the Chamber as Deputy for La Manche, and became Minister of Agriculture in the Brisson Ministry, in which he was of eminent service.

During the war of 1870 he gave proof of ardent patriotism. Night and day, during the siege of Paris, he made incessant observations in order to facilitate the despatch of letters by balloon. For six months he did not miss the departure of one of the balloons; he was always present, encouraging the aëronauts, and giving them valuable directions. When M. Tissandier was about to leave Paris in a balloon, laden with messages for the Government at Tours, M. Hervé Mangon said to him, "Vous avez bon vent est-nord-est; vous allez filer dans la direction de Dreux," and the balloon descended at the gates of that very town.

M. Hervé Mangon was the son-in-law of J. B. Dumas. He had a wide circle of friends, and many young men of science owe him a deep debt of gratitude for the encouragement they received from him in their work. For a long time he suffered from a painful malady, and on the 15th of May he died at Paris, in his sixty-seventh year.

NOTES.

THE annual Ladies' *Conversazione* of the Royal Society will be held on Wednesday, June 6.

MR. R. G. HALIBURTON writes from Oran, Algeria, that a few hours after he had read the account in the *Times* of the recent *soirée* of the Royal Society, at which two skeletons of Akkas, sent by Emin Pasha from Equatorial Africa, were exhibited, the discovery, made by himself in February last, of the existence of another dwarf race, in North Africa, also only 4 feet high, and called by the same name, Akkahs, was confirmed by the receipt of a letter on the subject from our late Minister at Morocco, Sir John Drummond Hay.

THE creation of the new Chair of Philosophie Biologique is to be proposed to the Sorbonne in the course of the next few days. There will be much opposition to the scheme, but not enough to prevent it from being carried out.

THE ceremony in honour of Prof. Donders, at Utrecht, on Monday, passed off most successfully. Many friends and admirers, not only from all parts of Holland, but from the Dutch colonies and other countries, assembled to show their respect for the illustrious investigator, and the Dutch Government was represented on the occasion by the Home Minister. A medal commemorative of the ceremony was struck, and the King of Holland conferred on Prof. Donders the distinction of Commander of the Golden Lion. King Humbert sent him the Order of the Crown of Italy, and Sir Joseph Lister congratulated him on behalf of the Royal Society of England. In responding to the address recognizing his services to science and humanity, Prof. Donders declared that although the law rendered it necessary for him, on the attainment of his seventieth birthday, to resign his professorship, he did not consider that he had finished his task. The sum subscribed as an expression of gratitude for Prof. Donders' work is to be appropriated, in accordance with his own decision, for the benefit of young physiologists and ophthalmologists at the University.

DURING the recent cruise of the Liverpool Marine Biology Committee in the s.s. *Hyæna*, the electric light was applied to deep and surface tow-netting after dark with important results. We hope shortly to publish fuller details.

A MARINE zoological station, on the plan of the one at Naples, is shortly to be established at Ostend. The proposal is supported by four Belgian Universities.

A LETTER has been received by Sir J. D. Hooker from Mr. Joseph Thomson, dated Mogador, May 6, stating that he is on the eve of starting by a route through the province of Shedma to Saffi, where, after a short stay with M. Hunot, H.B.M.'s Consul there, he will go direct to Demenat, an entirely unexplored part of the Atlas, north-east of the city of Morocco. Mr. Thomson describes the past season as having been exceptionally late and cold, and with an extraordinary rain and snowfall; the season's rainfall at Mogador having been more than 32 inches, against an average of less than 18 inches.

IT is stated that Mr. Knipping, of the Meteorological Department of Japan, is coming to Europe on a mission to report on European meteorological observatories.

IN the *American Meteorological Journal* for April, Mr. A. L. Rotch continues his article on the history of the meteorological organizations, dealing with the German Institute, and the various newspaper services. Prof. F. Waldo contributes a very interesting paper on the instruments for making observations of the amount and direction of the wind. Special attention is given to Dr. Robinson's anemometer, as the instrument almost universally adopted, and so called from his investigation of its principle, published in 1850. Its invention is attributed to Edgeworth, who first used it as a scientific instrument, but a similar apparatus, made of wood, with oval cups, is described in the *Mongolische Völker*, 1770. Dr. Robinson found that the velocity of the cups must be multiplied by the factor 3 in order to get the true wind velocity, and this value was generally adopted. Mr. Stow and Prof. Stokes in this country, and Dr. Dohrandt in Russia, first questioned the accuracy of this value, and recent careful experiments by Mr. Dines, just communicated to the Royal Meteorological Society, show that the factor for anemometers of this class must be reduced to about 2·15. And further, it has been found that the formula for conversion of velocity to pressure ($P = \cdot005V^2$) adopted by Smeaton (Phil. Trans. 1763), and repeated subsequently in text-books, requires amendment, so that the pressures deduced from velocity anemometers have been greatly exaggerated. In fact great doubt has been expressed by competent authority as to the value of the records of this class of instruments. Prof. Waldo's discussion of the subject is therefore very opportune.

AT Cragside, Rothbury, Northumberland, the seat of Lord Armstrong, a very fine female of Pallas's sandgrouse (*Syrrhaptes paradoxus*) killed itself against the telegraph-wires near Cragside on Wednesday, May 23. The bird was picked up by the gamekeeper, and was sent by Lord Armstrong to Mr. John Hancock at the Natural History Museum, Newcastle-on-Tyne, where it will be carefully preserved. This bird was in fine plumage, and was proved by dissection to be a female, the ovary containing seven ova about the size of No. 1 shot, and numerous others of very much smaller size. It is a curious coincidence that the first specimens of Pallas's sand grouse, recorded in 1863, were shot at Thropton, a few miles west of Rothbury, on May 21, and were sent to Mr. Hancock. The crop of another specimen (male) of this bird, which we are told was obtained at Winlaton, five or six miles west of Newcastle-on-Tyne, was sent to the Museum on the 23rd inst. The crop was full of the seed of a wild plant, probably charlock or wild mustard (*Sinapis arvensis*, L.).

DR. TRIMEN'S report on the five Royal Botanic Gardens of Ceylon, which has just been issued, contains much interesting matter relating to the economic aid given by the institution to planting in Ceylon and elsewhere. Referring to the gradual decline in the cultivation of coffee, Dr. Trimen mentions, as one of the causes, that it has suffered severely during the last few years from the attacks of a scale-insect or "bug" which has in some places actually killed out the bushes. Practical planters think the insect different from either of the "bugs" familiar hitherto as foes to coffee—*Lecanium coffeæ* and *L. nigrum*, the brown coffee and black bugs. The distinctions between the three have been pointed out by Mr. E. Green in a paper with illustrations printed by the Government of Ceylon. He names the new pest *L. viride*, it being generally known as the green bug. Dr. Trimen mentions that his principal employment during the past year has been the compilation, with the aid of the library and herbarium, of a catalogue of the contents of the gardens, for use by the staff, the public, and correspondents in other countries. The list as now completed is brought down to the end of 1886, and contains about 3000 species, mostly trees and shrubs. He also reports the commencement of the long projected museum of economic botany.

A VOLUME on the life and works of Lavoisier, by Prof. E. Grimaux, of the Polytechnic School of Paris, has just been published. It is illustrated by many interesting engravings, two of which represent Lavoisier in his laboratory. A number of hitherto unknown documents relating to Lavoisier have been discovered by Prof. Grimaux.

MESSRS. MACMILLAN AND BOWES, Cambridge, will have ready in a week a "Bibliography of the Works of Sir Isaac Newton, together with a List of Books illustrating his Life and Works," by G. J. Gray.

MESSRS. MACMILLAN AND CO. will shortly publish a work on "The Theory and Practice of Absolute Measurements in Electricity and Magnetism," by A. Gray, M.A., Professor of Physics in the University College of North Wales. Though nominally a second edition of the small book by the same author published in 1884, it has been entirely re-written and extended in plan, so as to form a fairly complete treatise on the absolute measurement of electric and magnetic quantities. This has necessitated the division of the work into two volumes, of which the first, extending to over 450 pages, is about to be issued. The following is a synopsis of the contents :—Vol. I. contains a sketch of the theory of electro-statics and flow of electricity, chapters on units, general physical measurements, electrometers, comparison of resistances, comparison of capacities, and measurement of specific inductive capacities, and concludes with an appendix of tables of units, resistances, and useful constants. The chapter on the comparison of resistances contains full details of the various methods of comparing high and low resistances, calibration of wires, &c. ; the chapter on capacities discusses methods generally, and contains an account, as full as possible, of the principal determinations of specific inductive capacity made up to the present time. Vol. II. will contain an account of magnetic theory, units and measurements ; electro-magnetic theory and absolute measurement of currents, potentials and electric energy ; the definitions and realization of the ohm and other practical units ; the relations of electro-magnetic and electro-static units and the determination of v ; practical applications of electricity, and especially related points of theory and measurements. (This volume is in hand, and will be issued as soon as possible after Vol. I.) An attempt has been made to arrange the work so as to avoid any too sharp distinction between what is theoretical and what is practical, and at the same time preserve a logical order in the former and prevent the constant introduction of digressions on theory into accounts of instruments and processes of manipulation.

A WORK of some interest and importance, "Excursions zoologiques dans les Iles de Fayal et de San Miguel (Açores)," has just been producedby M. Jules de Guerne, at the expense of Prince Albert de Monaco. Of the new species mentioned, some, perhaps all, have been elsewhere recorded in contemporary periodicals. M. de Guerne concludes from his researches that the land fauna of the Azores has a definitely European character ; that the fresh-water fauna has the same character, many of the species composing it being probably cosmopolitan, most of them provided with powerful means of dissemination, which have enabled them to reach the Azores ; that most of the species have been brought by the wind and by birds, the wind playing only a secondary part ; that the lakes in the craters are of modern origin, due to the accumulation of rain-water, and have not taken long to people ; that the character of the aquatic types and the absence of any great struggle for existence suffice to explain this rapid peopling of the waters ; that the land species, like those of the water, have been fortuitously introduced from the nearest islands and continents, though at a remoter epoch and more distant intervals, this greater antiquity accounting for the greater differentiation of the land fauna, and in especial of the Mollusca; that the alpine character of the land fauna has not been demonstrated, and that, on the theory of the gradual submergence of the islands, the animals of the littoral region in retiring to the higher grounds would have there produced a varied and numerous assemblage of species, which, as a fact, is not found. Incidentally, M. de Guerne points out a mistake which has crept into works of importance—a sudden depth of 58 fathoms at a single spot being attributed to the little Lagoa Grande in the Island of San Miguel, instead of the true depth, which is about 17 fathoms.

THE Bancroft Company, San Francisco, announces that there will shortly be added to the series of guide-books to the Pacific Coast a hand-book of the Lick Observatory, which has been prepared by Prof. Edward S. Holden, Director of the Observatory. This book is intended to give all the information which will be of value to each one of the many visitors to the Lick Observatory, which possesses the largest and most powerful telescope in the world, and is situated in one of the wildest and most romantic portions of California. Besides the useful and necessary information of a mere guide-book, the work is to contain interesting and popular accounts of the various astronomical instruments, and of the way in which they are made and used. It will be illustrated by twenty or more woodcuts from photographs and drawings.

MR. HENRY BEDFORD, of All Hallows College, Dublin, writes to us :—"I see among the notes in your last number (p. 87) that Herr Sander, in his paper on some recently deciphered runic inscriptions in Sweden, says that 'in four of them appeared the word *Pim* or *Piment* (*i.e.* a strong drink composed of wine, honey, and spice), which, as well as *Klaret*, was mentioned in the *Saga* of Rollo the Ganger and the Normans,' and that 'all these inscriptions were referred to the close of the pagan age.' Now if the word *Klaret* refers like *Piment* to some kind of drink, does not this point to the direction in which we are to look for some more satisfactory explanation of our modern word *Claret* than that which our dictionaries give—as a derivation from the French *clairet*—although the word is not used in that language to describe the French wine to which we apply it. Perhaps you or some of your readers will throw some light upon the origin of this obscure word."

THE 800th anniversary of the University of Bologna will be celebrated on June 12 next. An oration will be delivered by the poet Giosué Carducci. There will also be a musical performance, an ode having been written for the occasion by Panzacchi, and set to music by Baron Franchetti.

Last week we printed a letter from M. Julius, of Delft, Holland, asking a question with regard to tables of reciprocals. Mr. T. S. Barrett and Mr. A. Freeman write to us recommending Barlow's tables of squares, cubes, square roots, cube roots, and reciprocals of all integers up to 10,000. The reciprocals are given to seven places of significant figures, besides the leading zeros. The work was edited by the late A. De Morgan, and published for the Useful Knowledge Society by Taylor and Walton, London, 1840.

Prof. Ball, General Director of the Science and Art Museum, Dublin, mentions in his report for 1887, that early in the year he brought before the Council of the Royal Irish Academy the desirability of its handing over to the Museum an old collection of moulds of Irish crosses and miscellaneous sculptures, together with casts, most of which had been prepared for the Exhibition of 1853. To this proposition the Academy cordially assented, and, after much piecing together of broken fragments, it was found that the material provided a very valuable and representative set of casts. It is proposed that casts of many objects of ancient Irish art not included in this collection shall also be obtained. The collection, when completed and properly arranged in the new Museum, ought to be of great service not only to archæologists but to workmen, who would be well rewarded for a careful and elaborate study of the ideas of the mediæval craftsmen of Ireland.

Herr H. Forsell has been chosen President of the Swedish Royal Academy of Science for the ensuing year, in place of Herr C. G. Malmström.

The Biological Society of University College will hold its annual *soirée* at the College on Thursday, June 7, beginning at 8 p.m. Prof. W. H. Flower, F.R.S., will deliver a lecture at 9 p.m. on "The Pygmy Races of Men." Tickets may be had on application to the secretaries of the Society.

We have received the *Annuaire* for the year 1888 of the Paris Society for the Encouragement of National Industry. Among the contents are a list of the members, and an extract from the programme relating to the prizes to be given by the Society from 1888 to 1893.

The Danish Government has granted a sum of £500 for the purpose of having the oyster-banks in Denmark examined by an expert. His object will be to ascertain the results of their continued preservation, with a view to the resumption of fishing.

Some months ago a large consignment of salmon ova was despatched from Denmark to Buenos Ayres, *viâ* Hamburg, for the stocking of certain lakes and rivers in the Argentine Republic. The experiment has proved very successful, the ova ariving in excellent condition, and further consignments are to be made.

The following incident in the trial of the great patent case, Edison and Swan Electric Light Company *v.* Holland and others, now proceeding in the Chancery Division of the High Court of Justice, before Mr. Justice Kay, is taken from the shorthand report in the *Electrician* of May 18. On May 16, Prof. James Dewar, F.R.S., Professor of Chemistry in the University of Cambridge was under examination. A small crucible was produced and handed to the witness, who said: In that crucible I have, with Mr. Gimingham, carbonized filaments in the precincts of the court, using no packing and no luting of any description. The filament was a thread so far as he could remember.

Sir Horace Davey urged that this did not arise out of the cross-examination.

Mr. Justice Kay said it should have been produced in the examination-in-chief. If it were pursued, Sir Horace Davey would be entitled to ask any questions upon it.

Sir Horace Davey, cross-examining:—About what heat was this produced at?—It was a mere experiment. It was a spirit-lamp that was used.

Do you suggest that this coil, or whatever you like to call it, has been heated to a sufficient heat for use as a conductor in an incandescent lamp? —Not at the present time.

Then it is not completely carbonized?—It is carbonized; but it does not conduct well enough. It wants to be heated for a longer time at a higher temperature.

Has it been heated to a degree at which the oxygen would combine with or attack the carbon?—That I cannot say. I think it is probably at a low red heat.

Mr. Justice Kay: I am very much disgusted. I am here trying all I can to understand the case, and this is clearly an attempt to mislead. I am greatly disgusted.

Prof. Dewar: I have no desire to mislead your lordship. I have stated that this was a mere experiment. I did not produce it. It was put to me.

Mr. Justice Kay: You may stand down.

The additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Mr. George Somerford; a Barbary Ape (*Macacus inuus* ♀) from North Africa, presented by Miss Waterman; a Brazilian Tree Porcupine (*Sphingurus prehensilis*) from Pernambuco, presented by Mr. Clement J. Bateman; a Barbary Wild Sheep (*Ovis tragelaphus*), from North Africa, presented by Mr. E. H. Forwood; a Greater Black-backed Gull (*Larus marinus*), British, presented by Prof. E. Ray Lankester, F.R.S., F.Z.S.; a Herring Gull (*Larus argentatus*), British, presented by Mr. E. Wright: a Cape Dove (*Œna capensis*), a Tambourine Pigeon (*Tympanistria bicolor*) from South Africa, presented by Mr. R. H. Milford; a White-handed Gibbon (*Hylobates lar*) from the Malay peninsula, a Chimpanzee (*Anthropopithecus troglodytes* ♀), a Marabou Stork (*Leptoptilus crumeniferus*) from West Africa, two Caracals (*Felis caracal juv*) from Africa, three Red-crowned Pigeons (*Erythrœnas pulcherrimus*), a Praslin Parrot (*Coracopsis barklyi*), two —— Kestrels (*Tinnunculus gracilis*) from the Seychelles, a Laughing Kingfisher (*Dacelo gigantea*), a Black-backed Piping Crow (*Gymnorhina tibicen*), a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, two Glass Snakes (*Pseudopus pallasi*) from Dalmatia, deposited; six Common Pintails (*Dafila acuta*), eight Common Teal (*Querquedula crecca*), eight Garganey Teal (*Querquedula circia*), ten Wigeon (*Mareca penelope*), a Shoveller (*Spatula clypeata*), British, purchased; a Red Kangaroo (*Macropus rufus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Comet 1888 *a* (Sawerthal).—At the beginning of last week, apparently on May 20 or 21, the comet suddenly became very much brighter, gaining fully three magnitudes. It has since faded again. Only a few observations have as yet come to hand, but it is to be hoped that everyone who has observed it during the last fortnight, and made any estimate of its brightness, will publish his observations without delay.

The Short Period Comets and Asteroids.—Prof. Kirkwood, who has already given reasons for thinking that two short period comets originally belonged to the group of asteroids, has extended his argument in the *Sidereal Messenger* for May to include the class of short period comets as a whole. He points out that, of the twenty comets concerned, seven have disappeared, either by dissolution into fragments, like Biela's comet, or by the transformation of the orbit by the influence of Jupiter, as in the case of Lexell's comet. The instances of the comets of Lexell and Wolf (1884) are representative, Prof. Kirkwood considers, of the mode in which asteroidal may have been changed into cometary orbits. Had the latter, indeed, been discovered before its perturbation, it would probably have been considered simply an asteroid of unusually long period, for its eccentricity and inclination were well within asteroidal limits. Of the twenty comets, not only have seven disappeared, but five, or, including Encke's and Biela's, seven, have periods commensurable with that of Jupiter; all the twenty have direct motion; all but one have smaller inclination than Pallas; and, as with the asteroids, there is a tendency of the perihelia to concentrate in the 180° from 290° to 110°.

NEW MINOR PLANET.—A new minor planet was discovered by M. Borrelly on May 12 at Marseilles. This may possibly, but not very probably, prove to be Xanthippe, No. 156. Should it be really a fresh discovery, it will rank as No. 278, whilst the one discovered by Herr Palisa on May 16 (see NATURE, vol. xxxviii. p. 89) will be numbered 279.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JUNE* 3-9.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on June 3

Sun rises, 3h. 49m.; souths, 11h. 57m. 57·6s.; sets, 20h. 7m.: right asc. on meridian, 4h. 47·4m.; decl. 23° 24′ N. Sidereal Time at Sunset, 12h. 58m.

Moon (New on June 9, 17h.) rises, 1h. 42m.; souths, 7h. 30m.; sets, 13h. 29m.: right asc. on meridian, 0h. 18·6m.; decl. 2° 54′ S.

Planet.	Rises. h. m.		Souths. h. m.		Sets. h. m.		Right asc. and declination on meridian. h. m.		° ′
Mercury..	5 4	...	13 35	...	22 6	...	6 25·2	...	25 23 N.
Venus ...	3 21	...	11 14	...	19 7	...	4 3·4	...	20 4 N.
Mars ...	14 19	...	19 57	...	1 35*	...	12 47·6	...	5 3 S.
Jupiter ...	18 39	...	23 0	...	3 21*	...	15 51·7	...	19 12 S.
Saturn ...	7 38	...	15 32	...	23 26	...	8 22·0	...	20 5 N.
Uranus ...	14 19	...	19 59	...	1 39*	...	12 49·8	...	4 37 S.
Neptune..	3 20	...	11 5	...	18 50	...	3 53·9	...	18 38 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

June.		h.		
8	...	20	...	Venus in conjunction with and 3° 39′ north of the Moon.

Variable Stars.

Star.	R.A. h. m.		Decl. ° ′				h. m.	
U Cephei ...	... 0 52·4	...	81 16 N.	...	June	6,	0 16	*m*
Mira Ceti ...	... 2 13·7	...	3 29 S.	...	,,	9,		*m*
R Leonis ...	... 9 41·5	...	11 57 N.	...	,,	4,		*M*
S Ursæ Majoris	... 12 39·1	...	61 42 N.	...	,,	9,		*M*
V Virginis ...	... 13 22·0	...	2 36 S.	...	,,	8,		*M*
U Coronæ ...	... 15 13·6	...	32 3 N.	...	,,	7,	22 20	*m*
U Herculis ...	... 16 20·9	...	19 9 N.	...	,,	3,		*m*
U Ophiuchi...	... 17 10·9	...	1 20 N.	...	,,	7,	23 48	*m*
W Sagittarii	... 17 57·9	...	29 35 S.	...	,,	8,	22 0	*m*
U Sagittarii...	... 18 25·3	...	19 12 S.	...	,,	3,	2 0	*m*
					,,	6,	1 0	*M*
R Scuti... ...	... 18 41·5	...	5 50 S.	...	,,	4,		*m*
β Lyræ... ...	... 18 46·0	...	33 14 N.	...	,,	3,	3 0	*m*
R Capricorni	... 20 5·0	...	14 36 S.	...	,,	4,		*M*
X Cygni ...	... 20 39·0	...	35 11 N.	...	,,	8,	21 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °		Decl. °		
Near Antares ...	... 248	...	20 S.		
,, σ Ophiuchi ...	... 260	...	5 N.	...	Rather slow.

GEOGRAPHICAL NOTES.

IN the Report of the Survey of India for 1886–87, Colonel Strahan gives an account of the survey and exploration of the Nicobar Islands by himself and party. A very careful survey of the whole group was made, and the coast-lines at last accurately laid down. Owing to the dense vegetation, the party were unable to penetrate any distance into the interior, and only a few heights could be measured. The culminating point of the whole group, 2105 feet above sea-level, stands near the south-east corner of Great Nicobar, the area of which is 375 square miles, the total area of the group being 678 square miles. The scenery, especially of Great and Little Nicobar, is of indescribable beauty. There are several rivers in the former island which are navigable by boats for some miles, especially the Galatea, on the south coast. Its course is very tortuous, the banks are fringed with tree-ferns, canes, bamboos, and tropical vegetation of infinite variety, through which occasional glimpses are obtained of high mountains in the interior covered with dense forests to their very summits, and generally cloud-capped. The country through which the stream runs is almost uninhabited; a few huts appear here and there tenanted by an inland tribe of savages called "Shom Pen," of whom very little is known, except that they are in such an utter state of barbarism as to be held in contempt even by the Nicobarese inhabiting the coasts. On most of the islands the forest grows luxuriantly down to the beach. Mangroves, except in the island of Kamorta, are not very plentiful, and in this respect these islands differ widely from the neighbouring Andaman group, where the creeks are fringed with mangroves mile after mile. The sea-beach consists largely of coral. The climate is very equable day and night all the year through, and most pleasant to one's feelings, but unfortunately its character for unhealthiness is only two well established. The rainfall, which averages about 100 inches, is pretty evenly distributed throughout the year. The thermometer stands very steadily between 80° and 85° in the shade, and hardly varies day or night. The inhabitants of these islands, Colonel Strahan states, are allied to the Malays, and are a complete contrast to their tiny, intensely black, woolly-haired neighbours, the Andamanese. The Nicobarese are very strong, thickly-built men, not much if at all inferior to Europeans in physique, of a reddish-brown colour. They are unconquerably lazy, having no inducement whatever to exertion. They have a wonderful talent for learning languages. Fortunately, Mr. Man, the Settlement Officer at Kamorta, who has done so much for Andaman anthropology, has been carefully studying the Nicobarese, their habits and language, and is now engaged on a book on the subject, which will shortly be published.

MR. C. M. WOODFORD, the successful naturalist explorer of the Solomon Islands, is about to leave England on a third visit to the group. After spending some time in various parts of the islands not previously visited, he will investigate Santa Cruz, Woodlark Island, and other islands lying to the south-east of New Guinea.

ACCORDING to the new Survey Report, triangulation surveys have already been effected over 15,000 square miles in Upper Burma, and the out-turn of reconnaissance surveys amounts to 11,000 square miles on the scale of 4 miles to an inch, in the following States and districts: Northern Shan States and Ruby Mines district, 3000 square miles; Southern Shan States, 3000; Yemethin and Mehtila district, 2000; Yaw country, 1000; Mandalay and Kyaukse districts, 2000.

IN the summary Report of the Geological Survey of Canada for 1887, some of the results are given of the expedition under Dr. G. M. Dawson last summer, of the exploration of British Columbia. Mr. Ogilvie's instrumental survey to the intersection of the Yukon with the 141st meridian will form a sufficiently accurate base-line for future explorations in this region. In addition to this the geographical results include the completion of an instrumental survey of the Sitkine to Telegraph Creek by Mr. McConnell, which is connected with Dease Lake by a carefully placed traverse by Mr. M'Evoy. Thence a detailed running survey was carried by the Dease, Liard, and Pelly Rivers, connecting with Mr. Ogilvie's line at the mouth of the Lewis River, a total distance of 900 miles. Taken in conjunction with Mr. Ogilvie's line, these surveys include an area of over 6000 square miles, the interior of which is still, with the exception of reports received from a few prospectors and from Indians, a *terra incognita*. The same remark may be applied to the whole surrounding region outside the surveyed circuit, but much general information has been obtained respecting the entire district, which will facilitate further explorations. The whole region is more or less mountainous, though intersected by wide areas of flat or valley country. The country, though generally mountainous in character, includes large tracts of flat and slightly broken land, and, according to Dr. Dawson, may eventually support a population as large as that found in corresponding latitudes in Europe.

THE anniversary meeting of the Royal Geographical Society was held on Monday in the hall of the University of London, General R. Strachey presiding. The report, which was read by Mr. Clements R. Markham, having been adopted, General Strachey was for the third consecutive year elected President of the Society. The Founder's Medal for the encouragement of geographical science and discovery was presented to Mr. Clements R. Markham, who retires from the honorary secretaryship after twenty-five years' service, in acknowledgment of the valuable services rendered by him to the Society during that period. Lieut. H. Wissmann was awarded the Patron's Medal in recognition of his great achievements as an explorer in Central Africa; Mr. J. M'Carthy, Superintendent of Surveys in Siam, the Murchison Grant; Major Festing, the Cuthbert Peek Grant, for his services as a cartographer on the Gambia River and the country in the neighbourhood of Sierra Leone. The Gill Memorial for 1888 was secured by Mr. Charles M. Doughty. Various scholarships and prizes to students in training colleges were also distributed. The President then delivered his annual address, passing in review the chief geographical events of the year.

THE LINNEAN SOCIETY.

THE hundredth anniversary meeting of this Society was held on Thursday last, 24th inst., at Burlington House, in the library, the usual meeting-room being inadequate for the reception of the large number of members present on this occasion. The President, Mr. Wm. Carruthers, F.R.S., took the chair at three o'clock, and was supported by the two former Presidents who are happily still with us—Prof. Allman and Sir John Lubbock—the Council of the Society, and many distinguished Fellows, amongst whom we noted Sir Richard Owen, Sir Joseph Hooker, Dr. Günther, Sir Walter Buller, Prof. Duncan, Mr. Romanes, Colonel Grant, and amongst the visitors Dr. Henry Woodward, F.R.S., and Mr. Studley Martin, a nephew of the founder.

After preliminary business, H.M. the King of Sweden was elected an honorary member. The Treasurer, Mr. Frank Crisp, laid the last year's accounts before the meeting, and briefly referred to the financial history of the Society during the century now closed. The senior Secretary, Mr. B. Daydon Jackson, presented an account of the Linneau collections from their formation, their purchase by the founder of the Society, and their possession by the Linnean Society. This was succeeded by the President's annual address, which was largely devoted to a review of the Society's past career. He spoke of the original quarto Transactions, then of the octavo Proceedings, finally of the Journal, of which forty-three volumes are extant. During the past year seven parts of the Transactions and twenty of the Journal had been issued, an amount equal to that published during fifteen years in the early part of the century.

A novel feature was then introduced, one of those intended to mark the centenary of the Society. Prof. Thöre Fries, the present occupant of Linnæus's Botanical Chair at Upsala, had been invited to pronounce a eulogium on his illustrious predecessor. As he was detained by his professorial duties in his University, his essay was read by the President. In it he spoke of the profound sleep of natural science during the Middle Ages, and the hard struggle which had to be fought before men of science could liberate themselves from a narrow orthodoxy, or the fetters they had themselves forged by attaching infallibility to Aristotle and classic authors. Linnæus bore an honourable part in placing the study of natural science on a logical basis by his clear definitions, and admirable nomenclature, and by the enthusiasm he was able to rouse in his disciples for the same methods. England, unluckily for Sweden, became his heir; many consequently are the ties which unite the memory of Linnæus with this country, the strongest perhaps being the Linnean spirit, the genuine spirit of freshness and enterprise in which scientific research is carried on in England.

Sir Joseph Hooker then pronounced a eulogy on Robert Brown, the greatest botanist of the present century. He specially dwelt on the evidence afforded by the "Prodromus" of his untiring industry, accuracy of observation and exposition, together with sagacity, caution, and soundness of judgment, in which he has not been surpassed. Where others have advanced beyond the goal he reached, it has been by working on the foundations he laid, aided by modern appliances of optics and physics. His memory was wonderful, he seemed never to forget a plant he had examined; and the same with his books—he could turn to descriptions for a statement or a figure without needing a reference. The noble title conferred upon him by Humboldt has been confirmed by acclamation by botanists of every country, "Botanicorum facile princeps."

Prof. Flower, C.B., F.R.S., delivered an address on Charles Darwin, who, he said, had special claims on their consideration, inasmuch as a large and very important portion of his work was communicated to the world by papers read before the Society and published in the Journal. His life was one long battle against our ignorance of the mysteries of living Nature, and he sought to penetrate the shroud which conceals the causes of all the variety and wonders round us. His main victory was the destruction of the conception of species as being fixed and unchangeable beyond certain narrow limits, a view which prevailed universally before his time. That other factors had operated besides natural selection in bringing about the present condition of the organic world was admitted even by Darwin himself. His work, and the discussions which had sprung from it, had marvellously stimulated research, and he had shown by his life and labours the true methods by which alone the secrets of Nature may be won.

Prof. W. T. Thiselton Dyer spoke on George Bentham, who presided over the Society from 1863 to 1874. A nephew of Jeremy Bentham, and trained to some extent under him, he was early imbued with a taste for method and analysis, and through his mother's fondness for plants he was led to study them, with marvellous results. The records of his life-work are astonishing. Whilst President he delivered a series of masterly addresses, and the latter part of his career witnessed the preparation of the "Flora Australiensis" and a full share of the "Genera Plantarum." He stood in the footsteps of Linnæus, and although the descent was oblique he inherited the mantle of the master whose memory was that day commemorated.

The President stated that the Council had decided to establish a Linnean Gold Medal, to be presented to a botanist and a zoologist in alternate years, but on this occasion it would be awarded in duplicate. The medal bore on the obverse a profile of Linnæus, modelled from the bust in the library; on the reverse, the arms of the Society and the name of the recipient. The President made the first presentation to Sir Richard Owen, recounting the chief services he had rendered to zoology. Sir Richard, with some emotion, expressed his high sense of the honour conferred, and thanked the Fellows for their cordial reception of him. The President then presented a similar medal to Sir Joseph Hooker, with a like recapitulation of the splendid services he had bestowed on botany. Sir Joseph suitably replied, returning his cordial thanks for the distinction.

The remaining formal business included the announcement of the newly-elected Councillors, and the re-election of the officers—Mr. Wm. Carruthers, President; Mr. Frank Crisp, Treasurer; and Messrs. B. Daydon Jackson, and W. Percy Sladen, Secretaries.

The annual dinner was held at the Hotel Victoria, Northumberland Avenue, at seven o'clock. The President took the chair, about sixty of the Fellows being present. In addition to the usual toasts, that of "The Medallists" was given, and replied to by Sir Joseph Hooker, who alluded to the fact that he had personally known eight of the Presidents of the Society, and that the founder himself induced his father, Sir William Hooker, to take up the study of botany. As a proof of his close connection with the Linnean Society, he added that his father, grandfather, father-in-law, and uncle had all been Fellows.

The final portion of the centenary celebration took place the following evening, when the President and officers held a reception at Burlington House. A special feature was made of the Linnean manuscripts and memorials, which were displayed in glass cases with descriptions, a catalogue of them being also distributed. Memorials of other distinguished naturalists were also shown, conspicuously those of Robert Brown and George Bentham, lent by Sir Joseph Hooker and M. Alphonse de Candolle, of Geneva, a foreign member of the Society.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Rede Lecture on June 8, by Sir F. A. Abel, will be upon applications of science to the protection of human life. It will be illustrated by experiments and the exhibition of appliances.

Mr. Percy Groom, B.A., late of Trinity College, has been elected to the Frank Smart Studentship in Botany at Gonville and Caius College.

The fittings of the new Chemical Laboratory are costing £1000 more than was originally estimated (from rough drawings only) by Mr. Lyon, Superintendent of the University workshops. Some of this is due to the fact that the fixing of the tables on a bottom independent of the floors of the rooms, and making the cupboard doors fairly dust-proof, originally recommended, was not adopted till after the tables had been fixed, and much cutting of the floors had to be done. Also much of the iron and steel work was not particularized at first.

The Council are taking steps to carry out the appropriation of the old Chemical Laboratory to the department of pathology.

Prof. Darwin will lecture during the long vacation on the theory of the potential, and on attractions, commencing on Tuesday, July 10. The lectures will treat principally of gravitational problems, including attraction of ellipsoids, Gauss's paper, heat of tin, Jacobi's and Dedekind's ellipsoids, oscillations of a fluid sphere, the foundation of the theory of tides, atmospheres of planets, &c.

SOCIETIES AND ACADEMIES.

London.

Royal Society, May 3.—"Effect of Chlorine on the Electromotive Force of a Voltaic Couple." By Dr. G. Gore, F.R.S.

If the electromotive force of a small voltaic couple of unamalgamated magnesium and platinum in distilled water, is balanced through the coil of a moderately sensitive galvanometer of about 100 ohms resistance, by means of that of a small Daniell's cell, plus that of a sufficient number of couples of iron and German silver of a suitable thermo-electric pile (see Proc. Birm. Philos. Soc., vol. iv. p. 130), the degree of potential being noted; and sufficiently minute quantities of very dilute chlorine water are then added in succession to the distilled water, the degree of electromotive force of the couple is not affected until a certain definite proportion of chlorine has been added; the potential then suddenly commences to increase, and continues to do so with each further addition within a certain limit. Instead of making the experiment by adding chlorine water, it may be made by gradually diluting a very weak aqueous solution of chlorine.

The minimum proportion of chlorine necessary to cause this sudden change of electromotive force is extremely small; in my experiments it has been 1 part in 17,000 million parts of water,[1] or less than 1/7000 part of that required to yield a barely perceptible opacity in ten times the bulk of a solution of sal-ammoniac by means of nitrate of silver. The quantity of liquid required for acting upon the couple is small, and it would be easy to detect the effect of the above proportion, or of less than one ten-thousand-millionth part of a grain of chlorine, in one-tenth of a cubic centimetre of distilled water by this process. The same kind of action occurs with other electrolytes, but requires larger proportions of dissolved substance.

As the degree of sensitiveness of the method appears extreme, I add the following remarks. The original solution of washed chlorine in distilled water was prepared in a dark place by the usual method from hydrochloric acid and manganic oxide, and was kept in an opaque well-stoppered bottle in the dark. The strength of this liquid was found by means of volumetric analysis with a standard solution of argentic nitrate in the usual manner, the accuracy of the silver solution being proved by means of a known weight of pure chloride of sodium. The chlorine liquid contained 2·3 milligrammes or 0·03565 grain of chlorine per cubic centimetre, and was just about three-fourths saturated.

One-tenth of a cubic centimetre of this solution ("No. 1"), or 0·003565 grain of chlorine, was added to 9·9 c.c. of distilled water and mixed. One cubic centimetre of this second liquid ("No. 2"), or 0·0003565 grain of chlorine was added to 99 c.c. of water and mixed; the resulting liquid ("No. 3") contained 0·000003565 grain of chlorine per cubic centimetre. To make the solutions ("No. 4") for exciting the voltaic couple, successive portions of $\frac{1}{10}$ or $\frac{1}{20}$ c.c. of ("No. 3") liquid were added to 900 cubic centimetres of distilled water and mixed.

[1] As 1 part of chlorine in 17612 million parts of water had no visible effect, and 1 in 17000 millions had a distinct effect, the influence of the difference, or of 1 part in 500,000 millions, has been detected.

I have employed the foregoing method for examining the states and degrees of combination of dissolved substances in electrolytes, and am also investigating its various relations.

May 17.—"Magnetic Qualities of Nickel." By J. A. Ewing, F.R.S., Professor of Engineering, University College, Dundee, and G. C. Cowan.

The experiments described in the paper were made with the view of extending to nickel the same lines of inquiry as had been pursued by one of the authors in regard to iron (Phil. Trans., 1885, p. 523). Cyclic processes of magnetization have been studied, in which a magnetizing force of about 100 C.G.S. units was applied, removed, reversed, again removed, and re-applied, for the purpose of determining the form of the magnetization curve, the magnetic susceptibility, the ratio of residual to induced magnetism, and the energy dissipated in consequence of hysteresis in the relation of magnetic induction to magnetizing force. Curves are given, to show the character of such cycles for nickel wire in three conditions: the original hard-drawn state, annealed, and hardened by stretching after being annealed. The effects of stress have also been examined (1) by loading and unloading magnetized nickel wire with weights which produced cyclic variations of longitudinal pull, and (2) by magnetizing while the wire was subjected to a steady pull of greater or less amount. The results confirm and extend Sir William Thomson's observation that longitudinal pull diminishes magnetism in nickel. This diminution is surprisingly great: it occurs with respect to the induced magnetism under both large and small magnetic forces, and also with respect to residual magnetism. The effects of stress are much less complex than in iron, and cyclic variations of stress are attended by much less hysteresis. Curves are given to show the induced and residual magnetism produced by various magnetic forces when the metal was maintained in one or other of certain assigned states of stress; also the variations of induced and residual magnetism which were caused by loading and unloading without alteration of the magnetic field. Values of the initial magnetic susceptibility, for very feeble magnetizing forces, are stated, and are compared with the values determined by Lord Rayleigh for iron, and the relation of the initial susceptibility to the stress present is investigated. The paper consists mainly of diagrams in which the results are graphically exhibited by means of curves.

Chemical Society, May 3.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—The determination of the molecular weights of the carbo-hydrates, by Mr. H. T. Brown and Dr. G. H. Morris. The law established by Blagden in 1788, that the lowering of the freezing-point of aqueous solutions of inorganic salts is proportional to the weight of substance dissolved in a constant weight of water, was extended by de Coppet in 1871–72, who pointed out that when the lowering of the freezing-point is calculated for a given weight of the substance in 100 grammes of water, the result, which he termed the coefficient of depression, is constant for the same substance, and that the coefficients for different substances bear a simple relation to their molecular weights. Raoult extended the law to organic substances and to other solvents than water, and showed that when certain quantities of the same substance are successively dissolved in a solvent upon which it has no chemical action, there is a progressive lowering of the point of congelation of the solution, and that this lowering is proportional to the weight of the substance dissolved in a constant weight of water. The "coefficient of depression," A—that is, the depression of the point of congelation produced by 1 gramme of the substance in 100 grammes of the solvent—is given by the formula $\frac{C \times y}{x \times 100} = A$, where C is the observed depression produced by x grammes of the substance dissolved in y grammes of the solvent, and from this value the "molecular depression," T, is calculated by the formula $M \times A = T$, where M is the molecular weight of the substance in question. T is a value varying with the nature of the solvent, but remaining constant with the same solvent for numerous groups of compounds, whence it follows that A and T being known, the molecular weight of the substance in question may be determined from the equation $M = T/A$. This method of Raoult's, which is of value in cases where a vapour density determination is not possible, has been employed by the authors to determine the molecular weights of the following carbo-hydrates: dextrose, cane-sugar, maltose, milk-sugar, arabinose, and raffinose, and also that of mannitol (the solvent being water), with results which lead to formulæ identical with those ordinarily adopted for these substances.—The molecular weights of nitric peroxide and nitrous anhydride, by Prof. Ramsay. The molecular weight of nitric peroxide as determined by Raoult's method in acetic acid solution, accords with the formula N_2O_4. No definite results could be obtained with nitrous anhydride since dissociation occurred at the temperature of experiment (16°).—In the discussion which followed the reading of these papers, and in which Prof. Debus, F.R.S., Dr. Perkin, F.R.S., and others took part, Mr. Wynne remarked that most results hitherto obtained by Raoult's method pointed to a complete dissociation of the complex molecules present in solids and liquids, and would seem to show that the dissociation is not dependent on the particular solvent employed; Mr. Crompton referred to the great irregularities noticeable on comparing the molecular depressions of various substances as determined by Raoult, and thought that until more was known of the cause of such irregularities, and of the mechanism of the changes under discussion, such results as those brought forward by Messrs. Brown and Morris should be accepted with great reservation; and Prof. Armstrong, F.R.S., observed that, apart from the information as to the comparative molecular weights of dissolved substances which Raoult's method promised to afford, it appeared that, in order to gain as complete an insight as possible into the molecular composition of solids and liquids it was important to vary in every way the proportions of substance dissolved as well as the solvent.—The action of heat on the salts of tetramethylammonium, by Dr. A. T. Lawson and Dr. N. Collie. In the majority of cases, the salts examined decompose in a simple manner, yielding trimethylamine and a salt of methyl.—The action of heat on the salts of tetramethylphosphonium, by Dr. N. Collie. The salts of tetramethylphosphonium with the oxy-acids, when heated, undergo as a rule two changes: the first and most important is the production of trimethylphosphine oxide and a ketone, and the second, which occurs only to a very limited extent, results in the formation of trimethylphosphine and a salt of methyl.

Geological Society, May 9.—Mr. W. T. Blanford, F.R.S. President, in the chair.—The following communications were read:—The Stockdale Shales, by J. E. Marr, and Prof. H. A. Nicholson. The Stockdale Shales extend in an east-north-east to west-south-west direction across the main part of the Lake District, parallel with the underlying Coniston Limestone Series and the overlying Coniston Flags, with both of which they are conformable. They also occur in the neighbourhood of Appleby, and in the Sedbergh district. They are divisible into a lower group of black and dark gray and blue Graptolite-bearing shales, interstratified with hard bluish-gray mudstones, containing Trilobites and other organisms, and an upper group of pale greenish-gray shales, with thin bands of dark Graptolitic shales. The lower group (Skelgill Beds) are well seen in the stream which runs past Skelgill Farm, and enters Windermere near Low Wood; while the upper group (Browgill Beds) occurs fully developed in the Long Sleddale Valley, and its beds are very fossiliferous in Browgill. The authors divided these into a series of fossil-zones, and the beds were compared with the corresponding beds in Sweden, Bohemia, Bavaria, &c. The fossils other than Graptolites were shown to occur elsewhere in strata of Llandovery-Tarannon age, from which it was concluded that the Stockdale Shales occupy that horizon. A fault occurs everywhere between the Middle and Lower Skelgill Beds, except perhaps in the Sedbergh district; but it does not seem to cut out a great thickness of rock, and the authors gave reasons for supposing that it was produced by one set of beds sliding over the other along a plane of stratification. The beds are found to thicken out in an easterly direction, and the possibility of the existence of land in that direction was suggested. The authors directed attention to the importance of Graptolitoidea as a means of advancing the comparative study of the stratified deposits of Lower Palæozoic age. A description was given of the following new species and varieties:—*Phacops elegans*, Boeck and Stars, var. *glaber*, *Cheirurus bimucronatus*, Murch., var. *acanthodes*, *Cheirurus moroides*, *Acidaspis erinaceus*, *Harpes judex*, *H. angustus*, *Ampyx aloniensis*, *Proëtus brachypygus*, and *Atrypa flexuosa*.—On the eruptive rocks in the neighbourhood of Sarn, Caernarvonshire, by Alfred Harker.

Zoological Society, May 1.—Prof. Flower, F.R.S., President, in the chair.—Colonel Irby exhibited (on behalf of Lord Lilford) a specimen of *Aquila rapax* from Southern Spain, believed to be the first authentic specimen of this species known from the Peninsula.—Prof. Flower exhibited and made remarks on a specimen of the Japanese Domestic Fowl with the tail-coverts enormously elongated, the longest attaining a length of 9 feet. The specimen had been presented to the British Museum by Mr. F. D. Parker.—Mr. C. M. Woodford made some general remarks on the zoology of the Solomon Islands, and read some notes on the nesting-habits of Brenchley's Megapode, which lays its eggs in the sands on the sea-shore of these islands.—Mr. G. A. Boulenger read the description of a new Land-Tortoise of the genus *Homopus* from South Africa, based on specimens living in the Society's Gardens, which had been presented to the Society by the Rev. G. H. R. Fisk. The author proposed to name the species *H. femoralis*.—Mr. F. E. Beddard read the second of his series of notes on the visceral anatomy of birds. The present paper treated on the air-sacs in certain diving birds.—Mr. Francis Day read the first of a proposed series of observations on Indian fishes.

Royal Meteorological Society, April 18.—Dr. W. Marcet, F.R.S., President, in the chair.—The following papers were read:—Jordan's new pattern photographic sunshine recorder, by Mr. J. B. Jordan. The improvement in this instrument over the previous pattern of sunshine recorder consists in using two semi-cylindrical or D-shaped boxes, one to contain the morning, and the other the afternoon chart. An aperture for admitting the beam of sunlight is placed in the centre of the rectangular side of each box so that the length of the beam within the chamber is the radius of the cylindrical surface on which it is projected; its path therefore follows a straight line on the chart at all seasons of the year. The semi-cylinders are placed with their faces at an angle of 60° to each other. They are fixed on a flat triangular plate which is hinged to a suitable stand having levelling screws attached, and fitted with a graduated arc as a means of readily adjusting and fixing the cylinders to the proper vertical angle agreeing with the latitude of the station where used.—On the meteorology of South-Eastern China in 1886, by Dr. W. Doberck. This paper gives the results of observations made at the Custom-houses and lighthouses by officers of the Imperial Chinese Maritime Customs. In summer there is very little change of temperature with latitude. The temperature depends upon the distance from the nearest sea coast, and is greatest at stations farthest inland. The highest mean temperature occurred in July, and the lowest in January. The north-east monsoon blows from September to June, and the south monsoon during July and August; the latter does not blow with half the force of the former. Rainfall is greatest in Northern Formosa, and least in Northern China. Along the east coasts of Formosa and Luzon the winter is the wet season, while in China July seems to be the wettest month of the year.—Lightning in snowstorms, by Prof. A. S. Herschel, F.R.S.—Insolation, by Mr. Rupert T. Smith.

EDINBURGH.

Royal Society, May 7.—Lord Maclaren, Vice-President, in the chair.—Dr. G. Sims Woodhead communicated a paper written by Mr. Robert Irvine and himself, on the secretion of carbonate of lime by animals.—A paper by Mr. Irvine and Mr. George Young, on the solubility of carbonate of lime under different forms in sea-water, was also read.—Dr. Alexander Bruce described a case of absence of the *corpus callosum*, in the human brain.—Dr. J. Murray discussed the distribution of some marine animals on the west coast of Scotland.—Mr. W. E. Hoyle described some larvæ of certain Schizopodous Crustacea from the Firth of Clyde.

May 21.—The Rev. Prof. Flint, Vice-President, in the chair.—A series of photographs of the Nice Observatory, presented by M. Bischoffsheim through the Astronomer-Royal for Scotland, were exhibited.—A note by Prof. Cayley, on the hydrodynamical equations, was communicated. The author discusses the result of the elimination of the symbol denoting the pressure by differentiation of the three fundamental hydrokinetical equations.—Dr. Archibald Geikie treated fully the history of volcanic action during Tertiary time in the British Islands.

PARIS.

Academy of Sciences, May 22.—M. Janssen, President, in the chair.—Obituary notice of M. Hervé Mangon, member of the Section for Rural Economy, and Vice-President of the Academy for the year 1888, by the President. M. Mangon, who was born in Paris on July 31, 1821, and died there on

May 15, 1888, may be regarded as the founder of agronomic science, to which he devoted many years of assiduous labour. To him France is indebted for the introduction of all the more useful agricultural processes. He also gave a great stimulus to the associated science of meteorology, and rendered important services to ballooning, especially in connection with military tactics.—On the part played by atmospheric nitrogen in vegetable economy, by M. E. Chevreul. A few summary observations are made in reference to the memoir recently presented to the Academy by MM. Gautier and Drouin. These observers having announced as a result of their personal experiments and as something new to science that the gaseous nitrogen of the atmosphere is absorbed by plants, it is pointed out that the Commission appointed in 1854 to investigate the question decided in favour of M. Georges Ville's theory and against that of M. Boussingault. Since then the part played by atmospheric nitrogen in the vegetative process has been carefully studied both in France and Germany, and hitherto the results, such as those of MM. Gautier and Drouin, have tended to confirm the conclusions first arrived at by M. Georges Ville.—The sardine on the Marseilles coast, by M. A. F. Marion. The sardine appears yearly in these waters, where a total of 409,055 kilogrammes were taken during the period between March 1887 and the end of February 1888. Details are given regarding the food, migrations, and breeding-season of this fish.—Study of the planet Mars, by M. F. Terby. Three small round spots, white and brilliant, are visible on the continuation of *Erebus* (left or west side), when the *Trivium Charontis* is midway from the central meridian in the eastern half of the disk. These spots, at first scarcely perceptible, become brighter and whiter as they approach the limb, where they become diffused by irradiation like the polar spot. The black line, which seems to divide the north polar spot, has been perfectly visible since May 12. Facing it on the outer side is a small hyperborean tract, white or snowy, but less brilliant and white than the true polar spot, of which it seems at first sight to form an integral part. It is evidently the same phenomenon as that which has recently been simultaneously observed by M. Perrotin, as well as by M. Schiaparelli.—On an electro-chemical actinometer, by MM. Gouy and H. Rigollot. Copper oxidized or covered with basic salts, and plunged into water or into a solution of sulphate of copper, is known to undergo variations of electromotor force under the action of light, effects which can be clearly indicated only with intensely luminous means. But the authors find that the oxidized copper plunged into a solution of metallic chloride, bromide, or iodide becomes, on the contrary, extremely sensitive to luminous rays even of slight intensity, and may consequently be employed as an actinometer. Details are given of the process by which they have constructed the apparatus based on this phenomenon.—Determination of the heat of combustion of a new solid substance isomerous with benzine, by M. W. Louguinine. Five experiments with a beautiful specimen of this substance, discovered by M. Grimer, give a mean of 10,863·9 calories for the heat liberated in the combustion of 1 grain. The heat of combustion of benzine is much less (776,000 cal.), corresponding to a body whose constitution is absolutely different from that of the isomerous substance.—On the Pliocene formations of the Montpellier district, by M. Viguier. In this paper the conclusions are summed up of an extensive investigation of this geological area. Three distinct groups are determined: (1) Arnusian, fresh-water deposits, puddings and gravels, with remains of *Elephas meridionalis*; (2) Astian, also fresh-water, clays and marls, with remains of *Semnopithecus monspessulanus*, *Helix quadrifasciata*, *Triptychia sinistrorsa*, &c.; (3) Plaisancian, marine deposits, sandy and other marls, with remains of *Potamides basteroti*, *Melampus myotis*, *Rhinoceros leptorhinus*, *Mastodon brevirostris*, &c.

BERLIN.

Meteorological Society, May 1.—Dr. Vettin, President, in the chair.—Dr. Perlewitz spoke on aperiodic variations of temperature. He based his researches on the observations made at Berlin during the forty years 1848–87, and during ninety-three years, 1791–1883, at Breslau. If a year is divided into halves, the first half is characterized by a normal curve of rising temperature, the second half by a similarly normal curve of falling temperature. Both curves, however, show negative irregularities, whose number may be very considerable in any one month: thus in May these irregularities (fall of temperature) occurred on more than thirteen days as against seventeen days on which the curve rose regularly; and similarly, in October, there were more than twelve days on which an irregularity (rise of temperature) was observed as against nineteen days with a normally falling temperature. On the whole the number of these irregularities is greater in the first half of the year than in the second, so that the heat of the second half is greater than that of the first. A whole series of interesting details exists in connection with the number, magnitude, and periodic duration of the changes of temperature during both the normal and abnormal times; these cannot however be considered here.—Dr. Vettin communicated the results of his observations on the daily periodicity in the velocity of the wind, extending over a period of two years. From direct determination of the movement of smoke coming from a chimney, and from observations with a home-made anemometer, he found that in addition to the well-known maximum velocity of the wind which occurs at midday, there is a second maximum just after midnight. This latter maximum is very small in summer, but in winter, on the other hand, it is much greater and even exceeds that maximum which occurs at midday. This second maximum is not very marked as an average on the whole year. The speaker then gave a detailed description of the construction of his anemometer, which he exhibited to the Society. He further described a spring vane which he had made, which he has erected at the window of his house in a moderately wide street; this vane indicates accurately not only the direction of the wind which is blowing up or down the street, but also of any wind which may be blowing over the houses at right angles to this. Experiments made with tobacco-smoke in a glass-covered chamber have shown that the wind which blows over the houses gives rise to ascending and descending currents of air along their walls, causing an elevation or depression of the vane. The vane also records accurately the direction of a wind which blows at any angle other than at right angles to the axis of the street. Suitable as this spring vane is for observers who live in narrow streets, it is specially adapted for observations in narrow mountain valleys, in which the direction of the wind cannot be ascertained by any other means.

Physical Society, May 4.—Prof. von Bezold, President, in the chair.—Prof. Schwalbe gave expression to the loss which the Society had sustained through the death of Prof. Hoh, for many years an active collaborator with the "Fortschritte der Physik."—In the election which then followed, Prof. Kundt, the new Director of the Physical Institute, was chosen as first Vice-President in the place of the late Prof. Kirchhoff.—Dr. Koenig spoke on the instantaneous photographs made by Ottomar Anschütz, of Lissa, accompanied by demonstrations and examples of the photographs. Anschütz began taking instantaneous photographs in 1882, operating at first upon bodies of troops during the manœuvres. Later on, at the instance of the Minister of War, he photographed horses and riders moving at every sort of pace. In addition, up to 1885, he busied himself with photographing many animals in the different and frequently very bizarre positions in which they place themselves during their movements. Some of the most interesting photographs taken at this time are those of storks. From 1885 onwards he has been taking serial-photographs of men and animals in motion, obtaining pictures of the consecutive stages of each movement. From these serial-photographs it is possible to draw many scientific deductions, by following the course of the centre of gravity of the object in the successive pictures of horses and men when running and jumping. A complete knowledge of the mechanics of motion can, however, only be arrived at from these series of photographs when the interval of time between each consecutive member of the series is equal and extremely small, a result which Anschütz has nearly obtained. Latterly he has taken pictures of large masses in motion, such as processions, &c. The numerous photographs which the speaker exhibited and briefly explained, testified completely to the technical excellence at which Anschütz has already arrived. The apparatus used for instantaneous photography was exhibited at the same time.

Physiological Society, May 11.—Prof. du Bois-Reymond, President, in the chair.—Dr. Koenig spoke on his measurement of the intensities of light in the spectrum. The method employed was as follows. A circular field of vision was divided into two halves, of which one was illuminated with some colour of the spectrum of fixed intensity, usually with red; the colour to be compared with this was then applied to the other half, and made to vary until it produced the sensation of a light-intensity equal to that of the red. The first measurements were made on Dr. Broddahn, whose eyes are dichromatic (green colour-blind).

By taking the mean of the separate determinations for different parts of the prismatic spectrum, Dr. Koenig had constructed a curve for the light-intensity of all the colours of the spectrum; there was a difference of at most 2 per cent. between the values of the separate measurements and the mean. The speaker then made similar measurements with his own normal trichromatic eyes; in this case he obtained a greater difference between the value of the separate determinations and the mean (up to 5 per cent.) but the curve of light-intensity for the whole range of the spectrum was found to be identical with that obtained from Dr. Broddahn. By reducing the prismatic spectrum used in these experiments to one produced by diffraction, he was able to calculate the curve of light-intensity for a normal spectrum. Comparing this curve with those which he had obtained, in conjunction with Dr. Dieterici, for the sensations of the three primary colours, red, green, and blue (as determined for each point in a normal spectrum), he found that the curve of light-intensity of the spectrum was identical with that for the sensation of red. From this it must be concluded that the sensation of luminous intensity for each separate light is simply dependent on the amount of red contained in it, or, to state this more accurately, the brightness of each kind of light is determined by the extent to which it stimulates the red-perceiving fibres of the retina. Dr. Koenig had some time ago given expression to the conjecture that in the dichromatic eye it is not the fibres for the perception of the third colour which are wanting (the red-perceiving for red colour-blindness and green-perceiving for green), but that they are, so to say, differently tuned; tuned down in those who are colour-blind to green, so that they can only perceive the sensation due to light as red, tuned up to a higher pitch in those who are red colour-blind, so that when they are stimulated by rays of greater wave-length they only perceive green. It is now possible to verify the above conjecture experimentally as follows. The measurements of luminous intensities throughout the spectrum were made upon the eye of another person who was colour-blind, and this time on one who was red colour-blind; in this case the curve obtained was identical with that of the sensation of green. The phenomena observed by Dove, that the relative luminous intensities of red and blue vary according to the intensity of the illumination, were verified by Dr. Koenig, but only up to a certain limit; beyond this limit, the *relative* luminosities of these two colours underwent no further alteration in the brightness of the illumination.—Prof. Gad discussed Prof. Fick's views on blood-pressure in the capillaries, which the latter believed he had placed on an experimental basis by means of an artificial vascular scheme; according to this the pressure in the capillaries could not be much less than in the arteries, and only sinks appreciably as the capillaries are passing over into the veins. Prof. Gad showed that the conditions existing in the above scheme cannot be applied to the blood-capillaries; he further pointed out that the requisite data for calculating the true blood-pressure in the capillaries can be obtained from a theoretical consideration of the rate of flow in, and sectional area of, these vessels, and from this the pressure would appear to be about half of that which exists in the aorta. A true basis for any theory of capillary blood-pressure can only be obtained from such experimental investigation as admits of being applied to various parts of the purely theoretical consideration.

Stockholm.

Royal Academy of Sciences, April 11.—Prof. W. C. Williamson, of Manchester, was elected a foreign member of the Academy.—Critical remarks on the researches of Foeppl on the electrical conductibility of the vacuum, by Prof. Edlund.—A theory of isohydric solutions, by Dr. Arrhenius.—Remarks on the fossils of the Cretaceous formation of Sweden, by Prof. B. Lundgren.

May 9.—On *Triglaps pingelii*, an Arctic fish, found for the first time off the shores of Sweden, and on some specimens of *Syrrhaptes paradoxus* lately shot in Sweden, by Prof. F. A. Smitt.—The whale of Swedenborg (*Balæna swedenborgii*, Liljeborg) found in the diluvial strata of Sweden, described by Dr. Carl Aurivillius.—On the anazotic, stored up nutriments of the Gramineæ, by Dr. C. J. Johanson.—A generalization of the researches of Laplace on the libration in the orbits of the planets, by Dr. K. Bohlin.—On the points of approximation in the theory of perturbation, by the same.—Some extracts from the report of the French scientific expedition to Spitzbergen and other places in the years 1838, 1839, and 1840, by C. B. Lilliehöök, R.N.—Contributions to the theory of the undulatory movement in a gaseous medium (conclusion), by Dr. A. W. Bäcklund.—Derivates of the δ-amido-naphthaline-sulpho-acid, by Prof. P. T. Cleve.—Derivates of the γ-amido-naphthaline-sulpho-acid, by the same.—On naphthol acids, by Dr. Å. G. Ekstrand.—On abnormal forms of the first abdominal appendices of some female cray-fishes, by Dr. D. G. Bergendahl.—On two new Lamellibranchiates from the Arctic post-glacial beds of Scania, by Herr G. Clessin, of Ochsenfurth, Bavaria.

Amsterdam.

Royal Academy of Sciences, April 27.—Mr. J. A. C. Oudemans spoke of Airy's double-image micrometer, and stated the result of his efforts to discover the conditions to which this apparatus must be made to conform, in order that the value of one screw-turn may be independent of the adjustment of the eye. He had found that the distance from the first to the second lens must be equal to the focal length of the first lens—a condition already fulfilled in the micrometer for another purpose.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

British Petrography: J. J. Harris Teall (Dulau).—A Manual of Orchidaceous Plants, Part 3 (Veitch).—Longmans' Commercial Mathematics (Longmans).—A Wanderer's Notes, 2 vols.: W. Beatty-Kingston (Chapman and Hall).—Principles of Agricultural Practice: J. Wrightson (Chapman and Hall).—Discromatopsia, Enrico dal Pozzo di Mombello (Scariglia, Foligno).—Soaps and Candles: J. Cameron (Churchill).—Die Regenverhältnisse der Iberischen Halbinrel: G. Hellmann (Pormetter, Berlin).—Proceedings of the Geologists' Association, February (Stanford).

CONTENTS.

THURSDAY, JUNE 7, 1888.

TECHNICAL EDUCATION.

WE are glad to see that the Government Bill for the Promotion of Technical Instruction (which we print elsewhere) is down for second reading as first order of the day on June 14. The objects effected by this Bill are substantially the same as those of the Government Bill of last year, and of that already introduced by Sir Henry Roscoe and other friends of education, this year, on behalf of the National Association for the Promotion of Technical Education. That is to say, it is an enabling Bill, giving powers to localities, if they think fit, to apply local rates to the purpose of promoting technical instruction.

In Clause 6, "technical instruction" is defined to mean "instruction in the principles of science and art applicable to industries, and in the application of special branches of science and art to specific industries or employments." It does not include teaching the practice of any trade, or industry, or employment; but, subject to this reservation, it includes "instruction in the branches of science and art with respect to which grants are for the time being made by the Department of Science and Art, and any other form of instruction which may for the time being be sanctioned by that Department by a minute laid before Parliament, and made on the representation of a School Board or local authority that such a form of instruction is required by the circumstances of its district." This definition appears good, so far as it goes, but in our opinion it does not go far enough, for it does not specifically include, as Sir Henry Roscoe's Bill does, the commercial subjects and modern languages. This, however, may easily be amended by a slight alteration of the wording of Clause 6, which should read: "Technical instruction means instruction in subjects applicable to industry and commerce, and in the application of special branches of science and art to specific industries and employment." It is, however, to be noticed that Clause 5 suggests the possibility of Imperial grants in aid of instruction in technical subjects in the words, "Every minute of the Department of Science and Art with respect to the condition on which grants may be made for technical instruction shall be laid on the table of both Houses of Parliament." What the precise nature and amount of such grants may be is not stated, and we shall await with interest the explanation of the Government on this essential point.

In any case, however, it will be necessary that such grants should be accompanied by inspection under Imperial authority, but this does not necessarily form part of the Bill, which, after all, is one simply for giving rating power, and only contains one compulsory clause, viz. that in which School Boards availing themselves of the provisions are required to grant similar powers to voluntary schools in their districts claiming such powers, up, be it always understood, to the limit of one penny in the pound.

There are many points of difference between this Government Bill and that of last year. In the first place, the clause giving powers, granted by the last Bill, to fifty ratepayers to demand a poll is very wisely omitted from this Bill. In the second place, under the Bill of last year the powers of promoting technical instruction could only be exercised by School Boards or by Town Councils where School Boards do not exist. No provision was made for districts in which neither exist. Under the present Bill, where a School Board does not exist, the powers may be exercised by any local authority which can carry out the Public Libraries Acts, and this gives, of course, a much wider sphere of action than the former Bill. But, more than this, the present Bill gives power to Town Councils and other local authorities to grant aid from the rates (even where a School Board exists) to supply higher technical instruction, whereas under the former Bill technical instruction both of an elementary and of a higher character was in the hands of one authority, viz. that of the School Board. Another new point is that the annual rate in aid for technical instruction is limited to one penny in the pound in the case of that levied by the School Board, and at twopence in the pound where the powers given under the Public Libraries Acts are exercised concurrently. In the Bill introduced on behalf of the National Association no such limit is named. Possibly, in view of Parliamentary objections, some limitation is advisable, although very serious objections may be raised to this proposal. Admission to technical schools and classes, may, under Sir Henry Roscoe's Bill, be granted to all comers who pay the required fees; powers being, however, given to Boards and local authorities to institute an entrance examination in reading, writing, and arithmetic, should they think fit. The Government adhere to their former proposal to restrict all attendance in these schools and classes (with the exception of those in which manual instruction alone is given) to such pupils as shall have passed an examination equivalent to that of the Sixth Standard. The exception made this year in favour of manual instruction is a step in the right direction. We should have preferred perfect freedom of admission in the Technical, as is now the case in the Science and Art Classes of the Department, or at least to leave it to the locality to determine whether any such entrance examination is advisable or not.

No powers are granted in the Government Bill respecting payment of fees to deserving students or for the establishment of scholarships, as in Sir H. Roscoe's Bill. These seem to be minor defects, which can be easily remedied. A more important point, and one concerning which not only much discussion in the House of Commons may be expected, but upon which the success or failure of the Bill will probably depend, is the much-vexed question of whether, and, if so, under what conditions, any aid from local rates can be given for the special purposes of technical instruction to public elementary schools not under control of a School Board, *i.e.* to voluntary or denominational schools. Here the difference of opinion between the two great political parties is very marked. One party will not on any consideration sanction payment from the rates unless the spending of this is placed under the definite control of the ratepayers; the other will not permit the Board schools to reap a distinct advantage which is withheld from those carried on by voluntary enterprise. The Bill of the Association summarily cuts the Gordian knot by specifically excluding voluntary schools from participation in income derived from the rates; naturally, therefore, denying to any higher institution of a distinctly denominational type similar assistance. Sir Hart Dyke's Bill, on the other hand, having in its first clause declared that "Any School Board in England may from time to time supply or aid the supply of such manual or technical instruction, or both, as may be required for supplementing the instruction given in any public elementary school in its district, whether under its own management or not," goes still further in its second clause, and makes distinct provision as to the equality of treatment between Board schools and voluntary schools such that, if the Board aids its own schools, "it shall, on the request of the managers of any other public elementary school in its district fulfilling like conditions as to the supply of manual or technical instruction in that school, aid the supply of such instruction in that school in like manner as it aids such supply in the school or schools under its own management, subject to such terms as may be agreed on or determined in pursuance of this Act." Moreover, if the managers object to these terms, the Department of Science and Art shall act as umpire. The support or opposition to this Bill by those who object to payment from the rates without representation, and therefore the probable success or defeat of the measure, will, we venture to think, much depend upon the exact meaning which the Government attaches to these "terms of agreement." If the expression may be taken to mean that the School Board shall have some direct representation by its members on the governing body of the voluntary schools to whom that Board makes grants, *quâ* the technical instruction given in such schools, some of the opposition may possibly be removed. But this should be distinctly expressed; indeed, it would be better to make such an arrangement imperative. If this meaning is not to be attached to these words, we fear that the Bill will lose the support of very many ardent educationalists in the House.

Another provision which we do not find in the Government measure is the one contained in the third clause of the Association Bill, and also in the fourth clause of the Government Bill of last year, in which School Boards may join together to contribute towards the promotion of technical instruction, power being already possessed for this purpose by local authorities under the Public Libraries Acts. This power, in the case of small or sparsely-populated districts, is especially important, with a view to the foundation of higher elementary technical schools, which from their nature do not need to be very numerous, and which the School Boards of many of the single areas of the kind included in the Bill would be quite unable to create or maintain.

The above by no means exhausts the points which may be brought up for discussion on this Bill. It will, however, serve to show the general scope of the Bill, which, unless greatly modified, cannot, we fear, be considered a satisfactory one.

OLD BABYLONIAN AND CHINESE CHARACTERS.

The Old Babylonian Characters and their Chinese Derivates. By Terrien de Lacouperie. (London: Nutt, and Trübner and Co., 1888.)

PROF. TERRIEN DE LACOUPERIE has long been known as the advocate of a theory which would bring the ancestors of the Chinese from Western Asia, and see in the characters they employed derivatives from the cuneiform symbols once in use in Babylonia. The proofs of his theory have been gradually placed before the learned world. In two articles published in the Journal of the Royal Asiatic Society he has endeavoured to trace the history of the Yh-King, the oldest and most mysterious of Chinese books, and to show that its earliest portions contain lists of characters and their meanings, ancient poems and similar fragments of antiquity, misunderstood and misinterpreted by successive generations of commentators. Elsewhere he has given us for the first time a rational account of the vicissitudes undergone by the Chinese system of writing, based upon the statements of the Chinese writers themselves. Lately he has communicated to the Philological Society an interesting and exhaustive description of the languages spoken in China before the arrival of the "Bak" tribes or Chinese proper, as well as of the modern dialects which are descended from them. Now we have the last instalment of his proofs in the shape of a comparison between the primitive forms of the Chinese characters and the pictorial forms out of which the cuneiform script subsequently developed. Prof. de Lacouperie claims to have proved in a typical number of instances that the correspondence is exact, or fairly so, as regards form, signification, and phonetic value; and that consequently an early connection between Chinese and Babylonian must be assumed. Since the Babylonian forms can be shown to presuppose those of China, we must bring the Chinese from the West, and not conversely the Babylonians from the East.

I am not a Sinologist, and therefore can pronounce no opinion on the Sinological side of the argument. Chinese scholars must determine how far Prof. de Lacouperie's restoration of the primitive forms and values of the Chinese signs is correct. Assuming it to be so, the resemblance between many of them and the corresponding characters of Accadian Chaldæa is certainly surprising.

On the Babylonian side, Prof. de Lacouperie has been at great pains to secure accuracy, and has left but little to criticize. *Zik*, however, it may be observed, is not a value of the Babylonian ideograph of "ship," but goes back to an erroneous conjecture of Dr. Hincks; and the original meaning of the character which has the value of *pa* was "the leaf" or "leafy branch" of a tree.

The Babylonians seem never to have forgotten that the cuneiform characters they used had originated in pictures. Indeed, their scribes long claimed the privilege of adding to them, the result being that hieroglyphic forms took their place in the texts by the side of forms that had long degenerated into a cuneatic shape. The original hieroglyphics had been the invention of the so-called Accadians, the early population of Chaldæa, who spoke agglutinative dialects, and were eventually superseded by the Semites.

The Semites received the hieroglyphics from their inventors after they had already assumed a cuneatic form, and added still further to the heritage. When the Semitic king Sargon I. was reigning in Babylonia in B.C. 3800, the scribes at his court were still occupied in devising new forms of characters, and in increasing the number of phonetic values the student was required to learn. This is the cause of the fact pointed out by Prof. de Lacouperie, that, whereas most of the cuneiform characters have to be turned on their sides in order to be restored to their primitive position (Chaldæan writing having once been traced in vertical columns), there are other characters which have never been thus displaced. As time went on, the forms of the characters became more and more distorted; the number of persons in Babylonia who could read and write was very large, and while the general form of script varied from age to age, the individual in each age was distinguished by a peculiar form of handwriting as much as is the individual of to-day. An official scrip never prevailed in Babylonia as it did in Assyria, where education was practically confined to the class of scribes; and while, therefore, the Assyrian student has little need of learning more than one form of writing as long as he confines himself to the monuments of Assyria, he is bewildered by the number of cursive hands which the documents of Babylonia oblige him to decipher.

The oldest Babylonian monuments yet known are those discovered by the French Consul M. de Sarzec at Telloh in Southern Babylonia. They are earlier than the epoch of Sargon I., and belong to the pre-Semitic era. The inscriptions engraved upon them still preserve in some measure the old vertical arrangement of the characters, and in some few cases the characters themselves have a pictorial form. But more generally they have already become cuneatic, and not unfrequently have departed so widely from their primitive appearance as to make it impossible even to guess what they were primarily intended to represent. If this were the case in the fourth millennium before our era, we may have some idea of the vast antiquity to which the beginnings of Babylonian writing must reach back.

In other instances, though the transformation of the character is not so complete, it is difficult to determine with certainty the object originally portrayed. Some of Prof. de Lacouperie's examples are in this plight, and as regards at least two of them—those pronounced *da* and *du* or *tur*—I prefer the explanations suggested by Mr. Pinches and Mr. Bertin to those suggested by himself. In fact, in the first case he has misinterpreted, like the earlier Assyriologists, the Assyrian explanation of the ideograph *nasu sa nisi;* which signifies, not "the summit of man," but "the lifting up of a man." It is consequently natural to regard it as representing the uplifted arm.

Prof. de Lacouperie rejects the theory which saw in the mountains of Elam the birth-place of Babylonian writing. Whatever, however, may be the value of the arguments urged by the advocates of this theory, the arguments brought against it by Prof. de Lacouperie do not appear to me to be cogent. Certainly it is not my experience that the coast of a flat country like Chaldæa "always looks mountainous" to the seafarer; while the Accadian word *a* (misprinted *ai*) signifies "father," not because of the ideographic meaning of the character which represented it, but because the Accadian *ada*, "father," became in pronunciation, through phonetic decay, first *ad*, and then *a*. The symbol of "country" attached to the ideographs of "man" or "servant," "handmaid" and "wild ox," need not have been introduced before the Accadians had long been settled in the Babylonian plain, and it is not quite correct to say that "while [Babylonian writing] possesses primitive symbols for 'boat' and for 'wind,' represented by an inflated sail, there are none for 'river.'" Both "ship" and "river" are alike denoted by a double ideograph.

The question, however, whether the cuneiform system of writing originated in "the mountains of the East," as the Babylonians called them, or in the islands of the Persian Gulf, does not affect Prof. de Lacouperie's main contention. If this can be established, a new and important chapter will be opened in the history of the ancient East, and the mystery which has so long enveloped the origin of the Celestial Empire will be cleared away. I must leave it to the Sinologists to determine whether, on the Chinese side, Prof. de Lacouperie's conclusions are sustainable; on the Babylonian side, he has nothing to fear from Assyrian scholars.

A. H. SAYCE.

DR. EIMER ON THE ORIGIN OF SPECIES.

Die Entstehung der Arten auf Grund von Vererben erworbener Eigenschaften nach den Gesetzen organischen Wachsens. Von Dr. G. H. Theodor Eimer, Professor der Zoologie und vergleichenden Anatomie zu Tübingen. (Jena: Gustaf Fischer, 1888.)

IT is a little curious that, although Darwin was so much more an experimenter than an anatomist, the immediate stimulus of his work was to anatomy, and not to experiment. There is, however, ample evidence that morphology is beginning to advance on the lines prophesied for it at the end of the "Origin of Species," and that morphologists are to enter the "almost untrodden field of inquiry on the causes and laws of variation, on correlation, on the effects of use and disuse, on the direct action of external conditions."

Dr. Eimer's book is written from the stand-point of one who believes that there is more to be made out of the study of the influence of the environment on a single set of organisms than of the anatomy and microscopy of many organisms. It is an abundant storehouse of facts, old and new, about the influence of the physical environment. Many curious problems are dealt with, and the infinite fertility of the field of investigation is shown. But the book claims to be far more than this: it claims to supply a new theory of the organic world—a theory in which natural selection plays only a casual and incidental part.

Dr. Eimer starts from the premiss that natural selection is insufficient to account for the evolution of the organic world because it is essentially the rule of chance. One had thought that this misconception had, even in the controversy of the ignorant, long ago died of inanition. Not only is the whole tenour of Darwin's book opposed to such a conception, but Darwin has specifically guarded against it. For him and for his theory "chance" is but a convenient way of denominating processes of whose details, from their complexity or from their intricacy, we are ignorant.

From his study of the life-conditions of some lizards, Dr. Eimer has reached the conclusion that at any given time variations occur only in a few definite directions. These directions depend on inner constitutional causes. The variations are produced by the direct action of the environment, are always transmitted, and when accumulated, become the inner constitutional cause determining the direction in which the organism will respond to new stimuli. In old males which have been subjected for a longer time than other forms to the environment there is a tendency to the appearance of new characters. These show the direction in which species-variation is going to take place. Not only does the ontogeny repeat the phylogeny in a condensed form, but the later stages of the ontogeny are prophetic of the new phylogeny. Variation, so directed and limited, assimilation causing growth, and reproduction or discontinuous growth, are the chief laws of organic growth.

Suppose a primitive undifferentiated plasma capable of responding to stimuli of heat, light, moisture, &c. In response to the action of the environment ever slightly varied in such details, various conditions would "crystallize out" of the plasma, just as from a homogeneous inorganic mass crystals form in varied groups. As the organic world continued to grow, this original differentiation would increase. With increase of complexity due to the storage in each generation of the complete effect of the environment on each stage of the phylogeny, the different directions in which forms were developing would become more different. Each new character appearing would through correlation influence the whole organism. Allow a little to natural selection and a little to the results of sexual mingling, and the varied species, orders, and classes into which the organic world can now be divided appear as the inevitable result of its mode of growth. There is no need to search for intermediate forms: they may never have existed. As the branching of a tree is the natural consequence of its mode of growth, so is separation and isolation inevitable in the whole organic world.

The two crucial points in Dr. Eimer's theory are his view of the action of the environment and his extreme Lamarckian acceptance of the transmission of acquired characters. Probably he is correct in his supposition that the extent of the direct action of the environment has as yet been unappreciated. Many characters hitherto unexplained may come to be referred to direct action, and experiment only can determine its scope. But it is no explanation of the presence of chlorophyll to refer it with the author to the continued action of sunlight upon protoplasm. And still less is it an explanation of the difference between queen and worker bee to refer it to the difference in their food. But indeed in this latter case the refutation of the author is easy. The neuter is not a different kind of bee produced by a different kind of food. It is merely an arrested queen—a queen that has not become something else on account of a different diet, but a queen that is not quite a queen because it has not had enough to eat. That this is the true state of the case is apparent from the less specialized colonies of wasps. There the queen in spring lays female eggs, and has herself to forage for the whole brood. As a result the young do not get enough to eat, and the development of their sexual organs is arrested. They in turn help to feed the next brood, the individuals of which reach a further state of development. As the summer wears on, the ever-increasing band of workers bring in an increasing supply of food, till finally a condition is reached when there is enough food to make perfect females of a whole brood. Clearly the bee colony, with its sharper distinction between neuter and queen, is merely a specialization of this condition. It is but a verbal explanation of the difference between queen and neuter to refer it to the direct action of food upon the organism. Moreover, to explain the condition of things even in the wasp colony, natural selection is necessary. Obviously, insufficient food would arrest general development as well as sexual development, and natural selection acting on variations naturally arising had to select those whose genitalia suffered most with least detriment to general powers. From the many interesting cases adduced by the author, this one has been selected because it is fairly typical of the slight grounds on which he refers important characters to the direct action of the physical environment.

As for the inheritance of acquired characters, it may be said at once that Dr. Eimer has added nothing of importance to the controversy. He certainly has adduced a few isolated cases that seem to be explained best on this theory; and were the inheritance of acquired characters merely of incidental value to his argument, his easy acceptance of the traditional view might avoid criticism. But when it is said that the direct action of the environment, together with inner constitutional causes, produces varieties and species, and that these inner constitutional causes that determine the direction of variation are merely a summation of direct action, a summation effected by inheritance, we perceive at once that a new and all-important *rôle* is assigned to heredity. There is no attempt to meet the serious theoretical difficulties involved in every conception of the mechanism of the inheritance of acquired characters: there is no adequate attempt to establish the fact. Were it possible and were it true, undoubtedly it would be, as Dr. Eimer in elaborate and learned detail has shown, of immense importance. But to prove its possibility or truth Dr. Eimer has done little or nothing.

Dr. Eimer appears to have mistaken a generalized expression of the process of evolution for an explanation of it. Natural selection acts at a time only on the one or two characters which the environment temporarily elevates into criteria of existence. But, as these change, there are changed with them a vast multitude of minor characters—in a word, there results what the author happily calls "kaleidoscopic variation." These changes can be referred only indirectly to selection, though they may play no inconsiderable part in determining the appearance of the organism. With all these variations are correlated variations in the results produced by the direct physical action of the environment.

Dr. Eimer has concentrated his attention on these secondary and certainly neglected changes, and his theory is a statement of their course. But he has brought forward no motive power to take the place of natural selection in determining the ruling changes; and therefore his generalized statement, even when raised into a law and dignified with a name, is not an explanation of the phenomena. Darwin has convinced men of evolution where Lamarck failed and where certainly Dr. Eimer would fail, not because he discovered any law, but because he discovered an intelligible mechanism, an obvious sequence of cause and effect, which could, and probably did, act. P. C. M.

OUR BOOK SHELF.

The Birds of Dorsetshire: A Contribution to the Natural History of the County. By J. C. Mansel-Pleydell, B.A., F.L.S., &c. 8vo. pp. i–xvi., 1–179. (London and Dorchester: R. H. Porter, 1888.)

Notes on the Birds of Herefordshire, contributed by Members of the Woolhope Club. Collected and Arranged by the late Henry Graves Bull, M.D., &c. pp. i–xxxii., 1–274. (London and Hereford: Jakeman and Carver, 1888.)

COUNTY lists of birds are still the order of the day. First we have Mr. Mansel-Pleydell's book on the Ornithology of Dorsetshire, a very neat little volume, compiled evidently with the greatest care. The author's long acquaintance with the country and his well-known love of natural history have rendered him the most competent authority on the subject, and he has been aided by many well-known naturalists in supplying him with instances of the capture of rare birds, so that the list is a very complete one. The inevitable Great Black Woodpecker (*Picus martius*) of course appears, on Pulteney's authority, but no recent specimen is extant, nor is likely to be. The Pied-billed Grebe (*Podilymbus podiceps*), which was first recorded by ourselves as a British bird, is placed between brackets, and considered to be "extremely doubtful" by the author. All we can say is that we should not have been godfather to the specimen, to add one more doubtful species to the already overburdened British list, unless we had felt tolerably certain of its authenticity, while the fact of the specimen being immature renders its occurrence as a chance wanderer much *more* probable than if it had been an adult bird in breeding-plumage. The bird has ten times more claim to a place amongst our stragglers than such species as *Picus medius*, *Pycnonotus barbatus*, and dozens of others. A most interesting history is given of the celebrated swannery at Abbotsbury, with a photographic plate, in which the birds are well depicted, but the keeper's face lacks expression! Some pretty woodcuts by Mr. Lodge are interspersed in the text. The author informs us that *Puffinus obscurus* (p. 113) should be *P. griseus*.

Dr. Bull's "Birds of Herefordshire" is one of the most useful of the county lists; for it contains a complete list of British birds, with special notes on the Herefordshire species. A great deal of care has evidently been taken over this book, which is rendered more interesting by the poetical researches of the author. Mr. Phil. Robinson, when he issues a new edition of his "Poets' Birds" will certainly have to consult this work of Dr. Bull, which contains many quotations we have not seen elsewhere.

R. BOWDLER SHARPE.

Geology for All. By J. Logan Lobley, F.G.S., &c. (London: Roper and Drowley, 1888).

THE object of this little book is to give an account of the important facts and deductions in geology, without "unnecessary scientific terminology." That there is room for such a work will not be questioned, and doubtless many who have paid no heed to the subject would begin to study it if only their lessons were made easy and attractive. This was accomplished in old times by Hugh Miller, and more recently by Canon Kingsley in his charming "Town Geology"; and Mr. Lobley, in his enthusiastic preface, raises the hope that he will follow a similar course, and provide "all intelligent readers" with a simple record of the earth's history. In this respect, however, we are disappointed. The work is a condensed account of the leading geological facts and deductions, arranged much after the fashion of an ordinary text-book. Of its general accuracy and clearness we can speak with confidence; and indeed, through his long connection with the Geologists' Association, the author has had ample opportunities of qualifying himself for his task. The work, however, is more adapted for the young student who wishes to pursue the subject, than for the general reader. We fear the patience of the latter will be tried when he reads the explanations—and not always happy explanations—of outcrops, anticlinals, unconformities, and outliers, for there are no diagrams to give pictorial aid. Nor is the chapter on the composition of rocks likely to prove more readable; for surely the accounts of the physical characters of minerals, and the chemical formulæ, introduce "unnecessary scientific terminology." Again, when we read of the acidic and basic rocks, of the seismic focus and the meizoseismic curve, of the "homocircle (*sic*) or equal-lobed tailed fishes," and of those that present a "heterocircle-tailed character," we feel that the author has not sufficiently carried out his good intentions. In the chapter on metamorphic rocks a popular account might have been given of recent researches in the Highlands, and then perhaps the author would not have remarked that "rarely a reversed-fault is seen." H. B. W.

Sound, Light, and Heat. By Thomas Dunman.
Electricity and Magnetism. By the same Author. (London: Ward, Lock, and Co., 1888.)

THESE two books are revised reprints of the articles on the subjects which have already appeared in Messrs. Ward, Lock, and Co.'s well known "Universal Instructor." They have been published in their present form for the convenience of students. The work of revision and expansion has been undertaken by Mr. Chapman Jones, the death of the original author having rendered it necessary for other hands to perform this part of the work.

As might be expected, the books are of a popular character, but their value to students of elementary physics does not in the least suffer on this account. The almost entire absence of mathematical statements makes them suitable for the most elementary students.

The method of treatment is that of the orthodox text-book, and there is very little that calls for special remark. They differ mainly from other elementary text-books inasmuch as they are brought quite up to date, especially in electrical matters. The 300 diagrams which are distributed throughout the text, though not of a high order of excellence, will do much towards enlightening the minds of those who read the books.

Though not designed to suit the syllabus of any examining body, they are well adapted for students preparing for the Science and Art Department examinations.

Sea-side and Way-side. By Julia McNair Wright. (Boston: D. C. Heath and Co., 1888.)

THIS little volume is the first of a series of "Nature Readers," intended for the use of beginners in reading. As a rule, the authors of reading-books take little trouble to excite the interest of children. Their object is to bring together a number of simple sentences, and they seem to be indifferent whether the sentences express sense or nonsense. In the present series an attempt will be made to convey, through reading-lessons, some of the more attractive elementary facts of science; and, if we may judge from the degree of success attained in "Sea-side and Way-side," the volumes are likely to be cordially welcomed in many primary schools in England as well as in the United States. The author has taken, as the subjects of her lessons, crabs, wasps, bees, spiders, and shell-fish; and she has contrived to put into the simplest and most direct language a great deal of really useful and entertaining information. Almost all children find something to interest them in what they are told about the habits of animals, and it is not improbable that these bright and pleasant lessons will implant in a good many young minds the seeds of an enduring love of natural history.

Reminiscences of Foreign Travel. By Robert Crawford. (London: Longmans, Green, and Co., 1888).

MR. CRAWFORD is already favourably known as the author of "Across the Pampas and the Andes." The present volume will maintain his reputation as a traveller who knows how to observe what is most significant in the countries he visits, and who possesses the faculty of reproducing his impressions in a lively and attractive narrative. His reminiscences relate to Canada, Austria, Germany, Sardinia, Egypt, Algeria, and various other lands; and in every chapter he records something that most readers will find fresh and interesting. The most instructive sections of the book are, upon the whole, those relating to Canada and Algeria.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Dr. Giglioli and Lepidosiren.

DR. GIGLIOLI asserts, in his interesting letter published in the last issue of NATURE (p. 102), that the Lepidosiren whose capture he records is "the fifth specimen known." Reference to his earlier remarks (NATURE, vol. xxxv. p. 343), concerning that which he regards as "the fourth known" specimen, shows that while he has acknowledged the examples of Natterer and Castlenau, he has apparently overlooked that of Bibron and H. Milne-Edwards, recorded in 1840. Readers of NATURE interested in this wonderful creature, now apparently verging on extinction, will find a *résumé* of all that is topographically important concerning the last-named and the three previously recorded specimens in the *Zoolog. Jahrb.* for 1887 (pp. 575 to 583). For this welcome communication, to which a full bibliography is appended, we are indebted to Dr. G. Baur, of Yale College Museum, U.S.A. It forms one of the series of historical miscellanea with which he has enriched our recent literature; and, if the conclusions at which he (in common with Brühl) arrives are sound, Dr. Giglioli's "fifth" specimen will be in reality a sixth.

Zoologists in general will unite in congratulating Drs. Rodriguez and Giglioli upon their recent acquisition; and while hoping for a repetition of the same, they will eagerly await the results of the promised "future study." G. B. HOWES.

South Kensington, June 2.

"A Text-book of Biology."

WILL you allow me to point out that the reviewer, in your issue of May 17 (p. 52), apparently misunderstands the object of my "Text-book of Biology"? The work is not meant to supplant lectures, but to aid them, by reducing for the student the wearisome labour of note-taking, and by enabling the teacher to enlarge where necessary, and to treat the subject from other points of view, running meanwhile less risk of addressing an audience of mere scribbling-machines.

The review also implies that a previously published work covers the same ground as the present book. This, however, is not the case, as my book deals with the *Botany* as well as with the Zoology of the course.

I cannot but think that the reviewer is led by his enthusiasm into the common mistake of demanding that the ordinary "pass" man shall follow the same course as the specialist. I suppose that the University of London prescribes at the Intermediate Pass stage a portion, not too small, of Biology, which shall form part of a general course of science adapted to the average student, and to *the time at his disposal;* perhaps your reviewer will kindly explain, less vaguely, what other system he would propose to substitute? J. R. AINSWORTH DAVIS.

Aberystwyth, May 24.

Resistance of Square Bars to Torsion.

THE attention of writers on Applied Mechanics should be called to the error continuously repeated in about thirty editions of the late Prof. Rankine's different works which have appeared during the last thirty years. The error is still reproduced in quite recent works of other writers: Prof. Ewing's article, "Steam-Engine," in the Encyclopædia Britannica; Prof. Unwin's "Elements of Machine Design"; Prof. Alexander's "Elementary Applied Mechanics"; &c.

It is stated that the moment of resistance of a square bar to torsion appears from Saint-Venant's investigations to be—

$$0{\cdot}281\, fh^3,$$

where f = maximum intensity of stress, and h = side of the square. This formula is also quoted at discussions of Institutions of Engineers and accepted without dissent. It is easily seen to be wrong, because the moment of torsion of a round bar of equal area is only

$$0{\cdot}282\, fh^3.$$

The error is reproduced in the text of Prof. Cotterill's "Applied Mechanics," but is corrected in an appendix, where the author says Rankine gives the formula without further explanation. The explanation is that on the old theory the torsional moment of inertia was—

$$I = \frac{h^4}{6},$$

which had to be multiplied by the maximum intensity of stress and divided by the corresponding radial distance—namely, from the centre to the middle of the side, giving the moment of resistance

$$= \frac{fh^3}{3}$$

on the old theory. (Rankine was aware that the maximum stress does not occur at the angles, as in Coulomb's method.)

Now, in Saint-Venant's "Mémoire," the *torsional rigidity* of a square bar is proved to be the fraction

$$0{\cdot}843$$

of the fallacious result of the old theory. Rankine accordingly wrote

$$0{\cdot}843 \times \frac{fh^3}{3} = 0{\cdot}281 fh^3$$

as the true moment of torsion.

But the *torsional rigidity* determines the *amount of twist*, and not the *maximum stress*. A few pages farther on, Saint-Venant gives the correct formula, equivalent to

$$\mathbf{0{\cdot}208}\, fh^3.$$

It seems strange that the talented author of the expressive distinctions *strain* and *stress* should himself have taken the formula for the strain instead of that for the stress. The reason is, that up to that date (Todhunter's "History of Elasticity") the strain and stress were supposed to be proportional to each other.

Abstracts of Saint-Venant's researches are given in Sir William Thomson's article "Elasticity," in the Encyclopædia Britannica, Thomson and Tait's "Natural Philosophy," and Minchin's "Statics." Strange that in all of these the method is given which determines the *strain* to be 0·843 of the old fallacy, while nothing is said about what is of more importance in Applied Mechanics, the maximum *stress*, nor the *moment of resistance to torsion*, as given above.

Perhaps this hint may be attended to in future editions.

T. I. DEWAR.

Engineering Academy, 721 Commercial Road, E.

THE GEOLOGICAL STRUCTURE OF SCANDINAVIA AND THE SCOTTISH HIGHLANDS.

THE obvious connection and analogy between the geological structure of the crystalline rocks of the Highlands of Scotland and those of Scandinavia have long engaged the attention of geologists. Among the northern observers to whose labours we are largely indebted for our knowledge of the Scandinavian regions, Dr. A. E. Törnebohm has proved himself a keen and indefatigable explorer of the Swedish uplands. Many years ago he showed that above clay-slates and limestones, with recognizable Silurian fossils, there lies a great thickness of quartzites, gneisses, and schists, called by him the Seve group. In more recently studying the relations of these rock-masses, he encountered some great difficulties, of which he sent me at the time an account. I could not pretend to solve them, but suggested, as at least a working hypothesis, that the Scandinavian structure might be fundamentally similar to that now recognized as characteristic of the North-West Highlands, where the apparent conformable superposition of a series of schists upon fossiliferous Lower Silurian strata has been produced by great terrestrial displacements, whereby the overlying rocks have been crushed and deformed, until they have assumed a new crystalline structure along the planes of movement, these stupendous changes having occurred at some time subsequent to the Lower Silurian period. I have recently received from Dr. Törnebohm the following letter, which he gives me leave to publish, and which will no doubt be read with interest by those who are aware of the recent progress of research in this subject:—"It will perhaps interest you to learn that your suggestion four years ago regarding the construction of our Scandinavian *fjelds* has turned out to be correct, at least in my opinion. My late researches have little by little driven me to the conclusion that the crystalline schists belonging to what I have called the 'Seve group' have been placed over Silurian strata by an enormous eastward thrust. I admit that I have most reluctantly come to this conclusion, knowing that it implied a horizontal thrust of enormous masses of rock for more than 100 kilometres. Such a stupendous movement of entire mountain-regions is hard to realize; but facts are stubborn things."

It will be observed that Dr. Törnebohm speaks of the movement having been towards the east, whereas in the north-west of Scotland it has been in the opposite direction. In a more recent letter, in reply to one in which I had called his attention to this difference, he says:—"Though in Scotland the great thrusts are westward, in Scandinavia it is quite the reverse. Here the chief movement has been to the east or south-east. In the region of Trondhjem, indeed, there have been lesser movements towards the north-west, but these may have taken place somewhat later. At least I rather suspect this, but am not prepared positively to affirm it." I may remark that in Scotland also there are districts where the thrusts have not come from the normal direction but from the westward. In the Island of Islay, for example, I recently found the limestones and quartzites piled up by sharply-cut thrust-planes which had a general westward inclination at lower angles than the displaced strata. One of the great problems in working out the complicated geology of the Highlands is the determination of the positions and extent of such thrust-planes, and the direction in which the displaced rock-masses have been moved. There can be little doubt that much mutual help in this research will be gained by a co-operation between the field geologists who are engaged in the study of these problems in Scotland and in Scandinavia.

ARCH. GEIKIE.

TIMBER, AND SOME OF ITS DISEASES.[1]

VIII.

THERE is a large and important class of diseases of standing timber which start from the cortex and cambium so obviously that foresters and horticulturists, struck with the external symptoms, almost invariably term them "diseases of the bark"; and since most of them lead to the production of malformations and excrescences, often with outflowing of resinous and other fluids, a sort of rough superficial analogy to certain animal diseases has been supposed, and such terms as "canker," "cancer," and so forth, have been applied to them.

Confining our attention to the most common and typical cases, the following general statements may be made about these diseases. They usually result from imperfect healing of small wounds, the exposed cortex and cambium being attacked by some parasitic or semi-parasitic fungus, as it tries to heal over the wound. The local disturbances in growth kept up by the mycelium

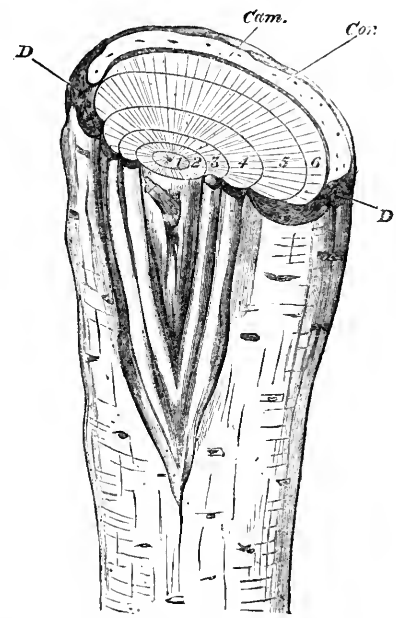


FIG. 28.—Piece of tree stem affected with "canker." The injury commenced after the two inner zones of wood (1 and 2) had been developed: it extended further in successive periods of growth, as shown by the receding zones 3, 4, 5, and 6, until all the cambium and cortex was destroyed except the pieces *D* to *D*. *Cam*, cambium; *Cor*, living cortex; *D D*, dead tissues. At each period of growth the attempt has been made to heal over the wound, as shown by the successively receding lips.

feeding on the contents of the cells of these tissues lead to the irregular growths and hypertrophies referred to; the wounds are kept open and "sore," or even extended, and there is hardly any limit to the possibilities of damage to the timber thus exposed to a multitude of dangers.

In Fig. 28 is represented a portion of a tree stem affected with "canker": the transverse section shows the periods of growth numbered 1 to 6 from within outwards. When the stem was younger, and the cambium had already developed the zones marked 1 and 2, the cortex suffered some injury near the base of the dead twig, below the figure 1. This injury was aggravated by the ravages of fungus-mycelium, which penetrated to the cambium and destroyed it over a small area: in consequence of this, the next periodic zone of wood (marked 3) is of course incomplete over the damaged area, and the cortex and cambium strive to heal over the wound by lip-like callus at the margins. The healing is prevented,

[1] Continued from p. 111.

however, by the mycelium, which is continually extending the area of injury: consequently the next zone of wood (4 in the figure) extends even a shorter distance round the stem, and so on with 5 and 6, the cambium being now restricted to less than half round the stem—*i.e.* from *D* to *D*, and the same with the living cortex. Of course the injured area extends upwards and downwards also, as shown by the lips of the healing tissue. As soon as the injury extends all round, the stem dies—it is, in fact, ringed. It is also interesting to note that the zones 4 and 5 (and the same would be true of 6 when completed) are thicker than they would have been normally: this is partly due to release from pressure, and partly to a concentrated supply of nutritive materials.

Much confusion still exists between the various cases: some of them undoubtedly are due to frost or to the intense heat of direct insolation; these are, as a rule, capable of treatment more or less simple, and can be healed up. Others, again, can only be freed from the irritating agents (which, by the bye, may be insects as well as fungi) by costly and troublesome methods.

I shall only select one case for illustration, as it is typical, and only too well known. As examples of others belonging to the same broad category, I may mention the "canker" of apple-trees, beeches, oaks, hazels, maples, hornbeams, alders, and limes, and many others; and simply pass the remark that whatever the differences in detail in the special cases, the general phenomena and processes of reasoning are the same.

Perhaps no timber disease has caused so much consternation and difference of opinion as the "larch-disease," and even now there is far too little agreement among foresters either as to what they really mean by this term, or as to what causes the malady. The larch, like other timber-trees, is subject to the attacks of various kinds of fungi and insects, in its timber, roots, and leaves; but the well-known larch-disease, which has been spreading itself over Europe during the present century, and which has caused such costly devastation in plantations, is one of the group of cancerous diseases the outward and visible signs of which are manifested in the bark and young wood.

The appearance presented by a diseased larch-stem is shown in Fig. 29. In the earlier stages of the malady the stem shows dead, slightly sunken patches, *a*, of various sizes on the cortex, and the wood beneath is found to cease growing: it is a fact to be noted that the dead base of a dried-up branch is commonly found in the middle of the patch. The diseased cortex is found to stick to the wood below, instead of peeling off easily with a knife. At the margins of the flattened patch, just where the dead cortex joins the normal living parts, there may frequently be seen a number of small cup-like fungus fructifications (Fig. 29, *b*), each of which is white or gray on the outside, and lined with orange-yellow. These are the fruit-bodies of a discomycetous fungus called *Peziza Willkommii* (Htg.), and which has at various times, and by various observers, received at least four other names, which we may neglect.

In the spring or early summer, the leaves of the tree are found to turn yellow and wither on several of the twigs or branches, and a flow of resin is seen at the dead patch of cortex. If the case is a bad one, the whole branch or young tree above the diseased place may die and dry up. At the margins of the patch, the edges of the sounder cortex appear to be raised.

As the disease progresses in succeeding years, the merely flattened dead patch becomes a sunken blistered hole from which resin flows: this sinking in of the destroyed tissues is due to the up-growth of the margins of the patch, and it is noticed that the up-growing margin recedes further and further from the centre of the patch. If this goes on, the patch at length extends all round the stem or branch, and the death of all that lies above is then soon brought about, for, since the young wood and cambium beneath the dead cortex are also destroyed, the general effect is to "ring" the tree.

To understand these symptoms better, it is necessary to examine the diseased patch more closely in its various stages. The microscope shows that the dead and dying cortex, cambium, and young wood in a small patch, contain the mycelium of the fungus which gives rise to the cup-like fructifications—*Peziza Willkommii*—above referred to (Fig. 30); and Hartig has proved that, if the spores of this *Peziza* are introduced into the cortex of a healthy living larch, the mycelium to which they give rise kills the cells of the cortex and cambium, penetrates into the young wood, and causes the development of a patch which everyone would recognize as that of the larch-disease. It is thus shown that the fungus is the immediate cause of the patch in which it is found.

The next fact which has been established is that the fungus can only infect the cortex through some wound or injury—such as a crack or puncture—and cannot penetrate the sound bark, &c. Once inside, however, the mycelium extends upwards, downwards, sideways, and inwards, killing and destroying all the tissues, and so inducing the outflow of resin which is so characteristic of the disease. The much-branched, septate, colourless

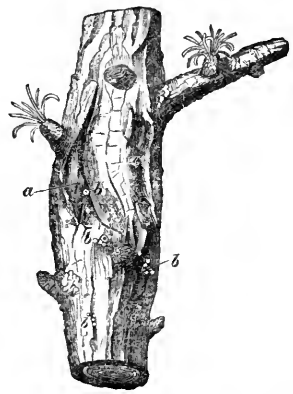


Fig. 29.—Portion of stem of a young larch affected with the larch-disease, as indicated by the dead "cancerous" patch of cracked cortex, *a*: at and near the margins of the patch are the small cup-like fructifications of *Peziza Willkommii* (Htg.), which spring from mycelium in the dead and dying cortex and cambium beneath. (After Hess.)

hyphæ can penetrate even as far as the pith, and the destroyed tissues turn brown and dry up.

After destroying a piece of the tissues in the spring, the growth of the mycelium stops in the summer, the dead cortex dries up and sticks to the wood, and the living cortex at the margins of the patch commence to form a thick layer of cork between its living cells and the diseased area.

It is this cork-formation which gives the appearance of a raised rim around the dead patch. It has long been known that the patches dry up and cease to spread in the dry season. It should be pointed out that it is one of the most general properties of living parenchymatous tissue to form cork-cells at the boundaries of an injury: if a slice is removed from a potato, for instance, the cut surface will be found in a few days with several layers of cork-cells beneath it, and the same occurs at the cut surface of a slip, or a pruned branch,—the "callus" of tissue formed is covered with a layer of cork.

If it is remembered that the cambium and young wood are destroyed beneath the patch, it will be at once clear that in succeeding periods of growth the annual rings of wood will be deficient beneath the patch.

Next year, the cambium in the healthy parts of the stem begins to form another ring; but the fungus mycelium awakens to renewed activity at the same time, and spreads a little further upwards, downwards, and sideways, its hyphæ avoiding the cork-layer and traversing the young wood and cambium below. During this second spring, therefore, a still larger patch of dead tissue—cortex, cambium, and young wood—is formed, and the usual cork-layer describes a larger boundary. Moreover, since the cambium around the, as yet, undiseased parts has added a further annual ring—which of course stops at the boundaries of the diseased patch—the centre of the patch is yet more depressed (cf. Fig. 28).

And so matters go on, year after year, the local injury to the timber increasing, and ultimately seriously affecting, or even bringing to an end, the life of the tree.

At the margins of the diseased patches, as said, the fungus at length sends out its fructifications. These appear at first as very minute cushions of mycelium, from which the cup-like bodies with an orange-coloured lining

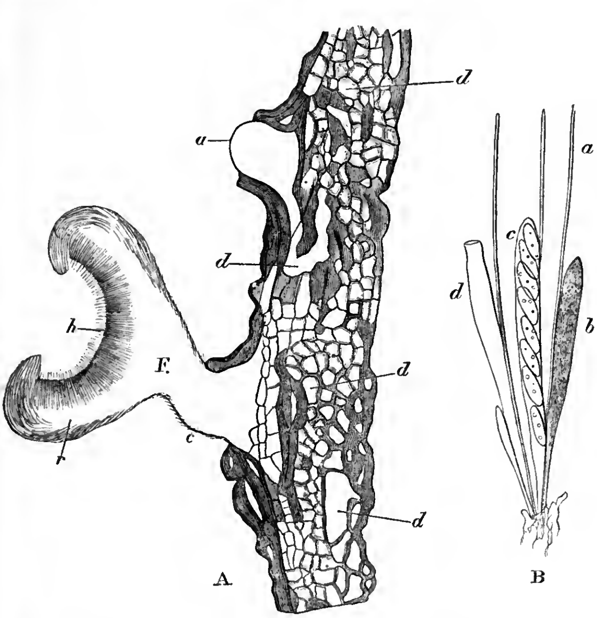


FIG. 30.—A, vertical section (magnified) through the dead cortex of a larch, infected with the mycelium (*d*) of *Peziza Willkommii* (Htg.), which is developing its fructifications (*a* and *E*). The mycelium fills up the gaps in the cortex, *d*, with a white felt-work. *a* is a boss-like cushion of this felt-work bursting forth to become a cup-like fructification; *F*, the mature *Peziza* fructification (in section); *c*, its stalk; *r*, the margins of the cup; *h*, the layer of spore-sacs (*asci*). B, four of the asci from *h*, very highly magnified. *a*, hair-like barren filaments between the asci; *c*, a fully-developed ascus, containing the eight spores; *d*, an ascus emptied of spores (they have escaped through the hole at the apex); *b*, a young ascus in which the spores are not yet formed: to the left below is a small one still younger. (After Hartig and Willkomm.)

arise: the structure of this fructification is best seen from the illustration (Fig. 30, A). The orange-red lining (*h*) is really composed of innumerable minute tubular sacs, each of which is termed an *ascus*, and contains eight small spores: as seen in the figure (Fig. 30, B), these asci stand upright like the pile of velvet lining the cup. They are formed in enormous numbers, and go on ripening and scattering the spores day after day. There are many interesting details connected with the development and structure of these fructifications and spores; but we may pass over these particulars here, the chief point for the moment being that very large numbers of the minute spores are formed, and scattered by the wind, rain, animals, &c. Moreover, as already stated, it has been shown by experiments that the spores will infect the stem of the larch if they are introduced into a wound; but it is important to notice that the fungus cannot penetrate the sound cortex.

It now remains for us to see if, in the natural course of events, infection of the larch can take place to any great extent; for, unless this is the case, we cannot reconcile the above peculiarities of the fungus with the prevalence of the disease.

It must be borne in mind that the larch is an Alpine tree, growing naturally at an elevation of from about 3000 to 6000 feet above sea-level, and even more. In its native heights, both the larch-disease and *Peziza Willkommii* occur associated as we have described them, but the malady does not become epidemic, as it has done in the valleys and plains of Europe.

Several insect-enemies of the larch are known, some of which feed on the buds, and others on the leaves, &c.: it is not impossible that insect-wounds may serve occasionally as points of entry for the fungus.

But attention should be directed to the remark made when describing the symptoms of the disease—namely, that a dead branch often springs from near the centre of the patch. Now it is a well-known fact in the hill-forests of Switzerland, Germany, Austria, &c., that heavy falls of snow often load the branches until they bend down to the ground, and the bark in the upper angle where the branch joins the stem is ruptured; similar cracks are also caused by the bending down of the branches under the weight of water condensed from mists, &c. If a spore alighted near such a place, the rain would wash it into the crevice, and it would germinate in the moisture always apt to accumulate there. This certainly accounts very completely for the situation of the dead branch, which of course would at once suffer from the mycelium. Another way in which such wounds as would give access to the parasite might arise, is from the blows of hailstones on the still young and tender cortex.

But probably the most common source of the crevices or wounds by which the fungus gains an entry is frost; and to understand this we must say a few words as to what is known of the larch at home in its native Alps.

It is well known, since Hartig drew attention to the fact, that in the high regions of the Alps the trees begin to put forth their shoots very late: the larch in the lowlands of Germany and the British Isles often begins to shoot at the end of March or beginning of April, whereas in the mountains it may be devoid of leaves in May. This is because the transition from winter to spring is very sudden on high slopes, whereas in the lowlands and valleys it may be very gradual. The consequence is that in the Alps, when the buds once begin to open they do this rapidly and vigorously, and the tender leaves and shoots are quickly formed and beyond the reach of those late spring frosts which do so much damage in our country: in the lowlands, on the contrary, the leaves slowly develop at a time when late frosts are very apt to recur at night, and they are for several weeks exposed to this danger; and if a sharp frost does come, the chances are that not only will the first output of tender leaves be killed off, but the whole shoot suffers, and frost-wounds are formed in the young cortex.

Another point comes into consideration also. In warm damp valleys the whole tree is apt to be more watery, and it is well known that the soft tissues, like the cortex, suffer more from frost when filled with watery sap, than do harder, drier, more matured ones. It has been shown, according to Sorauer, that dead patches, exactly like those which characterize the larch-disease in its early stages, can be artificially produced by exposing the stem to temperatures below zero, so as to freeze the water in the cells.

Given the above conditions for producing frost-wounds, then, and the presence of spores of *Peziza Willkommii*, there is no difficulty in explaining the well-known phenomena of the larch-disease.

But Hartig has brought to light some other facts of great importance in considering this admittedly complex question. We have already stated that the *Peziza* does occur at the margins of the wounds in the Alps where the larch is native. In these higher regions, however, the air is usually dry during periods of active growth and the young fructifications of the fungus are particularly sensitive to drought; consequently, even when many scattered trees are infected, the cups developed at the edges of the wounds are apt either to dry up altogether, or to produce relatively few spores, and these spores have fewer chances of germinating. In fact, the fungus enjoys at best a sporadic existence, chiefly at the bases of trees where the herbage affords a certain degree of dampness.

When the larch was brought down to the plains and valleys, however, and planted in all directions over large areas, the *Peziza* was also brought with it; but it will be clear from the foregoing discussion that the climatic conditions were now proportionally raised in favour of the fungus, and lowered to the disadvantage of the larch. Plantations in damp valleys, or in the neighbourhood of the sea, or of large lakes, were especially calculated to suffer from frost, and the damp air favoured the propagation of the fungus, and the disease tended to become epidemic. The enormous traffic in larch plants also shows how man too did his share in spreading the epidemic; and in fact the whole story of the larch-disease is of peculiar interest biologically, as illustrating the risks we run every day in trusting to the chapter of accidents to see us safely through any planting undertaking, no matter how great the stake at issue, or how ruthless the interference with those complex biological and physical conditions which always play such an important part in keeping the balance in the struggle for existence between all organisms living together.

Let us now very shortly see what are the chief lessons taught us by the bitter and costly experience which the larch-disease brought to foresters. It is evident that the larch should not be planted at all in low-lying situations exposed to late frosts; and even in more favoured valleys experience points to the advantage of mixing it with other trees: large areas of pure larch are planted at enormous risk in the lowlands.

As to the treatment of trees already diseased, it is possible (when it is worth while) to remove diseased branches from trees of which the trunk and crown are healthy, but it hardly needs mention that such diseased branches must be burnt at once. As regards trees with the stems diseased—in those cases where the patches are large, and much resin is flowing from the wounds, experience points to the advisability of cutting them down. In those cases where the tree is already very large, and the diseased wound but small, it may be expedient to let them alone: theoretically they ought to go, or at any rate the diseased tissues be excised and burnt; but it seems to be proved that such a tree may go on forming timber for many years before the wound will spread far enough to reduce the annual increment below the limits of profit, and we all know the view a practical forester will take of such a case. At the same time, it is the duty of the man of science to point out that even such a tree is a possible source of danger to its neighbours.

H. MARSHALL WARD.

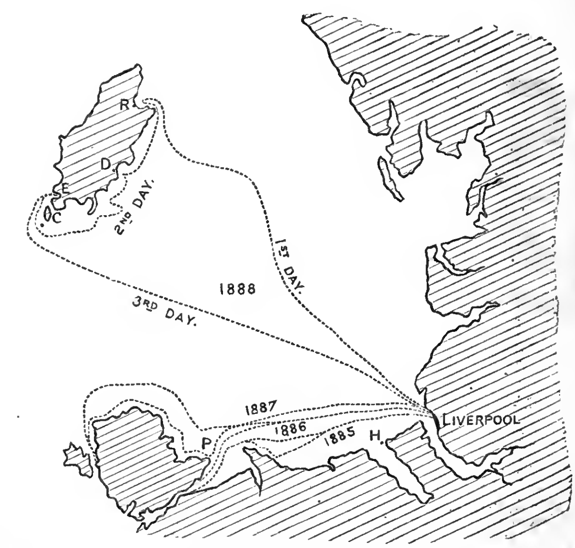
(*To be continued.*)

MARINE BIOLOGY AND THE ELECTRIC LIGHT.

THE Liverpool Salvage Association, with their usual liberality, placed their famous old steamer the *Hyæna* once more at the service of the Liverpool Marine Biology Committee this Whitsuntide, for a three days' dredging expedition. During the three former biological cruises of the *Hyæna* in 1885, 1886, and 1887, the region explored has been the southern part of the L.M.B.C. district, around the coasts of North Wales and Anglesey (see Fig.).

On the present occasion the Committee decided to run a couple of lines of soundings and dredgings between the Mersey and the Isle of Man, and to spend some time dredging round the southern end of that island; the general objects being (1) to get some knowledge of the depths, bottom, and animals, across the eastern half of the Irish Sea, and (2) to investigate the rich fauna living around the "Calf" and south end of the Isle of Man.

About 7 a.m. on Saturday morning, May 19, the *Hyæna* left the Liverpool landing-stage, with a party of nearly twenty biologists on board, and provided with dredges, trawls, tow-nets, sounding-line, deep-sea reversing thermometer, microscopes, and the other necessary instruments, dishes, bottles, and reagents. After the well-known sand-banks round the mouth of the Mersey had been passed, soundings and bottom temperatures were taken occasionally, and several times during the day a stop was made for trawling, dredging, and tow-netting. A fair amount of material, including some interesting larval forms, was obtained, and for the most part preserved for further examination. No greater depth than 23 fathoms



Map of the L.M.B.C. District, showing the course of the *Hyæna* in 1885, 1886, 1887, and 1888. H, Hilbre Island; P, Puffin Island; R, Ramsey: D, Douglas; E, Port Erin; C, the Calf.

was, however, met with; and there was nothing specially noteworthy amongst the animals dredged, so far as could be seen at the time.

It had been intended to anchor for the night in Douglas Bay, but during the dredging and trawling the vessel had drifted so far out of her course that when evening came it was found advisable to run for Ramsey. Here half the party went on shore for the night, the rest staying on board for the electric light experiments which will be described further on.

On the following morning an early start for the south was made, and the rest of the party was picked up at Douglas, and then the work of the day commenced. The *Hyæna* steamed slowly round the east and south coasts of the island to Port Erin, dredging and tow-netting at intervals, with very good results. When a stop was made for collecting, the fullest advantage was taken of it. The sounding-line and deep-sea thermometer were over amidships, and two dredges, a large bottom tow-net and one or more surface tow-nets, were put out astern. The deep tow-net, devised and worked by Mr. W. S. McMillan, was so weighted and buoyed as to work steadily at a distance of a foot or so above the sea-bottom, and it yielded a large amount of material, which was in some cases conspicuously different from the contents of the surface nets, worked by Mr. I. C. Thompson during the same time.

A large area of the sea bottom between Port Soderic and Port St. Mary is apparently covered by masses of *Melobesia calcarea* and the dead valves of *Pectunculus glycimeris*, and incrusting Polyzoa are especially abundant upon both the Nullipore and the shells. Mr. J. Lomas, who has charge of the Polyzoa, informs me that amongst a number of other rare forms he has identified *Stomatopora johnstoni* and *S. incrassata*, *Tubulipora lobulata*, *Lichenopora hispida*, *Cellepora dichotoma*, *Membranipora aurita*, and a peculiar variety of *Cellaria fistulosa*.

Towards evening three very successful hauls of the dredge were made, which covered practically all the ground in a line from the southern end of the "Calf" to the northern side of Port Erin Bay, just under Bradda Head. Amongst the material obtained in these hauls the following species were noticed: *Asterias glacialis*, *Solaster endeca*, *Stichaster roseus*, *Porania pulvillus*, *Luidia fragilissima*, *Antedon rosaceus*, *Ebalia* sp., *Xantho* sp., *Pleurobranchus membranaceus*, *Ascidia venosa*, *Ascidia plebeia*, *Corella parallelogramma*, *Polycarpa* sp., *Leptoclinum* sp., and other Compound Ascidians.

In Port Erin Bay after dark the electric light was again used successfully in the bottom and surface tow-nets.

On the third day an early start was again made, with the object of leaving time to run down into the deep water lying to the south of the Isle of Man. Unfortunately, however, a thick fog was encountered, which hampered our movements during the morning and changed all the plans for the day. After passing the "Chicken" Rock, the *Hyæna* steamed slowly for Liverpool, and reached the Mersey about 1 a.m. on Tuesday. A few hauls of the trawl and dredge were taken on the way home, with no great results, and the tow-nets, both bottom and surface, were worked whenever practicable.

The important feature of this cruise, however, was the use which was made of the electric light for collecting after dark. On the first night, in Ramsey Bay, after the shore party had left and the ship was anchored for the night, an electric light of 1000 candle-power was hoisted a few feet above deck, and this allowed work to be carried on almost as comfortably as during the day. Captain Young, of the Liverpool Salvage Association, who was in command of the *Hyæna*, then kindly arranged for me a 60 candle power Edison-Swan submarine incandescent lamp in the mouth of a tow-net. This illuminated net was carefully let down to a depth of 3 fathoms, and allowed to remain there for half an hour. At the same time, another tow-net without any light was let down to the same depth over the opposite side of the ship. When the nets were being hauled in, as the one with the electric light approached the surface numerous small animals (Crustacea probably) were noticed accompanying it, and darting about in the bright light. This tow net, when emptied into a glass jar of sea-water, was found to contain an abundant gathering, consisting mainly of Crustaceans; while the net in the dark on the other side of the ship had practically nothing.

The two nets were then put out again. The one had the electric light in its former position, but this time it was let down to the bottom at a depth of 6 fathoms; while the other net was placed in the dark at the ship's stern, and also reached the bottom. The tow-nets remained stationary, but were kept distended by the tide. The outline of the illuminated net could be made out indistinctly at a depth of 6 fathoms. After being out for three-quarters of an hour, both nets were hauled in, with the same result as before. The illuminated net contained abundance of Crustacea (chiefly Amphipoda, Schizopoda, and Cumacea), while the dark net again contained practically nothing. These two experiments showed pretty conclusively the effect of the brilliant light in attracting the free-swimming animals, the difference between the contents of the two nets being on both occasions most marked. Consequently, on the second night, in Port Erin Bay, *both* nets were illuminated, and while the one was let down close to the bottom, at a depth of 5 fathoms, the other was kept at the surface of the sea on the opposite side of the ship. This experiment was tried three times, with the same result each time: both the nets were found to contain abundance of animals, but the bottom and surface gatherings differed greatly in appearance and in constitution. The net from the bottom contained mainly large Amphipoda, and some Cumacea, while the gathering from the surface was characterized by the abundance of Copepoda. As Mr. A. O. Walker, who is reporting upon our higher Crustacea, pointed out to me, the Amphipods from the deep net appeared to be chiefly red-eyed species, such as *Ampelisca lævigata* and *Bathyporeia pilosa*. If this, on a detailed examination of the material, turns out to be the case, it may indicate an interesting relation between the colour of the eyes and sensitiveness to the electric light.

Mr. Thompson has already identified the following species of Copepoda from the illuminated surface net: *Calanus finmarchicus*, *Pseudocalanus elongatus*, *Dias longiremis*, *Idya furcata*, *Centropages hamatus*, *Anomalocera patersonii*, *Isias clavipes*, *Oithona spinifrons*, *Harpacticus chelifer*, and *Harpacticus fulvus*. The specimens of the last two species are remarkable for their unusually large size and their abundance.

The various groups of animals collected will as usual be worked up in detail by specialists, and the results will appear in future L.M.B.C. Reports; but the application of the electric light to marine biology, as a bait or attraction in the tow-net worked after dark, seems of sufficient importance to warrant the publication of this preliminary account of the results of the *Hyæna* cruise of Whitsuntide 1888. The obvious extension of this illumination method to deep-water tow-netting and trawling during the day-time I hope, thanks to the kindness of the Salvage Association, to be able to experiment upon in a future expedition.

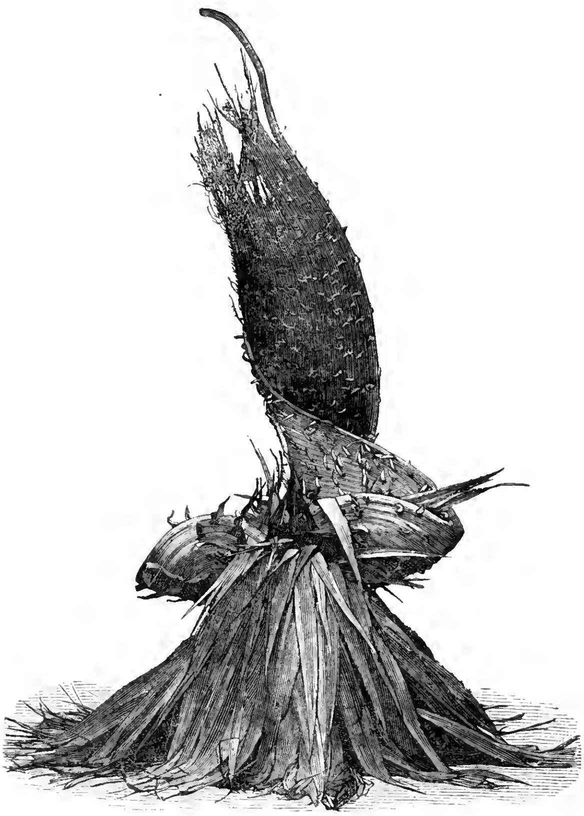
W. A. HERDMAN.

A REMARKABLE CASE OF FASCIATION IN FOURCROYA CUBENIS, *HAW*

THERE was lately exhibited in this city a plant of *Fourcroya cubensis*, Haw., in which the well-known, tree-like inflorescence had been deformed into what I believe to be the largest fasciation on record. The plant came from Carapa, a small village distant about 4 miles towards the west from Caracas. Its aspect is given in the accompanying figure, engraved after a photograph.

The stem of the plant, covered by the leaves, is about 1 metre in height. From between the upper leaves there branch out two flattened and curiously twisted bodies. The one to the left was soon checked in its growth, so that it forms but little more than a semicircle; whilst the other, after having described a curve somewhat like a very large capital S, rises to a height of about 4 metres from the soil. Both together have in the front view the appearance of a small boat with hoisted sail filled by the wind. The under and lower parts of this deformed flower-stem are covered by numerous bracts, and measure 80 centimetres in their greatest breadth. Towards the top it divides into shred-like branches bearing flower-buds; those of the latter I examined being in every respect of normal structure.

There can be little doubt that, in this case, the malformation is due to some injury done to the young flower-stem, when it was scarcely 1 foot high, vestiges being still visible that it was bent towards the right and kept in this forced position by some of the leaves. The upward growth being thus checked, numerous adventitious buds made their appearance on the injured organ, coalesced from the very outset, and formed by their subsequent growth the fasciated stem, the twisting resulting from the unequal rate of development of its component parts (Masters, "Veget. Teratology," 18).



Fasciation is likely to be not at all uncommon in *Fourcroya* and other allied plants, though I know of but three cases in the former, and never heard of any in *Agave*. In 1854 a very curious case of this kind was for several months the cause of considerable excitement among the good people of Caracas; it is described in the newspapers of the time as having been likewise twisted in the shape of a gigantic S. Another instance came under my notice in 1876, and was described in the *Journal of Botany* of that year, p. 180.

Caracas, April 19. A. ERNST.

NOTES.

THE following were elected Foreign Members of the Royal Society, on Thursday, May 31: Prof. Edmond Becquerel, of Paris, distinguished for his researches on the effects of light on bodies, especially with reference to phosphorescence; Prof. Hermann Kopp, of Heidelberg, for his researches on atomic volumes and boiling-points; Prof. Eduard F. W. Pflüger, of Bonn, for his researches in physiology, especially in relation to irritability of nerves, respiration, and animal heat; and Prof. Julius Sachs, of Würzburg, for his researches in botany, especially vegetable physiology.

THE Board of Visitors made their annual inspection of the Royal Observatory at Greenwich on Saturday last.

THE Vienna Correspondent of the *Times* announces that, in pursuance of a resolution passed at a recent meeting, the Vienna geologists will invite the International Geologists' Congress, which will assemble in London in September, to hold its next meeting in Vienna.

AT a recent meeting of the Victoria Royal Society, the President (Prof. Kerrot) announced that the first meeting of the Australian Association for the Advancement of Science would be held at Sydney, beginning September 4, the second at Melbourne, the third at Adelaide. The proposal that Victoria should join in the movement was favourably received, but at that meeting no action was taken in the matter.

IT will be seen from our list of the additions to the Zoological Society's Gardens during the past week that a living specimen of Pallas's sand grouse (*Syrrhaptes paradoxus*), the new visitor from Central Asia, has been presented by Mr. H. Hewart Crane, of Berwick-on-Tweed. It was captured at that place on May 25.

THE Tartar sand grouse seems to have appeared in Denmark and Scandinavia before making its appearance here. In the Island of Bornholm, in the Baltic, large flocks, numbering many hundreds, were seen early in May, some being shot, others captured alive. A few days later, birds were seen in various parts of Denmark and Sweden. In Norway a flock of birds was seen at Lister, on the extreme west coast, on May 12, and two were shot, a male and female. Their crops were full of tiny black seeds unknown to that country, whilst the eggs in the hen were far developed. During the immigration in 1863 these birds were seen as far north as Nordfjord. In that year, too, many nested on the west coast of Jutland, where the soil is sandy, but they were all gathered by the fishermen.

PROF. A. GRAHAM BELL, who is now on his way to England, will shortly appear before the Royal Commission engaged in making inquiry as to the best methods of caring for and educating deaf-mutes. In announcing this fact, *Science* reminds its readers that several years ago Prof. Bell presented a paper, at a meeting of the National Academy of Sciences, on the formation, through the intermarriage of deaf-mutes, of a deaf variety of the human race, and gave some important statistics to show that a much larger percentage of the children of deaf parents are deaf than of those whose parents possess the sense of hearing. This paper attracted wide attention, and gave rise to very interesting discussions both in America and elsewhere. The Royal Commission has requested Prof. Bell to lay before it the results of his subsequent investigations and studies upon this branch of the subject, and he has devoted much time to the preparation of facts and figures in regard to it. He will also give the Commission the result of his studies of other divisions of the subject.

ACCORDING to *Allen's Indian Mail*, Mr. Barrington Browne, the geologist sent by the Secretary of State to examine the Burma Ruby Mines, has left Simla for England. He has, it is understood, handed in to the Government of India his report on the mineral wealth of Upper Burma.

THE hydrographic survey of Canadian waters, which has already taken about five years, is now nearly half done. Commander Boulton is hard at work in Georgian Bay, one of the most dangerous of inland waters in Canada, and it is said that the survey will be extended to Lake Superior.

FROM September 15 to October 25 there will be in Vienna an International Exhibition of Amateurs' Photographs and Photographic Apparatus. The Exhibition is being organized by the Vienna Club of Amateur Photographers, and will be held in honour of what is called "the Jubilee" of the Emperor Francis Joseph. It will include every branch of art and manufacture connected with photography. The Club's Daguerre Medal and certificates of honourable mention will be awarded to the best exhibit or exhibits in each class of photography, photographic apparatus, lenses, &c., provided the jury deem any exhibit or exhibits of sufficient merit. From the decision of the jury there will be no appeal. The Club, as far as its funds permit, will purchase the most interesting exhibits. Amateurs have not to pay hire for the space allotted to them. On application they can obtain the use of frames free of charge. A catalogue will be published, possibly with illustrations of the most interesting objects. According to the statutes of the I. and R. Austrian Museum for Arts and Manufactures, admission will be free five days a week.

THE current number of the *Board of Trade Journal* contains an abstract of the third volume of the Reports of the Royal Commission appointed by the King of the Belgians in April 1886 to inquire into the condition of labour in Belgium. The volume contains the propositions of the various sections of the Commission with respect to the different questions relating to the condition of the working classes, and also the final conclusions of the whole Commission. The third section of the Commission dealt with technical education, and the conclusions adopted by the whole Commission are as follow :—(1) They recommend that in the technical schools practical lectures be given on the application of art and science to industry. (2) Manual dexterity should be cultivated in the elementary schools. At the industrial schools the theoretical application of science to industry should be taught. (3) The Government should limit its action to providing grants for these schools, and fixing the position each school is to occupy in a proper gradation of educational institutions. (4) The local bodies should introduce manual exercises into the primary schools, and found more technical schools and schools of design and modelling. (5) The aid of the Government and the communes should be given conditionally on a minimum age being fixed for apprentices, and on a test examination at entrance being made necessary. (6) The Government should aid in increasing the facilities by which workmen would get technical instruction in subjects suited to their occupation.

M. COUMBARY, Director of the Imperial Meteorological Observatory at Constantinople, has published a pamphlet upon the climatology of that place, deduced from twenty years' observations (1868–87). Hitherto, what has been known about its climate is mostly owing to observations and summaries contained in the periodicals of the French Meteorological Society, commencing with the year 1847, and to the telegraphic reports in the French *Bulletin International*. M. Coumbary issued a monthly Bulletin in 1869, containing observations made at several places in the Ottoman Empire, but this was discontinued in 1874. The present discussion shows that the mean temperature is 57°·7. The absolute maximum was 99°·1 in August 1880, and the minimum 17°·2 in January 1869, giving a range of 82°. The French observations show greater extremes, but this is probably owing to imperfect protection from radiation in earlier years. The greatest daily ranges were 37°·8 in December, and 36° in March ; in other months the range has not exceeded 27°. The extremes are of course modified by the influence of the Black Sea ; it is not unusual for the thermometer at Odessa, for instance, to indicate 24° or so below the temperature at Constantinople. The mean annual rainfall is 28 inches, and the days of rain average 84. Snow falls on 14 days, on an average. About three years ago the Sultan showed his interest in the subject by the establishment of a second observatory in his palace at Yildiz. Both institutions are furnished with the best instruments.

WE have received the twelfth Annual Report on the Meteorology of India, containing the observations taken in 1886. It deals with nearly the same area as last year, and is published in the form previously adopted. For fullness and thoroughness in the discussion of results, it remains unexcelled, and it includes, as before, monthly charts showing very clearly the mean pressure and temperature, and the resultant winds over the vast region embraced in the Report. Among the more important additions are an observatory at Mandalay, where the transitions of the seasons are said to be sudden, and earthquakes not infrequent, and a station on the Great Coco Island, in the Bay of Bengal, an island which is said to be destitute of drinking-water. The results show that in every month of the year 1886 the mean "equilibrium" temperature of insolation throughout India was below the average of the last ten or eleven years by amounts varying from 0°·8 to 1°·8. The annual variations for the past seven years show a fairly well-marked periodicity, and suggest a slight variation in the sun's radiating power. The rainfall is represented by 500 stations (14 more than in the previous Report), and was characterized by several striking features. On the mean of the whole area there was an excess of 2·77 inches as compared with the averages for previous years.

THREE important new chlorine compounds of titanium have been obtained by Drs. Koenig and von der Pfordten, of Munich. They may be considered as chlorine derivatives of titanic acid, $Ti(OH)_4$, and form the only complete series of such compounds with which we are as yet acquainted in the whole range of inorganic chemistry. They are formed by the replacement of the hydroxyl groups by chlorine, and have therefore the following constitutions: $TiCl(OH)_3$, $TiCl_2(OH)_2$, and $TiCl_3OH$. The well-known tetrachloride of titanium, $TiCl_4$, thus completes the series, and in reality formed the starting-point from which the three intermediate compounds were successively prepared. Trichloride of titanic acid, $TiCl_3OH$, was obtained by the careful addition of concentrated hydrochloric acid to the tetrachloride in such proportion that the amount of water present in the strong acid was that required by the following equation: $TiCl_4 + H_2O = TiCl_3OH + HCl$. The reaction is very violent, and a solid mass of the trichloride was almost instantly formed and considerably distended by the escaping hydrochloric acid gas. The substance was at once transferred to the vacuum of an air-pump, and after a few days was found to be entirely freed from last traces of the gas. The solid trichloride thus formed is extremely deliquescent, and readily dissolves with considerable hissing in water and alcohol, the aqueous solution being remarkably stable. The dichloride, $TiCl_2(OH)_2$, was prepared by addition of a slight excess of strong hydrochloric acid to the tetrachloride, and also by placing the latter compound in a small quantity of ice-cold water. In the latter case, the drops of $TiCl_4$ are at first decomposed with loud hissing, which, as the drops continue to fall, gradually diminishes until a point is reached when a drop floats on the surface and remains unattacked. This last drop is then removed, and the clear solution evaporated *in vacuo*, when the dichloride is left as a compact deliquescent solid. The monochloride, $TiCl(OH)_3$, is the product of the action of moist air upon the tri- and di-chlorides, hydrochloric acid gas being at the same time evolved, in accordance with the following equations :

$$TiCl_3OH + 2H_2O = TiCl(OH)_3 + 2HCl,$$
$$TiCl_2(OH)_2 + H_2O = TiCl(OH)_3 + HCl.$$

The monochloride thus formed remains stable in air ; on evaporation over oil of vitriol it is obtained as a white solid, crystallizing apparently in the hexagonal system, and very difficultly soluble in water. In conclusion, the Munich chemists show very conclusively that these new substances are true compounds and no mere mixtures ; and, it may be added, the analyses, which must of necessity have been extremely difficult, are quite satisfactory.

THE British Consul at Mogador, in Morocco, in his last report notes, in connection with the fisheries of the year, a curious phenomenon. A fish locally called the "tasargelt" (*Temnodon saltator*) has appeared in vast shoals, having left the waters unvisited, save a few stray specimens, since 1859. It weighs from six to eight pounds, and has flesh of rich flavour, of which the natives never seem to tire. It first appeared in large numbers early in September, and from that time till December the fishermen were busily occupied taking them. The mode of capture is rather primitive. A piece of white rag or a strip of the skin of the tasargelt itself is fastened to a large and often barbless hook, which in turn is tied by strong brass wire to the end of a short bamboo rod. When the bait is drawn rapidly through the water, the fish rises quickly to it. The tasargelt was accompanied by shoals of the "azlimzah" or "maigre," a fish which frequently weighs as much as sixty or seventy pounds. The presence of these voracious fish ruined the ordinary hook-and-line industry. Though shoals of bonito appeared, only one small specimen was taken, for they refused to take any bait. The sardine fishery was also a failure.

THE British Consul at Varna, in the course of his Report on the trade of his district for the past year, refers to the vineyards, and says that, though the Phylloxera has not made its appearance in these regions, there is a kind of insect pest which he believes to be peculiar to the Varna vineyards. Its ravages have been confined to certain areas, and the vine it attacks is disabled only for the year of the attack, and only to the extent of the particular shoots which it may lop off. The local name of the insect is *Kara terzi*, or "the black tailor," an appellation which is supposed to indicate its appearance and habits. In the absence of local entomologists, Mr. Brophy describes this new pest as an adipose black beetle, somewhat resembling the ordinary dung-beetle, measuring, when adult, about three-fourths of an inch in body-length, and furnished with a short pair of shears; with these, in the mornings of April and May, it cuts through and off the young vine-shoots, which it leaves on the ground until they are parched by the sun, when it drags them into the recesses of its deep and tunnelled hole, generally situated at the foot of the plant attacked. The vineyards chiefly affected are situated on ground near the sea-shore, whence the insect makes its way inland; "and as the *Kara terzi* does not appear to have obtruded itself upon the notice of the vine-growers by its obnoxious habits until comparatively recently, it may perhaps be fair to suppose that the temptation of green and succulent vine-shoots may, in the course of generations, have perverted the present race into abandoning the more innocent diet which satisfied their ancestors, and which, when the vine-shoots have passed the tender stage, has still to suffice those of the present day." Mr. Brophy says that if the circumstances of insect-life here related prove in any way new or interesting, it would not be difficult to procure, in summer, specimens of this beetle for inspection by qualified entomologists.

AT two successive meetings of the Oriental Society of Pekin, Prof. Russell, of the Tung-Wen-Kwan, or Foreign Language College, read two papers on subjects connected with Chinese astronomy. In the first he described the instruments in the Pekin Observatory, which were constructed by the order of the great Emperor Kanghi about 1670. In the course of the discussion which followed, it was stated that this prince was very fond of mathematics and astronomy, and that the present Emperor was credited with similar inclinations. Kanghi was sixteen when he ordered the instruments to be constructed. The clepsydra used in the Observatory, it was stated, consisted of five cisterns, and was used for observing the time of eclipses, being put in order for this purpose three days before each eclipse. One of the instruments is usually said to be of European design and to have been presented by Louis XIV. The inscription or emblem on it has been carefully removed, and its place supplied by a piece of bronze matching the metal of the instrument. In some Chinese books it is said that this instrument was manfactured by a foreign priest. Verbiest, a Jesuit of the time of Kanghi, pointed out a mistake in the Chinese calendar; the matter was referred by the Emperor to the Board of Astronomy, and Verbiest's accuracy was acknowledged. From that time a Jesuit missionary occupied the post of Vice-President of the Board down to 1828.

THE second paper, also by Prof. Russell, was on early Chinese eclipse calculations, and entailed vast labour in re-calculating. It appears from the investigations of the learned Professor that the earliest calculations of a solar eclipse and also of a lunar eclipse which have been preserved were made by the Chinese. The discussion turned largely on the historical value of the Chinese classics with regard to these astronomical observations, and the attention with which the Chinese from the earliest times have studied astronomy. Passages found in one or other of the few works which survived the destruction of the books before the Christian era bear witness to the devotion with which the stars were studied in China at that remote epoch. The full text of these two interesting papers will be awaited with interest.

AT a recent meeting of the Scientific Society of Upsala, Dr. C. Aurivillius read a paper on the skeleton of the so-called Swedenborg whale (*Eubalena svedenborgii*, Lillj.), discovered last November in the province of Halland, in a layer of marl 50 feet above the sea. Remains of this species of whale have only been found once before, viz. early last century, when some parts of one were discovered in the province of Western Gothland, 330 feet above the sea, and 70 miles inland. It was at first believed that they were the bones of some giant, but it is said that Swedenborg discovered their true nature. The skeleton has been presented to the Upsala Museum.

IN the Proceedings of the Moscow Archæological Society, there is a most interesting communication by M. Anutchin, on the use of sledges, boats, and horses, or saddles, at the burials of various races. He shows that until the seventeenth century the Slavonians used sledges even in summer for the transport of the corpse to the grave. The Samoyedes and Ostyaks, and many Russian peasants of Northern Russia, still follow this custom. The boat was used by the Normans, the Old Germans, and generally by races inhabiting the shores of lakes. Many tribes of North America used to bury their dead together with a horse, or transported the dead to the grave on a horse. It is remarkable that the same custom is found among the Lithuanians, who, even in the sixteenth century, put their dead on a saddle. The sledge, the boat, and the horse, or saddle, were obviously intended to aid the dead in passing into another world, and in visiting kinsfolk there.

AN ancient canoe has been found in the Tunhövd Fjord, in Valders, in South Central Norway. It has been hollowed out by means of red-hot stones, and is $4\frac{1}{2}$ metres long and 80 centimetres broad. It is in fair condition. The find is of interest, as no other primitive vessel of the kind has been found inland in Norway. The boat will be sent to the Museum at Christiania.

Science says that a citizen of the United States, who has long resided abroad, proposes to give to the Smithsonian Institution a large collection of armour from the Middle Ages—some of it connected with most famous historical names—including horse-armour, helmets, swords, and all the paraphernalia of ancient warfare. These objects, numbering about five thousand, have been brought together at great expense, and the collection is one of the most valuable of the kind in the world. The condition of the presentation is that the Smithsonian Institution shall furnish a fire-proof building for the collection.

At the last meeting of the Ceylon Branch of the Royal Asiatic Society, a lengthy paper was read by Mr. P. Ramanathan, the leading Tamil of Colombo, on the ethnology of the Moors of Ceylon. These Moors, or Moormen, are usually classified in the island as a race by themselves, apart from the Tamils, Singhalese, and other races inhabiting it, but Mr. Ramanathan came to the conclusion that the history, social customs, physical features, and language of the Moors, class them as Tamils who were converted to Mohammedanism in India before their migration to Ceylon. He does not think there is any difference between the two classes of "Ceylon Moors" and "Coast Moors" in race or in the history of their conversion, the difference drawn by the members of these classes between themselves being due to a break in the course of immigration from India caused by the persecution of Mohammedans by the Dutch when the latter had possession of Ceylon. He pointed out that it was impossible that the very large number of Moors now existing in India and Ceylon could be, as is popularly supposed, descendants of the small bands of Arab and Moorish merchants and refugees who visited India in early times. He thought that only about 5 per cent. of the existing Moors could owe their origin to these immigrants. The paper, which was a very long and exhaustive one, evidently could not be fully appreciated by those who merely heard it read; but in the subsequent discussion most of the speakers appeared to think that Mr. Ramanathan's conclusion was not satisfactorily established. It was argued that in several directions—especially in regard to the shapes of the skulls—the facts were insufficient, and that at best Mr. Ramanathan's evidence for his thesis was only secondary. The value of the paper as a starting-point for further investigation was generally acknowledged.

The *Comptes rendus* of the French Academy of Sciences for May 14, publishes some interesting remarks on the vital statistics of Germany, by M. Ch. Grad, author of a work on the power and resources of the German people. The population of the empire increased from 40,816,000 in 1870 to 46,855,000 in 1885; that is, an increase of over 6,000,000 in fifteen years, or at the rate of 1 per cent. per annum. Compared with this the increase in France has been extremely slow, less than 5,000,000 for the period of fifty years between 1831 and 1881 (32,560,000 and 37,321,000 respectively), or at the rate of only 0·3 per cent. per annum, with a constant tendency to diminish. During the last fifteen years the excess of births over deaths has been seven times greater in Germany than in France. The contrast becomes greater when it is added that, while few Frenchmen emigrate, as many as 4,000,000 Germans have removed to the United States since 1820. In 1880, the population of the empire included 2,860,000 of Polish speech, 300,000 of French, 150,000 of Danish, 150,000 of Lettish, 137,000 of Wendish, and 34,000 of Checkish or Bohemian. But on the other hand there are at present in Europe over 60,000,000 of Germanic speech, if the 8,000,000 Dutch and Flemish speaking inhabitants of the Low Countries be included. Altogether, the Teutonic nationality has doubled in Europe since 1840. But the increase has been almost entirely in the urban population, which advanced from 14,790,000 in 1871 to 18,720,000 in 1880, while that of the rural districts remained almost stationary (26,219,000 and 26,513,000 respectively). For the whole empire the density of the population is about 86 per square kilometre as compared with 72 in France.

Some figures with reference to alcoholism and criminality were recently communicated to the French Academy of Medicine by M. Marambat. They referred to an examination of 3000 condemned persons; and it appears that 79 per cent. of the vagabonds and mendicants were drunkards, 50 to 57 per cent. of assassins and incendiaries, 53 per cent. of persons convicted of outrages on morals, 71 per cent. of thieves, sharpers, &c. In acts of violence against the person, 88 per cent. were found to be drunkards; against property, 77 per cent. Among youths under twenty, drunkards were nearly as numerous as among adults, the difference being only 10 per cent. Of these youths, 64 per cent. were addicted to drinking. An examination of the departments showed the largest number of drunkards from the regions where spirits are most largely consumed.

A fifth edition of the late Prof. Balfour Stewart's "Elementary Treatise on Heat" (Clarendon Press) has just been issued. Prof. Tait undertook to read the proofs, but found that there was little for him to do. "Prof. Balfour Stewart had himself," he says, "given *imprimatur* to all but the last six sheets; and for these I was furnished with 'copy' (excepting four pages) fully revised and initialed by him. The book is published, therefore, precisely in the form in which its author intended it to appear."

The February and May numbers of the Journal of the Anthropological Institute are of more than usual interest. Among the contents are the following papers: on an ancient British settlement excavated near Rushmore, Salisbury, by General Pitt-Rivers; on the stature of the older races of England, as estimated from the long bones, by Dr. John Beddoe; the Lower Congo, a sociological study, by Mr. R. C. Phillips; the origin and primitive seat of the Aryans, by Canon Isaac Taylor; the Maori and the Moa, by Mr. E. Tregear; on the shell money of New Britain, by the Rev. Benjamin Danks; on tattooing, by Miss A. W. Buckland; on the evolution of a characteristic pattern on the shafts of arrows from the Solomon Islands, by Mr. Henry Balfour; on the occurrence of stone mortars in the ancient (Pliocene?) river-gravels of Butte County, California, by Mr. Sydney B. J. Skertchly; and the address delivered by Mr. F. Galton, as President, at the anniversary meeting of the Institute.

Messrs. John Wiley and Sons, the American publishers, have in preparation a translation of Rosenbusch's "Microscopical Physiography of Minerals and Rocks," by Joseph P. Iddings, of the United States Geological Survey.

Last week we referred to the edition of Barlow's Tables of Reciprocals issued by Taylor and Walton in 1840. The work has also been issued by E. and F. N. Spon. With reference to our note on this subject, Mr. V. B. Sprague and Mr. George King call attention to the "Table of the Reciprocals of Numbers from 1 to 100,000, with their differences, by which the reciprocals of numbers may be obtained up to 10,000,000, by Lieut.-Colonel W. H. Oakes, A.I.A. London: Charles and Edwin Layton, 150 Fleet Street, 1865." This table gives to seven significant figures the reciprocals of all numbers from 10,000 up to 99,999; and by means of the proportional parts the reciprocals of all numbers up to 10,000,000 may be obtained. Mr. Sprague points out that reciprocals can also be obtained with great facility by the use of Thomas's arithmometer; and this, he thinks, is the most convenient method when the number contains eight digits, and it is desired that the reciprocal should contain the same, or a larger number, of significant figures.

A Russian translation of Prof. Everett's "Units and Physical Constants" has just been published at St. Petersburg. This is the fifth language into which the work has been translated, the other four being Dutch, French, Polish, and German. The German edition was long delayed by the compiling of additional experimental data, and only made its appearance a month ago.

The New York State Museum of Natural History has issued a useful Bulletin (No. 3) on "Building-Stone in the State of New York." The author is Mr. John C. Smock.

THE additions to the Zoological Society's Gardens during the past week include a Pudu Deer (*Pudu humilis* ♀) from Chili, presented by Mr. G. E. Pugh Cook; two —— Squirrels (*Sciurus* ——) from Demerara, presented by Mr. R. Forrester Daly; a Blue and Yellow Macaw (*Ara ararauna*) from South America, presented by Mrs. Alfred Palmer; a Pallas's Sand Grouse (*Syrrhaptes paradoxus*) from Berwick-on-Tweed, presented by Mr. H. Hewart Crane; two Australian Waxbills (*Estrelda temporalis*); seven Spotted-sided Finches (*Amadina lathami*) from Australia, presented by Mr. David S. Hodge; a Nose-crested Iguana (*Iguana rhinolopha*) from St. Lucia, West Indies, presented by Dr. T. Dennehy; a Tent Tortoise (*Testudo tentoria*), a Fisk's Tortoise (*Testudo fiski*) from Cradock, Cape Colony, a Dwarf Chameleon (*Chamæleon pumilus*), a Purplish Gecko (*Phyllodactylus porphyreus*), a Hoary Snake (*Coronella cana*), three Narrow-headed Toads (*Bufo angusticeps*), five Gray's Frogs (*Rana grayi*) from South Africa, presented by the Rev. G. H. R. Fisk; two Tigers (*Felis tigris*) from India, two Puff Adders (*Vipera arietans*) from South Africa, deposited; a Long-billed Butcher Crow (*Barita destructor*) from New Holland, received in exchange; two North African Jackals (*Canis anthus*), born in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JUNE* 10–16.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on June 10

Sun rises, 3h. 46m.; souths, 11h. 59m. 15·5s.; sets, 20h. 13m.: right asc. on meridian, 5h. 16·3m.; decl. 23° 4′ N. Sidereal Time at Sunset, 13h. 31m.

Moon (New on June 9, 17h.) rises, 4h. 35m.; souths, 12h. 40m.; sets, 20h. 49m.: right asc. on meridian, 5h. 57·3m.; decl. 20° 45′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	5 25	13 46	22 7	7 3·0	24 0 N.
Venus.....	3 18	11 22	19 26	4 39·5	21 45 N.
Mars......	14 0	19 34	1 8*	12 52·2	5 45 S.
Jupiter....	18 7	22 29	2 51*	15 48·4	19 3 S.
Saturn....	7 15	15 7	22 59	8 24·9	19 55 N.
Uranus...	13 51	19 31	1 11*	12 49·5	4 35 S.
Neptune..	2 53	10 38	18 23	3 54·9	18 41 N.

* Indicates that the setting is that of the following morning.

June.	h.	
11	21	Mercury in conjunction with and 2° 29′ north of the Moon.
12	2	Mercury at greatest elongation from the Sun 24° east.
13	8	Saturn in conjunction with and 0° 20′ north of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m	
U Cephei	0 52·4	81 16 N.	June 10,	23 55	*m*
			,, 15,	23 35	*m*
S Cassiopeiæ	1 11·4	72 1 N.	,, 16,		*M*
η Geminorum	6 8 1	22 32 N.	,, 15,		*M*
V Geminorum	7 16·9	13 18 N.	,, 11,		*M*
U Monocerotis	7 25·5	9 33 S.	,, 16,		*M*
S Geminorum	7 36·3	23 43 N.	,, 14,		*M*
δ Libræ	14 55·0	8 4 S.	,, 15,	2 54	*m*
U Ophiuchi	17 10·9	1 20 N.	,, 13,	0 34	*m*
W Sagittarii	17 57·9	29 35 S.	,, 12,	2 0	*M*
β Lyræ	18 46·0	33 14 N.	,, 16,	1 0	*m*
η Aquilæ	19 46·8	0 43 N.	,, 12,	21 0	*M*
S Sagittæ	19 50·9	16 20 N.	,, 10,	22 0	*m*
			,, 13,	22 0	*M*
X Cygni	20 39·0	35 11 N.	,, 12,	21 0	*M*
T Vulpeculæ	20 46·7	27 50 N.	,, 10,	22 0	*m*
R Vulpeculæ	20 59·4	23 23 N.	,, 16,		*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near α Vulpeculæ	286	24 N.	Rather slow.
,, α Cephei	316	60 N.	Swift, streaks.
,, β Piscium	345	1 N.	June 11–13. Very swift.

GEOGRAPHICAL NOTES.

MAJOR HOBDAY reports of the operations in Upper Burma that during the season of 1887–88, the whole of the Yaw country has been thoroughly surveyed by surveyors attached to the various columns converging on Gangaw. On the north a connection has been made with the work executed by Colonel Woodthorpe's party last year in the Kubo Valley. A good deal of the geography of the Schwele River and the Mohlaing district has also been obtained. The extent of surveying that has been done by the surveyors who accompanied the column from Bhamo to Mogdung and thence by the Jade Mines and Endawgyi Lake to Katha, on the Irrawaddy, is not yet known, as reports have not yet been received. In the Southern Shan States a party under Lieut. Jackson, R.E., has carried on survey operations in continuation of last year's work from Fort Stedman to Pekon, in the Saga Valley, thence *viâ* Maukme, and Moné to Maing-pan and the Salween River, where the Siamese mission under Mr. Archer was met. Returning to Moné, they carried the survey through Legya and Bansan to Maing-ye. In the Northern Shan States a sub-surveyor has carried our surveys from Thibaw to Namsan, and across the Myit-nge or Namtu River to Theinni, on the Salween, and thence *viâ* Maing-yaw to Manse and Maing-ye, thus effecting a junction with Lieut. Jackson's work. Major Hobday himself has extended the triangulation from Kyan Nyat to Bhamo, of which the position is thus determined, and a basis provided for the surveys in the direction of Mogaung. It is hoped that the triangulation executed by this party will be connected during this season with that of the surveys in Lower Burma. In addition to the work done by members of this department, many reconnaissances have been executed by regimental and other military officers and the results given to Major Hobday for incorporation in his sheets.

WE are glad to notice that Signor Guido Cora's *Cosmos* now appears more regularly and frequently than formerly. The last number contains a detailed account of recent Danish expeditions in Greenland.

THE whole of the new number of the *Deutsche Geographische Blätter* is occupied with the narrative of J. G. Kohl's American studies, the results of journeys made thirty years ago in North America.

THE principal paper in the new part of the *Zeitschrift* of the Berlin Geographical Society is an elaborate examination of Sir John Mandeville's writings by Dr. A. Bovenschen, in which the author comes to conclusions decidedly unfavourable to Sir John's trustworthiness. Dr. G. Hellmann contributes an important paper on the rainfall of the Iberian peninsula. In the *Verhandlungen* of the same Society we find papers on the geography and ethnography of Southern Mesopotamia, by Dr. B. Moritz, and on the Isthmus of Corinth, by Dr. A. Philippson.

IT may be useful to state that in No. 1 of the third series of the *Bulletin* of the Egyptian Geographical Society is a connected account in French, by Dr. O. Lenz, of his last journey across Africa.

THE June number of the Journal of the Royal Geographical Society contains the first part of Mr. D. W. Freshfield's paper on the Caucasus; it deals with Suanetia, and is illustrated with maps and diagrams. The same number contains Mr. Woodford's paper on his explorations in the Solomon Islands.

TWO Swedish colonists, MM. Valdau and Knutson, have recently done some interesting geographical work in the Cameroons territory. M. Valdau has explored the northern slopes of the range, which are very thickly peopled by the Bomboko tribe. The main chain of the mountains does not extend as far as 4° 30′ N. lat., as the highest point attained by the traveller, about 4° 28′ N. lat., only measured 2850 feet. M. Knutson has explored the River Memeh, which, he ascertained, empties itself into the sea a little to the south of Rumbi. The river is navigable for thirty miles, to the Düben falls, which are 100 feet in height.

BIOLOGICAL NOTES.

FOSSIL FISH REMAINS FROM NEW ZEALAND.—Mr. Davis has recently described a number of fish remains from the Tertiary and Cretaceo-Tertiary formations of New Zealand. The memoir forms a part of the Transactions of the Royal Dublin Society, and is illustrated by seven well-executed plates of the fossils. Some short time ago Mr. Davis received the remains of some fossil Tertiary Elasmobranchs from Prof. F. W. Hutton, from New Zealand, which formed the subject of a short communication to the Geological Society of London; but a much larger collection having been in the meanwhile received, permission was granted for the withdrawal of the paper, and now, based on several additional collections, we have the present memoir, which for the first time does justice to these interesting fossil forms by full descriptions and excellent figures. The memoir opens with an account of the Tertiary formations of New Zealand, based on the results attained by the Geological Survey under Sir James Hector, while notice is taken also of the views of Prof. Hutton and Sir J. von Haast. In addition to the remains of fish, some Saurian teeth, as well as those of a Squalodon, have been found. Of the thirty-five species of fish described, no less than twenty-eight appear as new species; of these thirty-five, twenty-eight are Sharks, four are Rays, two belong to the Chimerids, and one to the Teleostei. A new species of toothed Whale, *Squalodon serratus*, is also described.—(Transactions of the Royal Dublin Society, vol. iv. (ser. 2), part i. pp. 1–50, plates i.–vii.)

MAMMALS OF LIBERIA.—Dr. F. A. Jentink continues his account of the recent zoological researches in Liberia, which have been carried on for the last seven or eight years by J. Büttikofer, C. F. Sala, and F. X. Stampfli. The amount of information collected by the first-named investigator is very great, and merits the high praise bestowed upon it by the Director of the Leyden Museum. Of the ninety species of Mammals sent home, thirteen belong to the Monkeys, eleven to the Carnivores, thirty-three to the Ruminants, five to the Pachyderms, twenty-five to the Rodents, one Sireniad, four Insectivores, seventeen to the Bats, and three to the Edentates. Among the more interesting species mentioned are the following: *Cercopithecus stampflii*, n. sp., from Pessy Country; *Terpone longiceps*, Gray; *Cephalophus doria*, Ogilby, and *Euryceros euryceros*, Ogilby; *Graphiurus nagtglasii*, n. sp.; *Claviglis crassicaudatus*, n. g. et n. sp.; *Crocidura buttikoferi*, n. sp., and *C. stampflii*, n. sp.; *Pachyura megalura*, n. sp.; *Epomophorus veldkampii*, n. sp.; and *Vesperugo stampflii*, n. sp. This number also contains notes of 151 species of Birds, collected by J. Büttikofer and F. X. Stampfli, during their last sojourn in Liberia. The last-named is still collecting on the Farmington River, a large confluent of the Junk.—("Notes from the Leyden Museum," vol. x. Nos. 1 and 2, January and April, 1888.)

ON NEW ENGLAND MEDUSÆ.—In a list of certain Medusæ, found by Mr. J. Walter Fewkes, off the coast of Maine and from Grand Manan, he redescribes and figures the interesting and beautiful *Nanomia cara*, A. Ag. This Physophore, described some twenty-five years ago, though repeatedly referred to in text-books and general works on zoology, seems to have since escaped attention, but many specimens were found at Grand Manan. It will be remembered that the form thought to be adult by A. Agassiz, is not above six inches in length, but Mr. Fewkes captured specimens measuring, when extended, over four feet in length, and three feet when retracted; while many hundreds were seen of the size of the specimen he figures, which is about sixteen inches long. When floating in the water they were easily distinguished from the southern Physophore, *Agalma elegans*; the nectocalyces are biserial, the specimen figured has thirteen pairs of well-developed bells, and many of the adults had fifteen pairs. Among the most interesting and it would seem exceptional structures in this form are the organs referred to by A. Agassiz as the "third kind of polyps," now called "hydrocysts" or "tasters"; these hang from the polyp stem midway between the polypites, a single adult and many half-developed tasters occurring between each pair of polypites. They are small, slender, flask-shaped bodies, the distal end is closed, and near the basal attachment there is a prominent red body of spherical shape, known as the "oil globule"; each taster has also a single long tentacle. Contrary to what A. Agassiz thought, the adult Nanomia has male and female bells on one and the same colony; each female bell carries a single ovum, which, when they escaped, could be easily seen by the unassisted vision. *Hydrichthys mirus*[1] is also described and figured as a new genus and species belonging to the Hydroida; it was found attached to the side of a small fish (*Seriola zonata*, Cuv.) which had been taken in the dip-net at a time when the sea was quiet. The patch had at first all the appearance of a Fungoid growth. The fish and Hydroid parasite were kept alive for some time in an aquarium, and from the latter many thousands of Medusæ were raised. The Hydroid colony formed a cluster of reddish and orange-coloured bodies; the basal attachment is a flat thin plate with ramifying tubes; upon it are separate clusters of gonosomes and (?) hydranths. Each gonosome is botryoidal; the free extremity of the gonosome is without tentacles, its rim is entire, and it is destitute of Medusa buds. It seems possible that no food is taken in by the gonosomes, but that the whole structure is dependent upon the tubes of the basal plate for its nutrition. The filiform structures (hydranths ?) are elongated flask-shaped bodies of about uniform size, with terminal openings. The Medusa is closely related to Sarsia, and so far shows the new Hydroid to be allied to the Tubularians, but there are not wanting certain features which hint at a kindred to the Siphonophores. The rare and interesting *Callinema ornata*, Verrill, is redescribed, and for the first time figured. With a remark of the author, "that histological researches lose some of their value if not preceded by an accurate identification or specific description of the animal studied, if it be different from known species," we heartily agree.—("Studies from the Newport Marine Laboratory," Bull. Mus. Comp. Anat. Harvard College, vol. xiii. No. 7, February 1888.)

THE BILL FOR THE PROMOTION OF TECHNICAL INSTRUCTION.

THE following is the Bill for the promotion of technical instruction, introduced by the Government:—

Be it enacted by the Queen's most Excellent Majesty, by and with the advice and consent of the Lords Spiritual and Temporal, and Commons, in this present Parliament assembled, and by the authority of the same, as follows:

1.—(1) Any School Board in England may from time to time supply or aid the supply of such manual or technical instruction, or both, as may be required for supplementing the instruction given in any public elementary school in its district, whether under its own management or not.

(2) Manual or technical instruction shall not be supplied or aided under this section except for such scholars as—

(*a*) are recognized by the Education Department as in attendance at a public elementary school and receiving instruction in the obligatory or standard subjects prescribed by the minutes of the Education Department for the time being; and

(*b*) (in the case of technical instruction only) have obtained from the Education Department certificates of having passed the examination in reading, writing, and arithmetic, prescribed by the standard set forth in the schedule to this Act, or an examination equivalent thereto.

(3) For the purpose of supplying or aiding the supply of manual or technical instruction under this section, a School Board shall have the same powers, but subject to the same conditions, as it has for providing sufficient public school accommodation for its district, subject to this restriction that the amount of the rate to be levied in any one year for the additional purposes authorized by this section shall not exceed the sum of *one penny in the pound*.

2.—(1) If a School Board aids the supply of manual or technical instruction in any school or schools under its own management, it shall, on the request of the managers of any other public elementary school in its district fulfilling like conditions as to the supply of manual or technical instruction in conformity with the requirements of the Department of Science and Art, and on proof of sufficient demand for such instruction in that school, aid the supply of such instruction in that school in like manner as it aids such supply in the school or schools under its own management, subject to such terms as may be agreed on or determined in pursuance of this Act.

(2) If the managers of a public elementary school in the district of a School Board object to the terms on which the School Board proposes to aid the supply of technical instruction in that school, the Department of Science and Art shall, on the appli-

[1] *Vide* NATURE, vol. xxxvi. p. 604, where we believe this genus and species were first described by the author.

cation of those managers, determine whether the terms so proposed are reasonable.

3.—(1) Any local authority empowered to carry into execution the provisions of the Public Libraries Acts with respect to the establishment and maintenance of public libraries, public museums, schools for science, art galleries, and schools for art, may from time to time supply or aid the supply of technical instruction by providing or aiding in the provision of teachers, apparatus, or buildings to such extent and on such terms as the authority think expedient, and may exercise its powers under this section either with or without exercising any of its powers under the Public Libraries Acts.

(2) Provided as follows :—

(*a*) In a district for which there is a School Board, the local authority shall not out of their own funds supply or aid the supply of technical instruction suitable for scholars receiving at a public elementary school instruction in the obligatory or standard subjects prescribed by the minutes of the Education Department for the time being, except to the extent, if any, to which the authority was so supplying or aiding before the establishment of a School Board.

(*b*) In a district for which there is not a School Board, the managers of a public elementary school shall not receive aid under this section except for scholars for whom technical instruction may be supplied or aided by a School Board in a district for which there is a School Board.

(3) The amount of the rate to be levied in any one year under the Public Libraries Acts as amended by this Act for the additional purposes authorized by this section shall not exceed the sum of *one penny in the pound*, and where the powers given by the Public Libraries Acts are exercised concurrently with the powers given by this section shall not exceed *twopence in the pound.*

4.—(1) The managers of any technical school in the district of a School Board or local authority may make an arrangement with the Board or authority for transferring their school to that Board or authority, and the Board or authority may assent to any such arrangement.

(2) The provisions of section twenty-three of the Elementary Education Act, 1870, with respect to arrangements for the transfers of schools, shall apply in the case of arrangements for the transfers of schools in pursuance of this section.

5.—Every minute of the Department of Science and Art with respect to the condition on which grants may be made for technical instruction shall be laid on the table of both Houses of Parliament within three weeks after it is made, if Parliament is then sitting, and if Parliament is not then sitting, within three weeks after the then next session of Parliament, and shall not come into operation until one month after being so laid.

6.—In this Act—

The expression "technical instruction" means instruction in the principles of science and art applicable to industries and in the application of special branches of science and art to specific industries or employments. It does not include teaching the practice of any trade or industry or employment, but, subject as aforesaid, includes instruction in the branches of science and art with respect to which grants are for the time being made by the Department of Science and Art, and any other form of instruction which may for the time being be sanctioned by that Department by a minute laid before Parliament and made on the representation of a School Board or local authority that such a form of instruction is required by the circumstances of its district.

The expression "technical school" means a school or department of a school which is giving technical instruction to the satisfaction of the Department of Science and Art.

The expression "manual instruction" means instruction in the use of tools and modelling in clay, wood, or other material.

The expression "the Education Department" means the Lords of the Committee of Her Majesty's Privy Council on Education.

The expression "local authority" means the Council, Commissioners, Board, or other persons or authority carrying into execution, or empowered to carry into execution, the Public Libraries Acts.

The expression "Public Libraries Acts" means the Public Libraries (England) Acts, 1855 to 1887, and the Public Libraries (Ireland) Acts, 1855 to 1884.

7.—This Act may be cited as the Technical Instruction Act, 1888.

SCHEDULE.

STANDARD.

Reading.—To read a passage from some standard author.

Writing.—A short theme or letter on an easy subject, spelling, handwriting, and composition to be considered. An exercise in dictation may, at the discretion of the inspector, be submitted for composition.

Arithmetic.—Fractions, vulgar and decimal, simple proportion, and simple interest.

AGRICULTURAL EDUCATION IN NORTHERN ITALY AND IN PRUSSIA.

MR. COLNAGHI, Consul-General at Florence, in the course of an elaborate Report on his district, refers at some length to agricultural education in the province of Florence. He describes especially the well-known "Academia dei Georgofili," the Tuscan Society of Agriculture, the Comizi Agrari, or Agricultural Boards, the Stazioni Agrarie, and also refers to the various institutes and schools which have been established of late years in the province. The "Academia dei Georgofili" of Florence was founded in 1753, and was the first Association of the kind formed in Italy to promote the science of agriculture. On the roll of the Academy are to be found the names of the most distinguished Italian agronomists, and the long series of its Transactions contains important papers on all points of interest connected with the agriculture of Tuscany.

The Royal Tuscan Society of Horticulture, which was established in 1854, now numbers about 700 members. Much useful work has been done by this body in encouraging the improved cultivation of fruit, vegetables, flowers, and ornamental places and by the holding of annual shows in Florence.

Each district of the province has its Comizio Agrario, the objects of which are to extend agricultural skill and knowledge, or encourage improvements, and to form a centre for the diffusion of information. The Comizi offer prizes for improvements in cultivation, hold Conferences on various subjects, and publish Bulletins containing much useful information on practical subjects. These bodies are supported by members' subscriptions, and by grants from the Minister of Agriculture and from the province. Besides the annual shows held at Florence, there are regional agricultural shows (Concorsi Agrarii Regionali), instituted by the Ministry of Agriculture and the Comizi Agrari, which are held at stated periods, and in which some five or six provinces are included. These larger shows have been useful in bringing agriculturists from various parts of the country together, showing the latest improvements in machinery, and in displaying the various products of the different districts.

At the "Stazione Agraria" of Florence, which is a branch of the Technical Institute, and is under the direction of Prof. Bechi, experiments are made on the culture and diseases of the vine, the olive, and other plants, and analyses are made of soil, minerals, water, wines, &c. Attached to the Stazione is an experimental farm six hectares in size, and also a Government depot of agricultural machinery.

There is also in Florence a Bureau of Agricultural Entomology, under Prof. Tragioni-Tozzetti, where great attention is paid to the Phylloxera. This Bureau is in fact the centre of information for the whole of Italy on entomological subjects.

For practical instruction the province contains the Regio Istituto Forestale (Vallombrosa), the Regia Scuola di Pomologia e d'Orticoltura (Florence), and the Scuole Agrarie of Castaletti, near Signa, and of Scandicci, in the immediate neighbourhood of Florence. The Forest Institute of Vallombrosa, now under the Presidency of Prof. Piccioli, who is assisted by eight professors, was founded in 1869, on the model of the forestry schools of France, Germany, and Austria, to supply a sufficient number of trained officers for the Department of Woods and Forests. From 1869 till the present time, 159 students have entered the school, and of these 136 have received diplomas. All of these have entered into the service of their native country, except one who was a Swiss. The course of study lasts three years, during which time instruction is given in forestry and kindred subjects, and in French and German. The limits of age at entrance are sixteen and twenty-two, and the annual charge for board, residence, and instruction is fixed at 700 lire. The State pays a portion of the cost of some of the students, and sometimes their respective provinces do so.

Attached to the Institute is a library of works on forestry, and also the requisite collections and instruments, both chemical and scientific. A nursery which contains nearly 450,000 plants, and which can supply annually nearly 100,000 plants of from three to five years old, is also annexed. There is also a small fish-breeding establishment, in which about 10,000 trout-fry are annually hatched, and placed in the neighbouring streams.

The Royal School of Pomology and of Horticulture was established in 1882, and is now under the direction of Prof. Valvassori. Its object is to train vegetable and fruit gardeners. The course lasts three years, and is both theoretical and practical. The age for the admission of pupils is from fourteen to seventeen, preference being given to the sons of the smaller farmers, and the charges are 25 lire per month, besides 20 lire for the purchase of gardening-tools, &c., and an entrance fee of 10 lire. There are five professors, with a censor and two gardeners, and at present the number of pupils is thirty-two. For practical instruction the school possesses an orchard, and kitchen and flower gardens.

The Agricultural Institute of Castaletti has been in existence since 1859, when it was founded by Commendatore Leopoldo Cattani-Cavalcanti. It is now under the direction of Signor Riccardi-Manelli. One section of the school was placed on the footing of a Government technical institute during the life-time of the founder; but this has now been changed by the present Director, because the school has for its object, not the production of engineers and surveyors, but of factors or agents and head gardeners. The course of instruction in this institution lasts for four years, and the age of admission is from eleven to fifteen. Of late the charges have been increased, and in consequence the number of students has fallen from seventy to fifty. The entrance fee is now 50 lire; board, lodging, &c., 165 lire for the first and second years, and 180 lire for the third and fourth years; and 8 lire in addition per month for washing. The institution is not self-supporting.

The Agricultural School of Scandicci was founded as recently as 1884 by Count Napoleone Passerini for charitable purposes, his own villa being given up to the work. It was first only a day-school, but this year boarders have been admitted, and there are now ten boarders and eight externs. The object of the institution is to make good managers of rural estates. The course of study lasts for three years; the ages of admission are from fifteen to eighteen; the entrance fee is 10 lire, boarders paying in addition 36 lire per month, and 2 extra for washing. There are in all seven professors and masters. There is an experimental farm of 100 hectares in extent attached to the school, and a good library, and zoological, mineral, and agricultural collections, a chemical laboratory, an apiary, and a pigeon-house. A meteorological observatory of the second class, affiliated to the Central Observatory at Rome, is also annexed. The diplomas awarded to the pupils at the close of their course of study are countersigned by a special delegate of the Government.

According to the Report recently presented to the Foreign Office by Sir E. Malet on agricultural education in Prussia, the State annually gives £49,625 for agricultural instruction in that country, and £38,401 to the veterinary Colleges. Out of the former grant are supported the two Agricultural Colleges of Berlin and Poppelsdorf, the Pomological Institutes of Proskau and Geisenheim, and a station near Wiesbaden for experiments in agricultural chemistry; and subsidies are given to various provincial schools which are supported by local Boards but inspected by the central executive of the province. At the two Colleges the education is mainly scientific and theoretical, the ordinary course consisting of two terms of six months each. At the end of each term the subjects of examination are the science of farming and planting, farm management, physics and chemistry, botany, zoology, animal physiology, mineralogy, and geology. On passing these examinations the students are entitled to diplomas of proficiency in agricultural science. Those who wish to become land surveyors can proceed to a further course of two terms of six months each, in which the instruction given is of a most advanced kind, embracing mathematics, trigonometrical surveying, levelling, engineering, forestry, and plantation, the science of breeding and rearing cattle, dairy farming, mechanics and agricultural machinery, besides a course of law bearing on questions with which land surveyors have to do. According to the most recent report, the Berlin Agricultural College was attended by 98 students in the summer term, 12 of whom proceeded to the more advanced course, and in the winter term by 155 students, 27 of whom went in for the higher course. Poppelsdorf College was attended by 76 in the summer term, of whom 45 went on to the higher course, and in the winter term by 87, of whom 57 attended the larger course. With regard to the lower-grade schools receiving help from the grant in aid of agricultural education, 16 are intermediate schools which get £13,365 every year from the State. The school money varies from £3 5*s.* to £1 10*s.* per term of six months, and the subjects taught in these institutions comprise chemistry, mineralogy, physics, zoology, veterinary science, and farming. There are also numerous local winter elementary schools which supplement by theoretical training the practical teaching which the pupils have had in the fields in spring and autumn. £6648 is annually given to them.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—An examination will be held at Cavendish College on Tuesday, July 24, and following days, according to the results of which it is intended to award eight Scholarships of £30 a year, provided that candidates of sufficient merit present themselves. Candidates must be under eighteen years of age on October 1, 1888, and may offer for examination one or more of the following subjects: Classics, Mathematics, Natural Science, Modern Languages. The Scholars elected will be required to come into residence at Cavendish College in October 1888, and commence study for a Tripos or the Engineering course. Medical students may conveniently combine their medical work with the course for the Natural Science Tripos. It is also intended to offer in June 1889 three Scholarships of £30 to be competed for by students of the College who will then have resided not longer than one year. The College fee for board, lodging, and tuition, is £25 for each of the three University terms, and £15 for residence (optional) in the Long Vacation. For further information apply to the Bursar, Cavendish College, Cambridge.

In the paragraph last week about Prof. Darwin's lectures (p. 117), for "tin" read "sun."

SCIENTIFIC SERIALS.

Bulletin de la Société des Naturalistes de Moscou, 1887, No. 4 —On organic compounds in their relations to haloid salts of aluminium, by G. Gustafson (in German). In this second part the following conclusions are arrived at. The organic compounds undergo deep modifications in presence of the above salts. The reactions of addition are the chief ones, but the most interesting are those undergone by the aromatic hydrocarbons under the influence of chloride and bromide of aluminium; although most unstable, and therefore sometimes viewed as mere molecular compounds, they show a deep modification of the hydrocarbons from which they issue. They explain also the *rôle* of salts in organisms.—On the regeneration of lost organs in spiders, by V. Wagner (in French). This is the result of a double simultaneous process; the atrophy of the tissues belonging to the lost member, and the growth of the new one in the atrophied remnants of the old member. Both processes are described and illustrated.—Short notes on some (eighteen) Russian species of the genus *Blaps*, by E. Ballion (in German). —On two new Branchiopods from the Transcaspian region (*Apus haeckelii*, n. sp., and *Artemia asiatica*, n. sp.), by Dr. A. Walter.—Enumeration of the vascular plants of the Caucasus, by M. Smirnow (continued). The Ranunculaceæ are described; they contain ninety-eight species, belonging to seventeen genera, and out of them thirty-seven belong to the genus *Ranunculus*, and thirteen to that of *Delphinium*. The *Myosurus*, *Garidella*, *Caltha*, and *Actæa* number only one species each. The total number of Caucasian Phanerogams, according to Ledebour's "Flora Rossica," is 2965; now it must be estimated at about 4000 species. Out of the ninety-eight species of Ranunculaceæ described, forty belong exclusively to the flora of the East, while fifty-two are met with in South Russia, thirty in the Crimea, thirty-three in the Altai, twenty-four around Lake Baikal, and only twenty-one in the Urals, and eighteen in North Russia. Very interesting remarks follow as to the distribution of the Ranunculaceæ in separate parts of the Caucasus.

1888, No. 1.—Some remarks on the consequences of the earthquake of February 1887 in the Riviera, by H. Trautschold.—The chief noxious insects on tobacco in Bessarabia, an elaborate research by Prof. K. Lindeman. (Both papers in German.)—Count Alexis Razumovsky, first President of the Society, by Dr. Benzengre (in French).—List of plants of Tambof, by D. Litvinoff (continued).—On the hairs called auditive of the spiders, by W. Wagner (*Gehör-Organe* of Dahl). They belong to different types, and none of them can be recognized as performing the auditive function; they seem merely to be tactile organs of a higher structure.—Studies on the palæontological history of the *Ungulatæ*, by Marie Pavloff (second memoir). After having discussed the genealogy of the horse as viewed by V. Kovalevsky, Messrs. Marsh, Cope, Lydekker, Branco, and Schlosser, and discussed the rich material which Mrs. Pavloff was in possession of, the writer arrives at the following scheme. The eldest ancestors of the horse, *Phenacodus*, are found in the Eocene of North America; in Europe they are represented by the *Hyracotherium leporinum*, which, together with the *Pachynolophus* and *Anchilophus*, inhabited both continents. In the Miocene we find the *Anchitherium*, in America first, and later on in Europe; it was transformed in America into the *Protohippus* of the Mio-Pliocene. This last gave rise to the *Hippidium* and *Equus*, which largely developed during the Pliocene period in America (*E. parvulus*), Asia (*E. nomadicus*), Europe and Africa, where the *E. stenonis* was the ancestor of the Post-Pliocene *Equus caballus*. In how far our present horse originates from this later will be discussed next. Two plates illustrate the paper, written in French.

The *Memoirs of the Odessa Society of Naturalists* (vols. xi. and xii.) contain the usual quantity of elaborate work, especially in anatomy and physiology. The papers on the embryogeny of the fresh-water lobster, by M. Morin; on the embryogeny of the Caucasian scorpion *Androctonus ornatus*, by MM. A. Kovalevsky and Shulghin; on the development of the *Urospora mirabilis*, by M. Woltke; on the embryology of the *Mysis chameleo*, by M. Nusbaum; and on the morphology of the *Haplotrichum roseum*, by M. Khmielevsky, are elaborate articles profusely illustrated by excellent plates.—M. Krasilschik's researches on the structure and life of the *Cercobodo laciniægerens*—a new genus of the Flagellatæ—are most interesting, showing how this microscopic organism preys on Bacteria and digests them, and how complicated is its organization altogether.—The same author contributes an interesting paper on the parasite Fungi of insects, and M. Khawkin has an article on the buccal apparatus of the *Euglenæ* and *Astasiæ*, as also on the laws of heredity in the case of unicellular organisms; and Dr. Kultchitsky studies the intestinal canals of several fishes.—Geology and mineralogy are represented by R. Prendel's article on the Wiluite, from which it appears that the crystals of this interesting mineral have a double composition—those parts of it which penetrate into the depth of the crystal as cones set upon the surfaces of the pyramids differing both by their density and refractive power from the parts which are built upon the faces of the prisms; three papers by Prof. Sintsoff on the water-bearing deposits of Kishineff, the Steppe deposits on the left bank of the Lower Volga, and the Pliocene of South Russia; and on the crystalline rocks of Crimea, by M. Prendel.—Prof. Klossovsky contributes a paper on the oscillations of temperature and density of the water of the Black Sea in the neighbourhoods of Odessa; and Mrs. Mary Balashoff has an article on the influence of small ponds and of limited supplies of water on the development of *Planorbis*.—Chemistry is represented by one paper, on the laws of dissolution of salts, by R. Umoff.

SOCIETIES AND ACADEMIES.

London.

Royal Society, April 26.—"On the Occurrence of Aluminium in Certain Vascular Cryptogams." By A. H. Church, M.A., F.C.S. Communicated by Dr. J. H. Gilbert, F.R.S.

Most of the older and more complete analyses of plant-ashes disclosed the presence of sensible quantities of alumina. But of late years this substance has been regarded as accidental, and has been excluded from ash-constituents with the single exception of certain species of *Lycopodium*. Since 1851 several analysts have proved the presence of large quantities of alumina in the ashes of these plants The author has confirmed and extended their results, and has shown that the allied genus *Selaginella* does not absorb alumina. He found, however, two species of *Lycopodium*—namely, *L. Phlegmaria* and *L. billardieri*—from which this constituent is absent. The anomaly was explained by the epiphytic nature of these plants, which have no direct access to the soil. The author has further examined certain species belonging to genera nearly related to *Lycopodium*, such as *Equisetum*, *Ophioglossum*, *Salvinia*, *Marsilea*, and *Psilotum*, in all cases with negative results. But he has found 20 per cent. of alumina in the ash of a New Zealand tree-fern, and has also discovered abundance of this substance in *Cyathea medullaris* and *Alsophila australis*, and more than mere traces in *Dicksonia squarrosa*. The last part of the paper is occupied with some considerations having reference to the connection between elementary plant-food and the periodic law.

May 17.—"On the Electromotive Properties of the Leaf of *Dionæa* in the Excited and Unexcited States." No. II. By J. Burdon-Sanderson, M.A., M.D., F.R.S., Professor of Physiology in the University of Oxford.

The author has continued his experimental inquiries, of which the results were communicated to the Royal Society under the same title in 1881. In the introduction to the paper he gives a summary of his previous observations, which led to the conclusion that the property by virtue of which the excitable structures of the leaf respond to stimulation, is of the same nature with that possessed by the similarly-endowed structures of animals. He then proceeds to state that the main purpose of his subsequent investigations has been to determine the relation between two sets of phenomena which might, in accordance with the language commonly used in animal physiology, be termed respectively those of the "resting current" and of the "action current" of the leaf, *i.e.* between the electrical properties possessed by the leaf when stimulated, and those which it displays when at rest. Assuming the excitatory response in the leaf to be of the same nature as the excitatory variation or "action current" in muscle and nerve, the question has to be answered, whether in the leaf the response is a sudden diminution of a previously existing electromotive action (according to the pre-existence theory of du Bois-Reymond), or the setting up at the moment of stimulation of a new electromotive action—in short, whether and in how far the two sets of phenomena are inter-dependent or the contrary.

An observation recorded in his former paper suggested proper methods. It had been shown that by passing a weak voltaic current through the leaf for a short period in a particular direction, its electromotive properties could be permanently *modified* without loss of its excitability. If it could be shown that the influence of this modification extended to both orders of phenomena, those of rest and excitation, and that both underwent corresponding changes of character under similar conditions, this would go far to prove that an essential relation existed between them.

Acting on this suggestion, the author has had recourse to modes of experiment similar to those which have been employed during the last few years in the investigation of the newly-discovered "secondary electromotive" phenomena of muscle and nerve (see "Oxford Biological Memoirs," vol. i. part 2). The details of these experiments, made in 1885, are given in the first three sections of the paper. They relate to (1) the more immediate effect of the current as seen in the records of successive galvanometric observations made at regular intervals; (2) the more permanent influence of the current on the electromotive properties of the unexcited leaf, and on its electrical resistance; and (3) the concomitant modification of its behaviour when stimulated.

The general result of these experiments is to show that the two orders of phenomena, the excitatory and those which relate to the resting state, are so linked together that every change in the state of the leaf when at rest conditionates a corresponding change in the way in which it reacts to stimulation—the correspondence consisting in this, that the direction of the response is opposed to that of the previous difference of potential between the opposite surfaces, so that as the latter changes from ascending to descending, the former changes from descending to ascending.

The author considers that this can only be understood to mean that the constantly operative electromotive forces which find their expression in the persistent difference of potential between the opposite surfaces, and those more transitory ones which are called into momentary existence by touching the sensitive filaments or by other modes of stimulation, have the same seat, and that the opposition between them is in accordance with a principle applicable in common to the excitable structures of plants and animals, viz. that the *property which renders a structure capable of undergoing excitatory change is expressed by relative positivity, the condition of discharge by relative negativity*.

The fourth section of the paper is devoted to an investigation made in 1887, of the events of the first second after excitation, made with the aid of a pendulum-rheotome specially adapted for the purpose. The fifth contains the description of the records obtained by photographing the electrical phenomena of the excitatory reaction, as observed with the aid of the capillary electrometer, on rapidly-moving plates. Both of these series of observations serve to confirm and complete the results obtained by other methods. The photographs were exhibited.

Physical Society, May 12.—Prof. Reinold, F.R.S., President, in the chair.—The following papers were read:—Note on the condition of self-excitation in a dynamo machine, by Prof. S. P. Thompson. It is a well-known fact that a series dynamo running at a given speed will not excite itself unless the resistance is less than a certain value, depending on the speed and construction of the machine, and if the resistance is slightly less than this critical value the excitation will not be such as to saturate the magnets. According to the primitive statement of the action of self-exciting dynamos on the "compound interest law," a dynamo should excite itself to saturation at any finite speed providing the resistance is not infinite. An explanation of the observed facts is given in the paper, without any assumption as to the curve of magnetization. If $E =$ E.M.F. of the machine, $n =$ speed, $C =$ number of wires on outside of armature, $N =$ number of magnetic lines, $i =$ current, $S =$ number of turns on magnet, ΣR and $\Sigma \rho$ the sums of the electric and magnetic resistances respectively, then $E = nCN$, $i = nCN/\Sigma R$, and $N = 4\pi Si/\Sigma\rho$. From these it is easily seen that $4\pi nCS = \Sigma\rho \cdot \Sigma R$, (A).; *i.e.* for a dynamo running at constant speed the product of the magnetic and electric resistances is constant, and the dynamo will not excite itself if ΣR is greater than $4\pi nCS/\Sigma\rho$. Similarly for a given value of ΣR, excitation is impossible if n is less than $\Sigma\rho \cdot \Sigma R/4\pi CS$. For a value of ΣR less than the critical value the excitation increases until the magnetic resistance is increased so that equation (A) is satisfied. The corresponding formula for shunt machines is $4\pi nCZ = \Sigma\rho\left\{(r_a + r_s) + \frac{r_a r_s}{R}\right\}$; where $Z =$ number of shunt turns; r_a, r_s, and R, the resistances of armature, shunt, and external circuits respectively. In the discussion which followed, Mr. Kapp descibed a method used in testing dynamos, for determining the minimum speed at which dynamos will excite themselves, and from thence determining the magnetic resistance of the air gap. In all cases experiment showed this to be less than the calculated resistance, generally in the proportion of 1500 to 1860, the difference being greater in low-tension machines. Prof. Ayrton pointed out that permanent magnetism was not taken into account, and that the apparent resistance due to self-induction, and between the brushes and commutator were considerable for small currents. Lord Rayleigh and Sir W. Thomson had shown critical speeds for given resistances to exist in Faraday's disk dynamo. He (Lord Rayleigh) did not approve of the term "magnetic resistance," and thought "reluctance," as recently suggested by Mr. Heaviside, would be preferable.—Note on the conditions of self-regulation in a constant potential dynamo machine, by the same author. In "Dynamo-Electric Machinery" a formula $\frac{Z}{S} = \frac{r_s}{r_a + r_m}$ is given as expressing the ratio of the number of turns in the shunt and series windings of a compound dynamo. This is on the assumption that there is no saturation within the working limits. As this assumption is not legitimate, a correcting factor is necessary. The factor is shown to be the ratio of the average permeability over the whole working range to the permeability corresponding with no external current. The formula is transformed so as to be expressed in terms of the "satural" data of the machine, which, as shown in a previous paper, can be calculated from its details.—On magnetic lag, and the work lost due to magnetic lag in alternating current transformers, by Mr. Thomas H. Blakesley. The method adopted to detect the lag is to place dynamometers in both circuits, and one with a coil in each. Then, on the supposition that the E.M.F. of the secondary circuit is entirely due to the changing magnetism of the core, the author proves that the tangent of the magnetic lag angle must be equal to $\dfrac{\frac{m}{n}C\alpha_3 - B\alpha_2}{\frac{m}{n}\sqrt{(AB\alpha_1\alpha_2 - C^2\alpha_3^2)}}$; where m and n are the number of turns in the primary and secondary coils respectively; A, B, C, the constants of the dynamometers; and α_1, α_2, α_3, their angular reading. A is such that $A\alpha_1 = \frac{I_1^2}{2}$, where I_1 is the maximum value of the primary current. A table of actual results is given, where the magnetic lag is about $5\frac{1}{4}°$. The whole power given out by the machine takes the form $r_1 A\alpha_1 + r_2 \frac{m}{n} Cd_2$, where r_1 and r_2 are the resistances of the primary and secondary circuits, while the power lost in hysteresis is expressed by $r_2\left(\frac{m}{n}C\alpha_3 - B\alpha_2\right)$. The lag is attributed to an induced magnetic stress called into being by the increasing or decreasing magnetism itself, and always opposing it as motion in a medium induces an opposing force of friction. By supposing such an induced magnetic stress in quadrature (as Mr. Blakesley expresses it) with the magnetism, and of such a value as when compounded with the stresses due to the currents shall bring the resultant into quadrature with the secondary current, the effective magnetic stress is obtained. This involves a new idea called magnetic self-induction with its coefficient. The whole problem is treated by the geometrical method, which the author has applied to several other problems in alternating currents. Mr. Kapp, Profs. Thompson, Perry, and Ayrton, and Lord Rayleigh took part in discussing the paper.—On a simple apparatus for the measurement of the coefficient of expansion by heat, by Prof. W. E. Ayrton, F.R.S., and Prof. J. Perry, F.R.S. The apparatus consists of a metal tube, within which the wire or rod whose coefficient is to be determined is placed. One end of the wire is rigidly attached to one end of the tube, and the other end connected to an Ayrton and Perry magnifying spring, a pointer attached to which indicates the change of length due to alteration of temperature. Steam or water may be passed through the tube, the temperature of the wire being shown on a thermometer. The arrangement is very sensitive, and with a pointer about 20 cm. long, the motion is magnified about 1000 times.—A magnifying spring attached to an aneroid was also shown, and its great sensibility demonstrated. A combination of a spring of large diameter and pitch with one of small diameter and pitch was exhibited. By such a combination small rotations can be immensely magnified. The great features of the patent spring as a magnifier are the entire absence of friction and back lash, and the large range of proportionality.

Chemical Society, May 17.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—Researches on the constitution of azo- and diazo-derivatives; (iv.) diazo-amido-compounds, by Prof. Meldola, F.R.S., and Mr. F. W. Streatfield.—The colour of some carbon compounds, by Prof. Carnelly, and Mr. J. Alexander. An investigation of a number of metallic derivatives of ortho- and para-nitrophenol has given the following results: (1) in all cases without exception the colour passes towards the red end of the spectrum as the temperature rises; (2) the colour of the ortho-derivative is nearer the red end than that of the corresponding para-compound; (3) a comparison of the nitrophenates of the metals belonging to the same sub-group shows that the colour passes towards the red end as the atomic weight of the metal increases; (4) when the same salt occurs in both the anhydrous and the hydrated state, the colour passes towards the red end as the quantity of water of crystallization diminishes; (5) as regards the salts investigated, the para-compound always takes up a larger quantity of water of crystallization than the corresponding ortho-compound. In the course of the discussion which followed the reading of the paper, Prof. Armstrong, F.R.S., remarked that the facts advanced were far too few to justify the very general conclusions arrived at by the authors; all who had worked with the nitrophenols were well aware that the colour changed on heating in the manner described; and there was no novelty in the statement that the para-nitrophenols crystallized with the larger proportion of water. Referring to the authors' fourth deduction, he quoted calcium parachlorodiorthonitrophenate as an exception, since this compound can be obtained either in yellow anhydrous crystals, or in deep-orange hydrated crystals.—The identity of natural and artificial salicylic acid, by Prof. Hartley, F.R.S. Spectroscopic examination of the two compounds establishes their identity.—Researches on the relation between the molecular structure of carbon compounds and their absorption spectra (part viii.), by the same.—A definition of the term atomic weight and its reference to the periodic law, by the same. The author is of opinion that the fact that the atomic weights are real measures of the quantity of matter in the atoms of the elements is often overlooked, and advocates the adoption of the definition: The atomic weight of an element is the ratio of the mass of its atom to the mass of an atom of hydrogen. The periodic law then admits of being stated thus: The properties of the atoms are a periodic function of their masses.

Geological Society, May 23.—Dr. W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—On the spheroid-bearing granite of Mullaghderg, Co. Donegal, by Dr. Frederick H. Hatch. Communicated with the permission of the Director-General of the Geological Survey. This paper deals with a remarkable variety of granite which may be compared with the well-known orbicular diorite or Napoleonite of Corsica. According to Mr. J. R. Kilroe, of the Geological Survey of Ireland, who first discovered this interesting rock, the concretionary balls occur in close juxtaposition in a mass of granite of 5 or 6 cubic yards in size. They have not been found in any other portion of the granite area. The author gave a detailed description of the microscopic structure of the normal granite. He also described the spheroidal bodies, and gave a synopsis of the literature concerning the occurrence of similar concretionary bodies in granite. The conclusion arrived at was, that concretionary bodies occurring in granite may, according to the mode of arrangement of their constituents, be divided into three classes, viz. (1) the *concretionary patches* of Phillips; (2) the *granospherites* of Vogelsang; (3) the *belonospherites* of Vogelsang. The spheroids from Mullaghderg belong to the last-mentioned class. They must be regarded as concretions formed, during the consolidation of the granite magma, by a process of zonal and radial crystallization around an earlier-formed nucleus. Remarks on this paper were offered by Mr. Rutley, Prof. Bonney, Dr. Hicks, and Prof. Judd.—On the skeleton of a Sauropterygian from the Oxford Clay near Bedford, by R. Lydekker.—On the Eozoic and Palæozoic rocks of the Atlantic coast of Canada in comparison with those of Western Europe and the interior of America, by Sir J. W. Dawson, F.R.S. The author referred to the fact that since 1845 he had contributed to the Proceedings of the Geological Society a number of papers on the geology of the eastern maritime provinces of Canada, and it seemed useful to sum up the geology of the older formations and make such corrections and comparisons as seemed warranted by the new facts obtained by himself, and by other observers of whom mention is made in the paper. With reference to the Laurentian, he maintained its claim to be regarded as a regularly stratified system probably divisible into two or three series, and characterized in its middle or upper portion by the accumulation of organic limestone, carbonaceous beds, and iron-ores on a vast scale. He also mentioned the almost universal prevalence in the northern hemisphere of the great plications of the crust which terminated this period, and which necessarily separate it from all succeeding deposits. He next detailed its special development on the coast of the Atlantic, and the similarity of this with that found in Great Britain and elsewhere in the west of Europe. The Huronian he defined as a littoral series of deposits skirting the shores of the old Laurentian uplifts, and referred to some rocks which may be regarded as more oceanic equivalents. Its characters in Newfoundland, Cape Breton, and New Brunswick were referred to, and compared with the Pebidian, &c., in England. The questions as to an upper member of the Huronian or an intermediate series, the Basal Cambrian of Matthew in New Brunswick, were discussed. The very complete series of Cambrian rocks now recognized on the coast-region of Canada was noticed, in connection with its equivalency in details to the Cambrian of Britain and of Scandinavia, and the peculiar geographical conditions implied in the absence of the Lower Cambrian over a large area of interior America. In the Ordovician age a marginal and a submarginal area existed on the east coast of America. The former is represented largely by bedded igneous rocks, the latter by the remarkable series named by Logan the Quebec Group, which was noticed in detail in connection with its equivalents further west, and also in Europe. The Silurian, Devonian, and Carboniferous were then treated of, and detailed evidence shown as to their conformity to the types of Western Europe rather than to those of America. In conclusion, it was pointed out that though the great systems of formations can be recognized throughout the northern hemisphere, their divisions must differ in the maritime and inland regions, and that hard and fast lines should not be drawn at the confines of systems, nor widely different formations of the same age reduced to an arbitrary uniformity of classification not sanctioned by Nature. It was also inferred that the evidence pointed to a permanent continuance of the Atlantic basin, though with great changes of its boundaries, and to a remarkable parallelism of the formations deposited on its eastern and western sides. The President, whilst recognizing the importance of the paper, doubted whether the question of correlation of the Pre-Cambrian rocks on either side of the Atlantic was ripe for discussion. Dr. Hicks agreed with most of the conclusions of the author, including the correlation of the Huronian with the Pebidian. Some observations on the paper were also made by Dr. Scott, Dr. Hinde, and Mr. Marr.—On a hornblende-biotite rock from Dusky Sound, New Zealand, by Captain F. W. Hutton.

Zoological Society, May 15.—Dr. A. Günther, F.R.S., Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of April 1888; and called special attention to two Rock-hopper Penguins from the Auckland Islands, presented by Capt. Sutcliff, R.M.S.S. *Aorangi*, on April 19; also to two Indian Hill-Foxes, and to a fine example of the Spotted Hawk-Eagle (*Spizaëtus nipalensis*), presented by Colonel Alex. A. A. Kinloch, and received on April 20.—A communication was read from Mr. George A. Treadwell, containing an account of a fatal case of poisoning from the bite of the Gila Monster (*Heloderma suspectum*).—Mr. Boulenger exhibited the type-specimen of a singular new genus of Snakes (*Azemiops feæ*) recently discovered by M. Fea, of the Museo Civico of Genoa, in the Kakhim Hills, Upper Burma. Mr. Boulenger proposed to refer this genus provisionally to the family Elapidæ.—The Secretary read a letter addressed to him by Mr. E. C. Cotes, Entomological Department, Indian Museum, Calcutta, respecting the insect-pests of India, and requesting the assistance of entomologists in working out the species to which they belong.—Mr. H. Seebohm exhibited and made remarks on a series of specimens of Pheasants from Mongolia, Tibet, and China, including examples of the two species discovered by Colonel Prjevalski, *Phasianus strauchi* and *P. vlangali*.—Prof. F. Jeffrey Bell exhibited and made remarks on three specimens of a large Pennatulid (*Funiculina quadrangularis*) obtained by Mr. John Murray on the west coast of Scotland. They showed very clearly the differences between examples of this species of different ages.—Mr. R. Bowdler Sharpe gave an account of a third collection of birds made by Mr. L. Wray in the main range of mountains of the Malay Peninsula, Perak. The present paper contained descriptions of ten species new to science, amongst which was a new *Pericrocotus*, proposed to be called *P. wrayi*.—Prof. F. Jeffrey Bell read the descriptions of four new species of Ophiuroids from various localities.—Mr. F. E. Beddard read a paper containing remarks on certain points in the visceral anatomy of *Balæniceps rex* bearing upon its affinities, which he considered to be with the Ardeidæ rather than with the Ciconiidæ. Mr. G. B. Sowerby gave the description of a gigantic new species of Mollusk of the genus *Aspergillum* from Japan, which he proposed to name *A. giganteum*.

Institution of Civil Engineers, May 29.—Annual General Meeting.—Mr. George B. Bruce, President, in the chair.—After the reading of the Report, hearty votes of thanks were passed to the President, to the Vice-Presidents, and other members of the Council, to the Auditors, to the Secretaries and staff, and to the Scrutineers.—The ballot for the Council resulted in the election of Mr. G. B. Bruce, as President; of Sir John Coode, Mr. G. Berkley, Mr. H. Hayter, and Mr. A. Giles, M.P., as Vice-Presidents; and of Mr. W. Anderson, Mr. B. Baker, Mr. J. W. Barry, Sir Henry Bessemer, F.R.S., Mr. E. A. Cowper, Sir James N. Douglass, F.R.S., Sir Douglas Fox, Mr. C. Hawksley, Mr. J. Mansergh, Mr. W. H. Preece, F.R.S., Sir Robert Rawlinson, K.C.B., Sir E. J. Reed, K.C.B., F.R.S., M.P., Mr. W. Shelford, Mr. F. C. Stileman, and Sir William Thomson, F.R.S., as other members of the Council.—The Council has made the following awards to the authors of some of the papers read and discussed at the ordinary meetings during the past session, or printed in the minutes of proceedings without being discussed, as well as for papers read at the supplemental meetings of students :—For papers read and discussed at the ordinary meetings: a Telford Medal and a Telford Premium to Robert Abbott Hadfield, for "Manganese in its Application to Metallurgy," and "Some Newly-discovered Properties of Iron and Manganese"; a Watt Medal and a Telford Premium to Peter William Willans, for "Economy-Trials of a Non-condensing Steam-Engine, Simple, Compound, and Triple"; a Telford Medal and a Telford Premium to Dr. Edward Hopkinson, for "Electrical Tramways—the Bessbrook and Newry Tramway"; a Watt Medal and a Telford Premium to Edward Bayzand Ellington, for "The Distribution of Hydraulic Power in London"; a Telford Medal and a Telford Premium to Josiah Pierce, Jun., for "The Economic Use of the Plane-Table in Topographical Surveying"; a George Stephenson Medal and a Telford Premium to Sir Bradford Leslie, K.C.I.E., for "The Erection of the 'Jubilee' Bridge, carrying the East Indian Railway across the River Hooghly at Hooghly"; and the Manby Premium to the late Hamilton Goodall, for "The Use and Testing of Open-hearth Steel for Boiler-making." For papers printed in the Proceedings without being discussed: a Watt Medal and a Telford Premium to Prof. Victor Auguste Ernest Dwelshauvers-Dèry, for "A New Method of Investigation applied to the Action of Steam-Engine Governors"; and Telford Premiums to William Mann Thompson, for "Improved Systems of Chaining for Land and Engineering Surveys"; to James William Wyatt, for "Sizing Paper with Rosin"; and to Dugald Drummond, for "The Heating of Carriages by Exhaust Steam on the Caledonian Railway." For papers read at the supplemental meetings of students the following Miller Prizes have been given: to David Sing Capper, for "The Speed-Trials of the latest addition to the *Admiral* Class of British War-Vessels"; to Lawrence Gibbs, for "Pumping-Machinery in the Fenland and by the Trentside"; to Harold Medway Martin, for "Arched Ribs and Voussoir Arches"; to John Henry Parkin, for "River-Gauging at the Vyrnwy Reservoir"; to Alfred Chatterton, for "The Prevention and Extinction of Fires"; to John Holliday, for "Boiler Experiments and Fuel-Economy"; to Arthur Wharton Metcalfe, for "The Classification of Continous Railway-Brakes"; to Robert Jarratt Money, for "Railway Engineering in British North America."

Victoria Institute, June 4.—The annual general meeting was held at the house of the Society of Arts. The President, Prof. G. G. Stokes, P.R.S., M.P., took the chair. The twenty-second Annual Report was read by Captain Frank Petrie, the Honorary Secretary, and Sir Monier-Williams delivered an address on mystical Buddhism. A vote of thanks was accorded to the President.

Paris.

Academy of Sciences, May 28.—M. Janssen, President, in the chair.—New theory of equatorials (continued), by MM. Lœwy and Puiseux. In order to verify the already explained theory, the authors here compare the values of the constants obtained by physical processes with those resulting from the astronomical methods based on the observation of transits or on the apparent variations of the right ascensions or declinations. They conclude with some general remarks on the employment of the equatorial *coudé*.—On the measurement of low temperatures, by MM. L. Cailletet and E. Colardeau. The researches here described have been undertaken for the purpose of obviating the difficulties hitherto felt in employing hydrogen thermometers for the measurement of low temperatures.—Researches on ruthenium, by MM. H. Debray and A. Joly. These studies are occupied chiefly with the rutheniates of potassa and silver, and the heptarutheniates of potassa and soda. The authors find that, although there exists an evident analogy in the composition and reactions of the rutheniate and heptarutheniate of potassa on the one hand, and the manganate and permanganate of potassa on the other, no relation of isomorphism has been detected between the salts of the acids of ruthenium and those of manganese. The rutheniate of potassa is hydrated, while the manganate, like the sulphate, is anhydrous.—On the monthly charts of the North Atlantic currents, by M. Simart. Continuing the work of Commander Brault, the author has prepared two series of charts (diagrams and results) based on 60,400 observations obtained from the records of the French Admiralty and various other sources. The charts of results give the currents most likely to be met with from month to month all the year round, while the diagrams indicate the currents that may possibly be met, especially near the coasts, where they present the greatest dangers to seafarers.—Origin of the aurora borealis, by M. Jean Luvini. This phenomenon is regarded as analogous to the discharge of electricity in thunderstorms, the only difference consisting in their different degrees of intensity. Both are attributed to the friction of particles of water and ice and occasionally of other minute bodies drawn by the aërial currents into the higher atmospheric regions and disseminated over the terrestrial atmosphere some hundred miles thick. The northern lights are most frequent about the pole, where the air abounds most in icy particles and where the field of terrestrial magnetism is most intense.—Observations of the new planet Palisa (279) made at the Observatory of Algiers with the 0·50m. telescope, by MM. Rambaud and Sy. These observations, which include the positions of two comparison stars and the apparent positions of the planet, cover the period from May 18 to May 22.—Observations of the planet Borelly (278) made at the Observatory of Marseilles with the 0·26m. Eichens equatorial, by M. Esmiol. During these observations, continued from May 13 to May 21, the planet appeared to be of magnitude 11·5.—On the supernumerary arcs accompanying the rainbow, by M. Boitel. The position of these arcs, as determined by Airy on the principles of diffraction, and generally accepted as absolute, is shown to be merely a first approximation, which the author hopes soon to supplement by more accurate calculations.—Researches on the application of the rotatory power to the study of the compounds formed by the action of the neutral tungstates of soda and potassa on the solutions of tartaric acid, by M. D. Gernez. From these experiments it appears that the neutral tungstates of soda and potassa behave analogously in their action on tartaric acid.—On the sesquisulphide of rhodium, by M. E. Leidié. The author describes the methods of preparation of this substance and of the double sulphides both by the wet and dry processes.—On two isomerous naphthoquinoleins, by M. Alphonse Combes. The only terms hitherto known of these rare compounds are those obtained by Skraup by making glycerine act on the naphthylamines in the presence of sulphuric acid. The author here describes two new terms of the series, as well as a means by which several others may also be obtained.—On a new species of Coregonus, by M. Victor Fatio. To this species, discovered in the French Lake Bourget, the author has given the name of *Coregonus Bezola*. It is a well-defined local variety.—On the germination of *Anemone apennina*, by M. Ed. de Janczewski. This species presents in its germination a curious and most remarkable anomaly, differing in this respect from all other dicotyledonous plants.—On the bust of a woman carved in the root of an equine tooth, by M. Ed. Piette. This specimen of prehistoric art, recently discovered by the author in the cave of Mas d'Azil, Ariège, presents several points of interest to the anthropologist. Owing to the contracted space, the artist had to suppress shoulders and arms, merely suggesting the outlines of the sides. But the pendant breasts are well executed, and the profile of the face carefully delineated. The nose is large and rounded, the lips thick, the chin retreating like that of the Naulette jaw, but the forehead is high and not receding like that of the Neanderthal skull. It is the third extant representation of a woman of the Quaternary period, the two others being M. de Vibraye's "Venus" and the "Reindeer Woman," both from Laugerie-Basse.

Berlin.

Physical Society, May 18.—Prof. du Bois-Reymond, President, in the chair.—Dr. Dieterici gave an account of his experiments on the determination of the latent heat of evaporation of water at 0° C. Regnault's experiments on the latent heat of evaporation of water were made at higher temperatures, and had led to the construction of a formula according to which the latent heat of evaporation at 0° C. must be 607 units of heat. The speaker, using an ice-calorimeter, had made a direct determination of this value. A glass tube, with its lower end blown out into a bulb and filled with water, was immersed in the chamber of the calorimeter, the upper end of the tube being connected with an air-pump, and a small column of sulphuric acid being interposed between the pump and the tube. As soon as the apparatus had assumed a perfectly uniform temperature, a vacuum was produced by the air-pump, whereupon the water in the tube evaporated, taking up from the calorimeter the heat necessary for its evaporation. Values were obtained from a series of ten experiments, which differed from each other by not more than $\frac{3}{4}$ per cent. In order to meet the objection which might be raised—namely, that the temperature at which the evaporation took place was not 0° C.,—Dr. Dieterici repeated his experiments, using a platinum instead of a glass tube. The values obtained in this set of experiments only differed by ¼ per cent. The mean of the two sets of experiments was identical, and the final outcome of the whole research was that the latent heat of evaporation of water at 0° C. is 596·4 thermal units. The speaker then discussed fully the theoretical significance of the above results, and described an experiment he had made in order to determine the latent heat of evaporation of ice at 0° C. The method employed was the same as above, but it did not yield the value which was theoretically expected, which should have been equal to the sum of the latent heat of evaporation of water and of the latent heat of fusion of ice. The cause of the divergence was due to the fact that the ice used was not clear and crystalline, but milky and opaque. Dr. Dieterici intends to repeat these determinations next winter.—Prof. von Bezold gave an account of a paper which he had recently read before the Berlin Academy on the thermodynamics of the atmosphere. Recent meteorology has derived very considerable benefit from the application of thermodynamics to events taking place in the atmosphere; but up to the present time all the researches had only dealt with adiabatic and reversible processes. As a matter of fact, these processes are neither adiabatic nor reversible, since, when the air is cooled, its aqueous vapour is condensed, and the water thus formed falls as either rain, hail, or snow. If both these facts are taken into account, the calculations involved thereby become so complicated that Prof. von Bezold was only enabled to proceed to the application of thermodynamics to the processes which really take place in the atmosphere by employing an artifice; the latter consisted of the graphic method introduced by Clapeyron with such marked success as a technical method. For this purpose the consideration starts with the assumption that the air is dry, in which case the equation for its condition is given in terms of its volume, pressure, and temperature, and can be represented by plane co-ordinates. The variable amount of aqueous vapour in the air is then treated as a further variable in the third co-ordinate, in such a way that for any given amount of aqueous vapour in the air a new co-ordinate representing the change in condition of the air is obtained. When, on cooling, a portion of this aqueous vapour is condensed, the curve representing the change of condition passes over from one plane to the other, pursuing its further course in the latter plane. In this way it becomes possible, as the speaker fully showed, to treat non-reversible and pseudo-adiabatic processes theoretically, according to the laws of thermodynamics. It can thus be shown in the case of the Föhn and of cyclones, as well as of anti-cyclones, which are not reversible but reversed processes, that the theoretical considerations lead to results which are found to be confirmed by experience. Thus, according to theory, in an anticyclone occurring in winter, there should be a rise of temperature at some height above the earth, a fact which is now observed at all meteorological stations at high altitudes.

Physiological Society, May 25.—Prof. du Bois-Reymond, President, in the chair.—Dr. Weyl gave an account of the results of his further researches on silk. Among the products of decomposition of albumen and proteid substances, one is known as a snowy crystalline body, which is considered to be leucin, and is generally regarded as being also a product of the decomposition of silk. Since this substance may be obtained in large quantities by the decomposition of silk, the speaker had prepared it from this source and analyzed it, and has come to the conclusion that it is not leucin (amidocaproic acid), but rather another amidated acid—namely, alanin. Of the two possible isomers of alanin, it is α-alanin which is obtained by the decomposition of silk. Dr. Weyl laid stress on the fact that Schützenberger had also concluded that alanin and glycocol occur among the products of decomposition of silk, notwithstanding that, during his elaborate and careful researches on proteids, he employed a method which is as unfavourable as can be imagined for determining this point: this result is now confirmed by the speaker's researches. Schützenberger's further supposition, that an amido-acid of the acrylic series can be prepared from silk, was not supported by Dr. Weyl's analyses.—The same speaker further communicated the results of his researches on the physiological action of anthrarobin and chrysarobin, which have recently been largely used in medical practice. These two substances, whose chemical constitution and relationship to alizarin and anthracene have been made clear by Liebermann, are largely used as reducing-bodies, especially in skin diseases. Dr. Weyl endeavoured, by means of experiments on rabbits and dogs, and on himself, to determine the physi logical action of anthrarobin, and found that it possesses absolute no action on the living organism, even when taken by the mou in relatively large doses, or injected subcutaneously. It cou be detected in an unaltered condition in the urine, so that th substance, notwithstanding that it possesses a great affinity f oxygen, passes through the body without being oxidize Chrysarobin, on the other hand, has a very different action notwithstanding its close relationship to the non-injurio anthrarobin, it has a powerfully poisonous action, so that a experiments made with it were of necessity confined to rabbi and dogs. The speaker was unable to confirm the statemen of several authors that chrysarobin reappears in the urine chrysophanic acid. It is rather his opinion that chrysarobin first excreted in an unaltered condition, and only subsequentl undergoes a change into chrysophanic acid. It remains f further experiments to clear up this point.—Prof. Gad spoke o the phosporescent moss *Schistostega osmundacea*, which he ha been for some time cultivating, and which he exhibited. thorough investigation of the phosphorescent powers of this pla promises a rich harvest of facts from a physical point of view it is well known, on the basis of morphological research, that th phosphorescence is due to a reflection of the incident light.

In the report of the Berlin Meteorological Society, May (p. 119), the expression "a spring-vane," should have been " vane made of feathers."

BOOKS, PAMPHLETS, and SERIALS RECEIVE

Travels in Arabia Deserta, 2 vols.: C. M. Doughty (Cambridge Press).—Modern Science in Bible Lands: Sir J. W. Dawson (Hodder and Stoug ton).—Catalog der Conchylien-Sammlung, Lief. 7: Fr. Paetel (Berlin).—Charts showing the Mean Barometrical Pressure over the Atlantic, India and Pacific Oceans (Eyre and Spottiswoode).—Inorganic Chemistry, 2n edition: by Kolbe, translated and edited by Humpidge (Longmans).—Longmans' Test Cards in Mechanics, Stages I., II., III. (Longman —Flora of North America (the Gamopetalæ): Dr. Asa Gray (Smithsoni Institution, Washington).—La Biologie Végétale: P. Vuillemin (Ba lière, Paris).—Applications of Dynamics to Physics and Chemistry J. J. Thomson (Macmillan).—Lingua: G. J. Henderson (Trübner).

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THURSDAY, JUNE 14, 1888.

THE BOYS' "YARRELL."

An Illustrated Manual of British Birds. By Howard Saunders, F.L.S., F.Z.S. Part I., April 1888. (London: Gurney and Jackson.)

A BOYS' "Yarrell" is a book that many ornithologists have long wished to see. More than six years ago a scheme for producing one was thought out, and Mr. Howard Saunders was invited, and consented, to aid in its production. *Dîs aliter visum*—the scheme fell through for the time; but now the proposed coadjutor has been favoured by fortune, and has by himself been able to put it into operation. He is to be heartily congratulated accordingly, and not only he, but scores if not hundreds of boys—to whom the work, just begun, will afford no less delight than good instruction.

From the moment when the first part of the original edition of Yarrell's "British Birds" appeared—now more than fifty years ago—it was seen that a new era in the study had dawned. The author had no other scientific training than that which, amid the turmoil of business, he had been able to acquire for and by himself; but he knew the value of scientific work, and having an uncommon amount of common-sense, he knew that the introduction of too much of it into his books would render them indigestible to the unscientific public of those days. Hence his "British Fishes" and "British Birds," though intentionally popular, are permeated by an only half-concealed thread of scientific thought, which, without its interfering with their readableness, the true aspirant could catch, and guide himself thereby to a higher level. From the publishers' point of view, these works were successful beyond expectation; but they had one great drawback. They were abundantly illustrated, and therefore necessarily expensive. This put them, and especially the "British Birds," to which the theme of the present notice relates, out of the reach of almost all but those in easy circumstances. Scarcely a school-boy, however much he might covet a copy, such as he might happen to see in some more favoured hands, could out of his pocket-money afford to buy a "Yarrell"; and even though the price of the later editions has been somewhat reduced, there is not one of them that would be within his means. Moreover, they contained a good deal more than he cared to know. The sort of information he wanted was, let us say, whether the bird he saw on the top of a hedge was a Cirl-Bunting or not; or whether, as the gamekeeper had told him, the Sparrow-Hawk was "that artful to turn hisself" into a Cuckoo in the spring; or, again, whether the bird that had suddenly risen as he walked along the brook-side was a Summer-Snipe or a Sandpiper, and what was the difference, if any, between them. Of course he would also like to know how the Wild Duck got her ducklings down into the water from the hollow tree in which he had found her nest, and what became of the Swallows in winter, and the Fieldfares in summer. Yarrell's work gave all this in the best way possible, but it added a great deal more that the school-boy did not care a button for. It told him of "orders" and "genera," and gave "characters"—which to him were as hard as Greek verbs. Every now and then there was a bit of anatomy; but that was to the good, for your inquiring school-boy rather likes making a rough dissection, and is pleased to find that the windpipe of a drake differs from that of a duck. But then, again, there was a good deal of "distribution," and he was bored by recollections of dreary geographical lessons,[1] and was not interested at learning that such or such a bird was found in some country with a long name not easy to pronounce.

With all these merits and defects, Yarrell's work, in all its editions, undoubtedly held the field, and there grew up more than one imitation of it—specious, pretentious, and misleading. One of these plagiarisms has been "embellished" (that, at least, is the word used by the publisher) with coloured figures; but unfortunately, among people who knew no better, as well as among people who ought to have known better, they have met with a success hardly inferior to the work from which they have been ingeniously and shamelessly "cribbed." This shows the exceeding popularity of the subject; but it is disgusting to find in nearly every school-library one or more of these piratical works—generally instead of the good, though more costly, original, though sometimes on the same shelf with it, as if the two were of equal authority. The common excuse is the high price of the "Yarrell," but no excuse can justify the corruption of youthful minds by ignorance, twaddle, and inaccuracy. Better hunger than poison—if both be deadly, one will kill more quickly than the other; and, since while there is life there is hope, the chance of proper aliment being timeously supplied exists in the former case, but in the latter even the antidote, if such a thing there be, may be exhibited in vain.

The work now begun by Mr. Saunders ought to abolish for ever the excuse just spoken of. This "Manual of British Birds" is cheap, marvellously cheap, and as fully illustrated[2] as ordinary boys can wish. That the design he has followed is certain to have a good effect few in a position to give an opinion can doubt, and his treatment of it is satisfactory, considering the enormous difficulties in the way. When one thinks of the vast amount that has been written about British birds by men who have written from their own knowledge—leaving wholly aside the pilferers above complained of—it will be evident that no ordinary discrimination is needed to extract the essence and serve it up on an octavo page and a half, or perhaps a few lines more, for this is practically the amount of letterpress at Mr. Saunders's disposal, the top of the first page being reserved for a woodcut of the species. But Mr. Saunders has been a "Zoological Recorder," and therefore has learnt the art of "boiling down." Occasionally there is a tendency to "straggle"—a favourite word of his, and one that is seldom apposite—and if verbal criticism be allowable, a protest might be made against "segregate" (more than once used) where *separate* is meant. But generally Mr. Saunders sets an admirable example in the matter of language, and one that all ornithological writers might well follow, since some of the more profuse of them have lately banished grammar and etymology to the outer planets, while style is a quality unthought of.

[1] All the same, the school-boy of forty or fifty years ago did learn *some* geography—a kind of learning that has lately been almost wholly dropped.

[2] The illustrations consist mostly of reproductions of the well-known "Yarrell" woodcuts.

There is one drawback in a work of this kind, and to some extent it is perhaps unavoidable. Mr. Saunders, following literally the scheme originally laid before him, and disregarding the exceptions therein provided for, devotes two pages to each species of bird. Now it is evident that this Procrustean plan cuts off many details of the greatest interest from what might be said of some species, and compels the story of comparatively uninteresting species to be stretched out. Among these last must of course be reckoned those which have only a few times made their way to the British Islands, and have scarcely a claim to be called "British" birds. In a work like this, Mr. Saunders, with the justly-earned reputation he possesses, might well have taken a new departure; but, unfortunately, four out of the twenty species included in his first part come under this condemnation. Their room, where every line is precious, would have been better than their company, and their introduction gives the beginner a wholly mistaken notion of the British fauna. Figures of the same absolute dimensions are often useful for certain purposes of comparison; but to treat the Rock-Thrush and three exotic species of Wheatears on an equality with our real denizens, that have inhabited these islands longer probably than any human beings, is to present a piece of distorted perspective. The practice was excusable in old days, and those that had to tread the ancient tracks were compelled to follow it; but here was an opportunity of striking out a fresh line. Of course there is great difficulty in drawing that line, for it must be drawn arbitrarily, but an arbitary line would be better than none. On a wharf a post-and-rail fence, or a suspended chain, may be placed almost at random, and people may say that it should have been a few inches nearer to, or further from, the brink, but if it saves them from falling into the water, few persons will not recognize the service it does.

It is a pity that almost the first word in this excellent book is one to which exception must be taken. Mr. Saunders has brought back the vulgar name of "Missel-Thrush," which some people fondly hoped had been for ever abrogated—as being either a corrupt abbreviation or wholly without meaning. Of course he can cite Willughby and a long string of subsequent authorities in his favour; but the "*auctorum plurimorum*" principle is directly opposed to sound scientific sense; and if Mr. Saunders will look up Willughby's predecessors—Charleton and Merrett—he will find that they do not admit the solecism. In a work of this kind, which cannot fail to have a great effect upon the rising generation of ornithologists, the least tendency to return to exploded errors is to be deplored. So much for criticism of the part which is now before me: I gladly say of the whole book—*Floreat*. ALFRED NEWTON.

THEORY AND USE OF A PHYSICAL BALANCE.

Theory and Use of a Physical Balance. By James Walker, M.A., Demonstrator at the Clarendon Laboratory. (Oxford: Clarendon Press, 1887.)

THE author states that this publication was originally intended as a chapter of a book on practical physics for the use of students at the Clarendon Laboratory, but that he proposes to publish each chapter when ready, without waiting for the completion of the work. This method certainly has some advantages both from the author's and the student's points of view. The practical study of physics, like that of all other sciences, and perhaps even to a greater extent than any other, is rapidly becoming specialized, with the necessary consequences that while each subdivision is expanding and becoming weighted with more details and technicalities, many diligent workers on one part of the subject are indifferent to the methods and appliances used in other branches. The numerous army of students in electricity and magnetism may take, for example, but a very superficial interest in the experimental side of acoustics or optics. At the same time it may be open to question whether it is advisable to break a work up into comparatively small fragments, as appears to be the intention in the present case. Like all other matter, the subject may lose in cohesion by being presented in too fine a state of division.

The instalment now issued gives a detailed and precise description of one of Oertling's balances used in the Clarendon Laboratory. The very clear explanation of the mechanism is assisted by three plates, one a photograph showing a general view, and the others line drawings of the various parts. Any want of clearness and definition in the photograph, which is not a particularly happy example of a collotype, is amply atoned for in the sectional diagrams.

Details are given of the methods adopted by the manufacturers to insure the accurate adjustment of the knife-edges, to test for their parallelism, for their being in the same plane, for the equality of the lengths of the arms, and of the masses of the pans, &c. The expression for the sensibility of the balance is determined from the general equations of equilibrium, and practical instructions are given with the necessary formulæ for performing some half-dozen of the usual physical operations with the balance, such as the determination of density of bodies heavier and lighter than water, of bodies in small pieces, &c.

In a thorough and somewhat elaborate investigation, which seems hardly suitable for a work intended as a handbook for a student entering on a course of laboratory instruction, the writer discusses the equations of motion of a balance, and shows that the method of determining the position of equilibrium from the amplitude of the oscillations on either side of the zero is not rigidly correct, since the beam with its adjuncts have not a simple definite period of vibration like a pendulum. The reassuring result is, however, arrived at, that the errors introduced are of a vanishing order, if the masses in the pans remain constant during a set of weighings.

Borda's method of counterpoising to eliminate errors of the instrument is recommended according to the usual practice, a mass heavier than the substance to be weighed being placed in the left-hand pan, while the substance and known masses are placed in the right-hand pan to bring the beam into an observed position of equilibrium. This procedure has advantages over the more tedious and less cleanly plan of exactly counterbalancing the substance with shot and fine sand, &c.

In allowing for the supporting force of the atmosphere, the author assumes that the average amount of moisture in the air may be taken as two thirds of the maximum possible. This seems a very high value for a closed and artificially heated room; certainly much in excess for air in a balance case which contains any substance, such as chloride of calcium, for absorbing the moisture. Perhaps it is the uncertainty as to the condition of the air thus artificially treated which causes the author to omit any reference to any of the hygroscopic substances usually employed.

The standard masses used at the Clarendon Laboratory are stated to be marked with their apparent value in air at 10° C. and 76 cm. of mercury. It is the usual custom, we believe, to mark the absolute value of the masses. For work not requiring the most refined precautions, the convenience of weights marked with their apparent value is obvious: no correction need be made for the supporting force of the air on the weights; but if that accuracy is considered sufficient, it seems an unnecessary refinement to complicate the formulæ by introducing a correction for the difference between the temperature of the air and of the water in which the substance is weighed.

The work is very clearly written and admirably printed, and will doubtless form, when completed (and we hope this will not be at a distant date), a valuable addition to the text-books on this subject. We have only noticed two mistakes in the text—the omission of the small over-weight w at line 23, p. 12, and of the length of the arm, a, at line 10, p. 16; but neither of these omissions affects the final results. The average student would, however, probably prefer that a larger portion of the space should be devoted to the more practical side of the subject, to hints and precautions to be taken in various operations; those given are very good, but they might with advantage have been extended. It would also, we think, be useful to indicate by numerical examples the order of magnitude of the various corrections to be applied, so that a student may judge what corrections may be safely omitted in the particular observation on which he is engaged. Some of the space given to the description of the instrument might, we think, have been more profitably devoted to a general account of other types of construction. Only a passing reference is made to the "short-beam" balance, and other modifications of the physical balance are not alluded to.

THE FLORA OF WEST YORKSHIRE.

The Flora of West Yorkshire, with a Sketch of the Climatology and Lithology in connection therewith. By Frederic Arnold Lees. 8vo, pp. 843, with a Map. (London: Lovell Reeve and Co., 1888.)

IT is just a quarter of a century since John Gilbert Baker's excellent book on the botany, geology, climate, and physical geography of North Yorkshire appeared,[1] and the present volume, devoted to West Yorkshire, is avowedly moulded on that model. Since then, English county and other local "floras" have become very numerous—many of them well executed, others indifferently. We do not mean to say that Mr. Baker was the originator of local "floras," for this branch of botanical literature early took root in this country, and has perhaps attained a development unknown elsewhere. Interesting among the earlier of such publications is John Ray's "Catalogus Plantarum circa Cantabrigiam nascentium," which dates (1660) nearly a hundred years before the first edition of Linnæus's "Species Plantarum." It is interesting alike for its botany and its botanical history. But the importance of exactitude in recording the localities of plants was not thoroughly realized by amateur botanists until they were stimulated thereto by the methodical and conscientious, though somewhat discursive, phytogeographical writings of the late Hewett Cottrell Watson. Now, thanks to the exertions of the competent few, English amateur botanists are so thoroughly educated in geographical botany at the beginning of their studies, that the careless, or, what is worse, the unprincipled, recorder of assumed localities of the rarer plants, is at once discovered and exposed. The latitudinal and altitudinal range of each species is now known with such accuracy that any new record outside of the known limits is at once scrutinized and tested, and only accepted on the best authority. It is a question, however, whether this sort of thing is not being overdone.

[1] We understand that a new edition is in preparation.

Mr. Lees expresses a hope that the acknowledged adoption of Baker's admirable method of inquiry and statement will not be regarded as too servile. We think it will not; and had the imitation been carried a little further, and the briefer and more condensed style of the pattern followed, it would have been a distinct advantage, because it would have reduced the size of the book without in the least impairing its value. The area of West Yorkshire is about 2750 square miles, and this is divided into ten drainage districts, varying in size from 30 square miles (Mersey tributaries) to 570 square miles—Don with Dearne; and the stations, or a selection of stations, in which a given plant is known to occur in each of these districts are given—in many instances, in what we should regard as excessive detail. Whether it would not have been better to amalgamate some of the districts, instead of adhering so closely to a principle as to maintain a very small portion of a drainage area as a distinct district, we will not pretend to decide; but there is no doubt it would have resulted in a considerable saving of space, which might have been profitably devoted to a brief exposition of the total geographical area of each genus and species.

With regard to the manner in which Mr. Lees has executed the task he undertook, there is ample evidence that he has spared no pains; and we have means of knowing that those most concerned are very grateful for such a store of well-sifted records. Nevertheless, this work, which forms the second volume of the botanical series of the Transactions of the Yorkshire Naturalists' Union, has its peculiarities, chiefly of a literary kind. On opening the book, we happened to light on the "Foreword," first of all, and we naturally expected that our author was a purist who wrote only Saxon English; but we soon discovered that uncommon words, irrespective of their origin, are dragged into use, and sometimes so piled up as to obscure not a little the meaning of the somewhat inflated sentences. However, this peculiarity is not carried so far as to constitute a serious defect in the work, and may be passed over with this allusion.

Very interesting are two introductory chapters on the climatology and lithology of West Yorkshire, specially in relation to plant-life, which many persons would doubtless gladly possess, apart from the enumeration of the plants of the region. In the list of pelophilous (clay-loving) plants, we note *Spiræa Filipendula*, a plant so strictly associated with chalk in the south of England, that we are surprised to find it among those characteristic of clay and mud-soils. Perhaps it was a slip of the pen for *S. Ulmaria?*

The total number of species of vascular plants enumerated is 1042, whereof 995 are phanerogams, which is equal to the whole phanerogamic flora of New Zealand, even after allowing 40 off for "critical species" of various genera. On the other hand, the vascular cryptogams of West Yorkshire are only 47 against 138 in New Zealand, of which 120 are ferns. Fortunately for the New Zealanders, and Australians too, for that matter, they are free from the "horse-tails," which are such terrible pests to farmers in some districts of this country; but seven species are indigenous in West Yorkshire.

Cellular cryptogams are also included in Lees's "Flora," and occupy about 250 pages. The enumerations of some of the groups are exceedingly imperfect —imperfect in consequence of their not having been investigated—and it would have been much more convenient for the majority of workers had this class been reserved for a separate volume. W. B. H.

OUR BOOK SHELF.

A Manual of Practical Assaying. By John Mitchell, F.C.S. Edited by William Crookes, F.R.S. Sixth Edition. (London: Longmans, Green, and Co., 1888.)

MITCHELL'S "Assaying" is so well known to all whom the subject concerns, that it is hardly necessary at present to do more than announce the appearance of a new edition. In this edition, as Mr. Crookes explains, much new matter has been introduced, and matter which had become obsolete has been omitted. Among the more important of the additions are descriptions of the "automatic sampling-machine," invented by Mr. D. W. Brunton; many new gas-furnaces and burners for the laboratory, devised by Mr. Fletcher, Messrs. J. J. Griffin, and others; new blow-pipe reagents and operations; new processes, dosimetric, volumetric, and calorimetric, for the partial and complete assay of iron ores, iron, steel, spiegeleisen, &c. In the copper assay the American system of fire assay is here, for the first time in this country, fully described. In the assay of silver, the action of bismuth on the ductility of this metal has received adequate attention. Much has been added about gold ores; and improved modes of assaying the precious metal and detecting it in poor ores are given. The number of woodcuts has been increased from 188 in the last edition to 201 in the present edition.

Asbestos, its Production and Use. By Robert H. Jones. (London: Crosby Lockwood and Son, 1888.)

THIS little book, written in epistolary style, though possessing little or no scientific value, contains an interesting account of the "asbestos" mines of Canada, and of the methods pursued in working the mineral in that country. It is precisely ten years since the first Canadian chrysotile mines were opened, and the annual yield at the present time appears to be more than 2000 tons, so that the new locality is rapidly becoming an important rival to the older and better-known asbestos mines of the Italian Alps. The author gives a brief description of the mode of occurrence of the mineral in the Serpentine belt which traverses the provinces of Megantic and Beauce in Quebec, and prophesies a wider development of this industry in the future; he does not, however, supply any such details as would suggest either the origin or the probable extent of the Canadian "asbestos," and the book contains no original observations of any scientific importance. The author does not appear to be aware of the difference between asbestos and chrysotile. The pages most interesting to general readers are those which contain an account of the latest uses to which the mineral is now applied; among which may be mentioned fire-balloons, theatre-curtains, fire-proof paint, filters, and letter-paper.

Industrial Instruction. By Robert Seidel. Translated by Margaret K. Smith. (Boston: D. C. Heath and Co., 1888.)

IN the years 1882 and 1884 industrial instruction formed the subject of much discussion in the Synod of the Canton of Zürich. Herr Seidel, who had long devoted earnest attention to the question, carefully answered all the objections to industrial education which were raised in the course of these debates; and the substance of his replies is embodied in the work translated in the present volume. If there is still anyone who has doubts as to the value of manual training in schools, he would profit largely by reading this little book. Herr Seidel's main point is that such training is absolutely essential in the interests of true education, and in working out this view he displays great intellectual resource and a thorough appreciation of the laws of mental growth. He is not afraid that when the need for this "new departure" is generally recognized the task imposed upon teachers will be beyond their capacities. "The training of teachers for industrial instruction," he says, "offers no difficulty, and will not (as has been asserted) by any means involve the necessity for two kinds of teachers. The teacher can very well master the new task, and if his prejudice has disappeared, will very gladly undertake it. Probably the imparting of industrial instruction will become a favourite employment of the teacher, because the change refreshes and the labour gladdens him."

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Electric Fishes in the River Uruguay.

IN Sir Horace Rumbold's "Great Silver River" (London, 1887), the author, when on the Upper Uruguay above Uruguayana, speaks of a "kind of Electric Eel (*Gymnotus*) called here *Rayo* or Lightning, of the effects of contact with which, very curious and unrelateable stories are told."

The range of the *Gymnoti* is usually supposed to be confined to the waters of the Orinoco and Amazons and their affluents, so that it would be very desirable to ascertain what this supposed electric fish of the Rio Uruguay really is. Perhaps some of your readers in the Argentine Republic may be able to assist us in solving this problem, which would be best done by the transmission of specimens of the fish in question to the British Museum. P. L. SCLATER.

3 Hanover Square, London, W., June 8.

The Salt Industry in the United States.

MR. WARD in his letter to NATURE (May 10, p. 29), respecting the salt industry in the United States, makes no mention of the important and numerous contributions to the literature of that subject by Dr. Charles A. Goessmann, at the present time Director of the Massachusetts Agricultural Experiment Station, but formerly, from 1861 to 1869, chemist to the Onondaga Salt Company, at Syracuse, N.Y. While filling that position he investigated very thoroughly the salt deposits of New York, Michigan, Goderich, Canada, and Petit Anse Island, Louisiana, and his published reports and memoirs (some twenty in number) upon the salines, brines, and mineral springs of the country form, for the period which they cover, a very complete and valuable record of the salt industry in the United States.

Amherst, Mass., May 26. F. TUCKERMAN.

Prof. Greenhill on "Kinematics and Dynamics."

MAY I ask space for a few short comments on Prof. Greenhill's letter in your issue of May 17 (p. 54), so far as it is directed against myself.

(1) The "circumlocutions" referred to are not of my devising, but are current phrases which involve no ambiguity and are useful for avoiding frequent repetition.

(2) It is not true that "although such words as 'a force equal to the weight of the mass of 10 pound weights' do not occur in Prof. MacGregor's book, they are strictly derived from his definitions." According to my definitions, it is the body itself which has weight, not its mass; and the above phrase is therefore meaningless.

(3) Prof. Greenhill has not cited a single instance to justify his charge that I am at variance with my own definition of the weight of a body in the majority of the subsequent examples.

(4) He now seems to admit that in my hydrostatical equations pressure may be expressed in pounds on the square foot, but to claim that it can be done only in a clumsy manner. There is doubtless a certain clumsiness, but it seems to me to be due to the employment of a clumsy set of units.

(5) Your reviewer still demands that I should give the dimensions of the earth, not in terms of the actual metre, but in terms of what the original designers of the metre intended it to be; but he gives no reason for this strange demand.

(6) If the knot is a unit of velocity, the term *knots per hour* is of course redundant. I have always considered it an abbreviation, but have no means at hand of settling the point.

(7) Prof. Greenhill tacitly admits that he was in error in accusing me of misusing the term *elongation*.

(8) He makes no attempt to substantiate his statement that my equations of energy were not expressed in proper form.

(9) He does not answer my question as to which of the most recent treatises on dynamics my treatment of units shows me to have read without profit and discrimination.

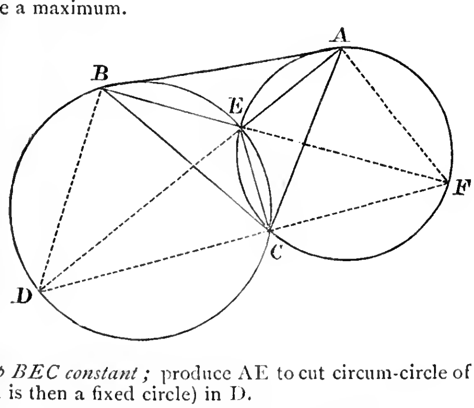
Edinburgh, May 31. J. G. MACGREGOR.

Further Use of Ptolemy's Theorem (Euclid, VI. D.) for a Problem in Maxima and Minima.

To find E within ΔABC such that

$$AE \sin BEC + BE \sin CEA + CE \sin AEB$$

shall be a maximum.



Keep BEC constant; produce AE to cut circum-circle of BEC (which is then a fixed circle) in D.

Then $\sin BEA = \sin BED = \sin BCD,$

$\sin AEC = \sin CED = \sin CBD,$

and $\sin BEC = \sin BDC;$

$$\therefore \frac{BC}{\sin BEC} = \frac{CD}{\sin AEC} = \frac{DB}{\sin AEB};$$

$$\therefore AE \sin BEC + BE \sin CEA + CE \sin AEB$$

is proportional to

$$AE \cdot BC + BE \cdot CD + CE \cdot BD,$$

and therefore to

$$AE \cdot BC + ED \cdot BC. \quad \text{(Eu. VI. D),}$$

which

$$= AD \cdot BC.$$

For a maximum AE passes through centre of circum-circle of BEC.

Similarly BE passes through centre of circum-circle of CEA. Let it cut it again in F.

$\angle BCE = \angle BDE,$

$= \angle BFA$ in same segment of circle through F, A, B, D,

$= \angle ACE.$

Similarly

AE, BE bisect $\angle$ CAB, ABC.

$\therefore$ E is the in-centre of ΔABC.

Bedford. E. M. LANGLEY.

Davis's "Biology."

IF I may argue from the contents of Mr. Davis's book, he should be a good judge of what constitutes "falling into a common mistake," and yet I cannot accept his opinion as to my having accomplished this feat. I have refrained from enumerating the common mistakes which his little book contains, but I am not prepared to allow him to lay down the law as to educational methods. In my opinion it is a grievous error to present any subject of study to University students under two aspects, that of "pass" and that of "honours." Whatever is worth doing at all (in academic exercises) is worth doing well, and no regulations sanctioned by any University Senate—however philanthropic, incompetent, and imperial—can make the perennial iteration of the statements in a cram-book concerning six plants and six animals a satisfactory substitute for the study of zoological and botanical science, or anything but a pernicious torturing of the youthful mind. THE REVIEWER.

M. FAYE'S THEORY OF STORMS.[1]

ACCORDING to M. Faye, "There exist in meteorology two theories diametrically opposed—one which considers air-whirls round a vertical axis, including cyclones, typhoons, tornadoes, and waterspouts, to originate in the upper currents of the atmosphere; and the other which considers each of these as the effect of a local rarefaction, giving rise at the surface of the ground, in an atmosphere in a more or less unstable condition, to an ascending current of air, which borrows a gyratory tendency from the rotation of the ground itself." Such is the opening sentence of the pamphlet before us, which embodies a *résumé* of M. Faye's discussions in the French Academy with those who do not accept his peculiar views on the generation of atmospheric disturbances.

M. Faye upholds the former theory with that incisive vigour which characterizes our Gallic neighbours, and attacks the meteorologists with whose writings he is acquainted, beginning with poor Franklin and ending with Sprung in 1885, without mercy, but at the same time without the smallest reference to physics apart from mechanics.

Before pointing out some of the grave errors of fact, as well as theory, into which we deem M. Faye to have fallen, it may be as well to see if we cannot attempt a reconciliation between these two opposite views, which are considered to be prevalent.

To avoid mixing up tornadoes and cyclones, which we hold to be, if not generically, at all events specifically, distinct, let us first consider the former alone. The point

[1] "Sur les Tempêtes." Par M. H. Faye. (Paris: Gauthier-Villars, 1887.)

that M. Faye insists upon all through is, that these arise solely through inequalities in the upper currents, causing gyration round a vertical axis, which, like a river eddy, is propagated from above downwards, by a *descending motion of the air*. M. Colladon, referring to M. Faye's view, describes the supposed action as "un mouvement tourbillonnaire aérien constituant à son intérieur une trombe *aspiratrice* à mouvement *descendant*." M. Faye, therefore, postulates two points: (1) that the movement commences above; (2) that it is propagated downwards by a descending motion, accompanied by gyration round a vertical axis. The opposite theory, as presented by M. Faye, is the exact inverse of this, since it makes the action (1) commence at the earth's surface; (2) propagate itself upwards; and (3) borrow its gyration from that of the earth. Here, however, we find ourselves distinctly at issue with M. Faye, for we do not believe that the leaders of modern meteorology entertain any such view as the latter. The surface of the earth is the most unlikely birthplace for a tornado, whirlwind, or waterspout. In order to maintain an ascending current, the air must be nearly saturated, and this will generally occur only in and near the lowest cloud stratum. The vertical temperature gradient and disturbances which start the action, will likewise operate most effectively at this level, so that all the conditions which unite to cause a tornado will tend to commence at some distance above the earth's surface. On the question of level, therefore, we may invite M. Faye to agree with us. Then comes the question of the downward propagation.

The entire gist of the question appears to us to lie in this downward propagation. The physical theory developed by Ferrel and Sprung makes the action commence in a slight upward motion in unstable air, due to a temperature inequality or some other cause, the only other condition being a gentle gyratory motion relative to some central point, which is never wanting in a cyclonic area. Once the motion is started, and the air which feeds it is nearly or quite saturated, the action will go on and be propagated downwards, not by a descent of the *air*, but by the transference of the physical conditions which favour the continuance and maximum development of the "courant ascendant." The increasing rapidity of gyration of the air as it approaches the axis, however gentle it may be at starting, only allows it to partially feed the initial and continually reproduced vacuum, which is thus compelled to draw its supplies chiefly from the non-gyrating air at the lower end of the aërial shaft. As this is drawn upwards, the centrally aspired surrounding air is made to gyrate more rapidly (partly by the friction of the superjacent rotating layer), and thus the gyratory and other *conditions* are propagated downwards until a balance is struck between supply and demand.

The theory thus sketched may be termed the modern theory of aspiration as applied to tornadoes, and will, we venture to think, be found to meet all M. Faye's objections to the first crude notions which prevailed in past years from a study of a few isolated surface conditions.

Before proceeding to notice the objections which M. Faye brings against the existence of either an upward current or any sort of aspiration in tornadoes, we must first touch upon the cognate question of cyclone generation, which he explains on the same principles;[1] and here, without attempting to give any review of the modern theory, which involves as a primary condition a horizontal temperature anomaly over a considerable area, we may observe that the two main objections brought by M. Faye against the ordinary view of their formation are, (1) that it assumes the existence of centripetal currents, and hence aspiration towards their axes; (2) that it gives no explanation of their movements over the earth's surface.

[1] There is no real connecting link between the two, *i.e.* the smaller cyclones do not begin where the larger tornadoes leave off. The average size of 600 tornadoes in the United States was found to be 1085 *yards*. The average size of the cyclones is as many *miles*.

With respect to the first objection, M. Faye draws attention to a principle which he develops on p. 46, according to which the isobars in the temperate zone do not correctly represent the motions of the air in a cyclone, and says we must look at the isobars in a tropical cyclone if we wish to arrive at correct conclusions.

Here, according to M. Faye (pp. 2 and 46), where "by the *ancient* theory the direction of the wind ought to cut the isobar at an angle of nearly 90°, the angle is sensibly nothing; the pretended centripetal component disappears; and the isobars and the wind arrows display an almost rigorous *circularity*." Again, on p. 12 he ridicules the idea of a barometric gradient in the *tropics*, "where the wind blows precisely *along* the isobars." It is with no desire to indulge in mere polemic that we take up the gauntlet thus thrown down, but the magnificent work of that most careful and renowned inductive meteorologist Prof. Loomis which he has been lately revising, enables us to show most conclusively not only that in the latitude of the Philippines which is nearly the equatorial limit of true cyclones, the direction of the wind in a particularly violent and well observed typhoon cut the isobars right through at the large angle of 62°; but that an extensive comparison of similar conditions, embraced in a large number of violent storms in different latitudes, shows that the angle between the winds and the isobars *increases as it should do according to theory from the poles to the equator*.

The accompanying figure represents the observations accurately, except that the isobars were not as there exactly circular; while the following table shows at a glance how entirely opposed M. Faye's statement is to the true facts, in the very region where he says, "les isobares elles-mêmes dessinent sur le sol comme les flèches du vent un édifice cyclonique *non encore déformé*." We have no hesitation therefore in saying that these observations of Prof. Loomis not only give the death-blow, if one were needed, to the purely circular theory of Reid and Piddington, but constitute a *conclusive* argument against M. Faye's theory of downward gyratory currents and non-aspiration in *cyclones*.

Inclination of the Wind to the Isobars in VIOLENT *Storms.*

	Latitude.	Inclination of wind to isobar.
	° ′	° ′
Arctic Regions ...	70 56 ...	28 35
Atlantic Ocean ...	56 15 ...	30 6
United States ...	45 0 ...	40 3
India and Bay of Bengal...	20 48 ...	57 12
Philippine Islands ...	14 35 ...	62 12

It is true both from theory and observation that the inclination is less on sea than on land, and usually less as we approach the centre; but the above cases suffice to show the danger which might attend an unmodified adherence to the circular theory, or the rough empirical law of Buys Ballot, which is its practical expression. Dr. Meldrum, F.R.S., of Mauritius, as we have pointed out in a previous article (NATURE, vol. xxvi. p. 31), has frequently exposed the danger of following the purely circular theory, and in a number of the Journal of the Mauritius Meteorological Society for July 1883, he mentions a case in which the captain of the ship *Calédonien* on January 24, 1883, deliberately ran it into the centre of a cyclone by following the circular rules. Fortunately he subsequently became aware of his error, and altered his course just in time to escape the centre.

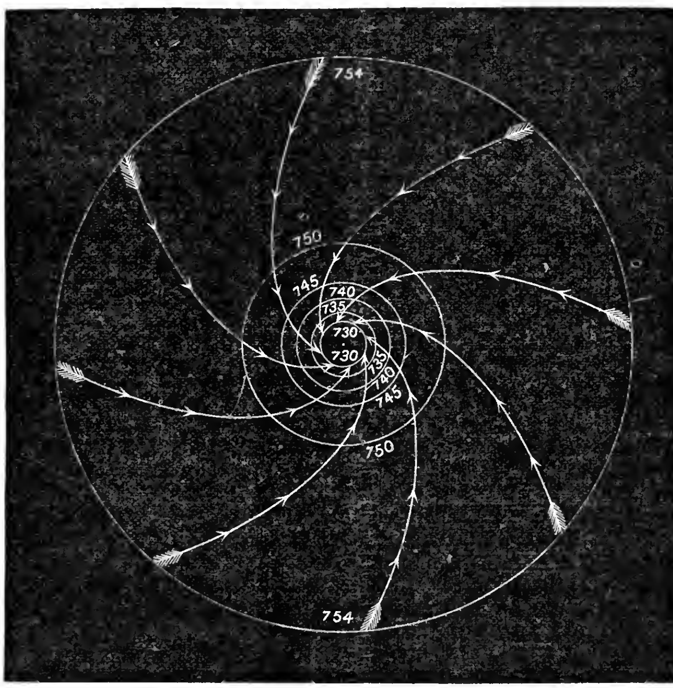
The second objection brought by M. Faye against the physical theory of cyclones is, that it cannot explain their general motions and course over the earth. We admit that the partial theory, sketched in his opening statement, which he considers to represent the modern meteorological views, could scarcely hope to account for this; but if he will allow the meteorologists to rise with him a few thousand feet above the ground, he will find that the "*drift* theory," of which he appears to regard himself as the discoverer and sole exponent, has for some years been recognized as one of the chief possible causes of the motion of cyclonic systems.

Prof. Ferrel, a representative *deductive* meteorologist, considers the motions of the upper and middle currents to be the *principal* cause of the motion of a cyclone in longitude; its motion in latitude, which is generally towards the poles, being due to the inherent tendency which a mass of fluid gyrating in the same sense as the hemisphere in which it is situated, has to press towards its pole.

Prof. Loomis, an equally representative *inductive* meteorologist, is more cautious; but in his latest work,[1] while admitting the existence of numerous other physical factors to account for the frequently anomalous movements of storm centres—which M. Faye elegantly ignores—he agrees in attributing their *general* directions of translation to the *general* extrinsic movement of the atmosphere at the time, at some height above the surface, in combination with the intrinsic mechanical principle just mentioned.

That these are not the *sole* causes of the motion of cyclones may, however, be freely admitted, and we quite agree with the remark which M. Faye triumphantly quotes in italics from Dr. Sprung's recent "Lehrbuch," on p. 14, viz. that "none of the theories which have been put forward will alone suffice to *completely* explain the motion of translation of cyclones."

Many facts, such as the observed direction of the upper clouds over and surrounding a cyclone, the velocities at the surface in different quadrants, the retardation of the barometric minima at mountain stations, and the frequently small elevation reached by the entire disturbance (not more than 6500 feet according to Loomis)—which are all entirely overlooked by M. Faye—tally more with



The Manilla cyclone of October 20, 1882. The arrows denote the direction of the wind; the circles denote the isobars at intervals of 5mm.; the inclination of the arrows to the isobars was constant all through, and = 62°·2.

a species of wave-motion by which the conditions are continually reproduced in a certain direction than with the drift theory, and in any case require other and additional causes for their complete elucidation.

We therefore entirely dissent from M. Faye's dictum that the failure up to date to discover all the causes of the motion of cyclonic areas is to be considered "an irremediable check to their meteorological theory," and we equally fail to recognize how the drift theory as put forward by him strengthens his case in favour of downward motion in tornadoes, or advances our knowledge of cyclone and tornado motion one step beyond the position it has already reached.

To return to tornadoes.

Fully armed with his preconceived theory of gyration, due to inequalities in the velocity of the upper currents, causing a downward motion of air along the axis of the whirl, and completely disregarding all evidence of upward motion or aspiration, M. Faye devotes the main part of his pamphlet to criticizing in turn the various experiments and opinions of MM. Weyher, Colladon, Lasne, and Schwedoff, with the result that he likes none of them, for the very obvious reason that, while they differ from one another in certain points, they all demand aspiration and upward motion along the axis.

We have not space to follow all these attacks in detail, but we venture to think that before attempting to strangle all adverse hypotheses it would have been wise if M. Faye had placed his own theory on a substantial basis of either physical and mechanical principles, or experiment. As it is, the sole foundations he appears to rest upon are (1) the analogy of the river eddy, and (2) the *fancied* absence of all indications of upward aspiration either during or after the passage of a tornado.

Regarding (1) we need only refer to M. Weyher's experiments, which we recently reviewed in NATURE, in

[1] "Contributions to Meteorology," chap. ii p. 142, revised edition, 1887.

order to point out that, by causing rotation at the surface, M. Weyher found himself unable to produce a gyratory system extending downwards into the liquid from the area of rotation. On the other hand, he always found rotation, whether above or below, produce aspiration (accompanied by gyration) *towards* the area initially set in motion. According to these results, therefore, river eddies produced by inequalities in the horizontal flow cannot propagate themselves *below the area of flow disturbance.*

Now it is precisely this very form of river eddy which M. Faye takes as his analogue to the aërial tornado, and it is here that his argument fails; for, while he draws attention to the system of downward motion and gyration in an eddy caused by an outflow through an orifice in the bottom of a vessel containing liquid, where such motion and gyration is evidently caused by the outflow, he is obliged to avoid all reference to outflow at the surface as a cause in the supposed downward atmospheric gyrations. At the same time he imagines that an entirely similar system takes place, in the river, and the atmospheric eddies, as in that produced by efflux, which propagates itself downwards simply through initial rotations taking place in the upper portions, of the liquid in the one case, and of the atmosphere in the other. We have no hesitation in saying that even if such an action were possible, which we strongly doubt, it is in direct opposition to all that we know of tornadoes, either deductively from physical theory, or inductively from the facts which have been recorded up to date.

It would be a laborious, though at the same time distinctly easy, task, to point out the numerous physical facts which accord with the upward aspiration and downward propagation of *conditions* only, and which are utterly opposed to M. Faye's theory of downward motion of the *air*. It would be equally easy to quote numerous observations showing the objective reality, which M. Faye questions, of upward motion in a tornado. Prof. Loomis, for example, who is noted for his caution, relates the following pregnant incident in his own life, in his preface to the revised "Contributions":—"In February 1842 a tornado of unusual violence passed within 20 miles of Hudson. As soon as I received the news, I started out with chain and compass to make a thorough survey of the track, and succeeded to my entire satisfaction. As the tornado passed over a forest of heavy timber, I had the best opportunity to learn the direction of the wind from the prostrate trees; and, by measuring the direction of the trees as they lay piled one upon another, I determined the successive changes in the direction of the wind. *The facts demonstrated incontestably that the movement of the wind was spirally inward and upward, circulating from right to left about the centre of the tornado.* This tornado was but an incident in a great storm which swept over the United States ..."; and he goes on to say that the results of his subsequent investigation of the latter showed that neither the purely circular theory of Redfield nor the purely inward theory of Espy was correct. The truth, as usual, lay between these two extremes, and the wind, like that shown in the diagram of the Manilla cyclone, really blew in a spiral, curving in towards the centre. Any of the accounts published by the United States Signal Service afford equally strong evidence in favour of both aspiration towards the centres and motion up the axes of the tornadoes. Thus, in the Report furnished by Rev. Charles Brooke, of the West Cambridge tornado of August 22, 1851, the following remark occurs: "No one saw any object driven *downward* by it, but all testify to its taking things *up*" (the italics are in the original); and then follows a list of articles taken *up* and carried, such as boards and slates, to a distance of 3 miles, a large barn 15 feet, a freight-car 60 feet, &c.

Again, in the Official Report of the Iowa and Illinois tornado of May 22, 1873, different witnesses say: "Saw boards whirling round in the funnel." "While the whirlwind was on the river, the water ceased to flow over the dam, although the river at the time was high." "Saw rails *flying out from the summit* [of the column]; an average rail weighs about forty pounds." And we may close the list with one quoted by Ferrel as a well-authenticated case, in the tornado at Mount Carmel, Illinois, June 4, 1877, in which "the spire, vane, and gilded ball of the Methodist church were carried 15 miles to the north-eastward." The whole evidence, in fact, both in tornadoes, and in their milder form of water and sand spouts, is overwhelmingly against M. Faye's views, and in favour of upward motion and aspiration to their very summits.

In his endeavour to bolster up a theory weak at all points, M. Faye seizes upon the well-known phenomenon of the central calm in cyclones, and cites one which occurred in the typhoon at Manilla on October 20, 1882, as proving the general existence of a downward current. In this case, while the thermometer during the first half of the storm marked 24° C., it rose during the passage of the central calm to 31° C., after which it fell again to 24° C. The relative humidity followed analogously inverse changes, falling from 98 to 53—an extraordinary degree of dryness for such a climate. With reference to this circumstance, M. Faye quotes with considerable triumph a remark of Dr. Sprung to the effect that "such a characteristic phenomenon can only be explained by admitting the existence of a descending current at the centre of this cyclone." Locally, and for a short space upwards, there might have been; but these particular features, accompanied by a clearing of the sky, and known as the "eye of the storm," are the exception and not the rule, even in tropical cyclones. It is, moreover, readily seen that if there were a descending current of any extent or velocity in cyclones it would necessitate *an outflow along the surface* for some distance round their centres—a condition utterly opposed to all observation and experience. M. Faye makes one more attempt to support the differential-current-motion hypothesis of tornado and cyclone generation, by referring to certain empirical laws of the relation of the former to the latter disturbances, deduced by Mr. Finley, of the United States Signal Service. For example, (1) the fact that tornadoes are usually found in the south-south-east or dangerous octant of a cyclone; and (2) a law formulated by M. Faye himself, according to which their trajectories, as traced by the areas of destruction, are parallel to those of the cyclones in which they are generated.

The first of these facts has been known for some time, and applies equally to thunderstorms. M. Faye considers it to arise from the air shot *down* from the upper currents reaching its maximum velocity "where the velocity of translation is added to that of rotation," an idea which concentrates in a truly tornadic manner two fundamental errors which pervade his work. Modern investigation has shown that the velocities of rotation and translation in cyclones are quite independent, and is in this matter as far ahead of M. Faye's view as his knowledge of cyclonic systems is superior to that of Franklin, who had no isobaric charts to help him.

Again, the south-east portion of a cyclone is precisely where, according to the corrected theory of aspiration, the conditions are most favourable to the production of local and parasitical disturbances of equilibrium, and since such disturbances take their birth in or just below the cloud-strata, their trajectories will naturally tend to follow the course of these higher strata, which in this part of the cyclone *generally* coincides with that of its translation. The violent motion, moreover, which M. Faye considers to be such an essential primary condition in the generation of tornadoes is by no means necessary, as Prof. James Thomson, among others, has pointed out in a recent paper before the British Association (British Association Reports, 1884, p. 641.)

Besides the objections we have all along pointed out to the existence of the downward current in cyclones, it renders M. Faye perfectly helpless when he contemplates an anticyclone. In the presence of such a formidable foe he is completely disarmed. Here, just where a downward current would come in really useful, he finds he has *used it all up*. All he can say, therefore, is that they have nothing cyclonic about them, which is quite true.

M. Faye concludes by drawing up a list of questions which relate to the phenomena exhibited by cyclones, tornadoes, and waterspouts, and which he considers yet unsolved. Some, doubtless, still await a more complete explanation, but we think the list might be considerably curtailed if M. Faye would descend, if possible, in one of his favourite eddies, and meet the *aspiring* meteorologist half-way. Atmospheric phenomena seldom present themselves in the form of purely mechanical problems. If, as M. Faye says, the question "is not one which can be treated by actual methods of rational mechanics on which everyone can agree," we are equally confident that it is one whose solution cannot be attempted without the aid of rational physics, or without reference to the facts already established by observation.

E. DOUGLAS ARCHIBALD.

THE VISITATION OF THE ROYAL OBSERVATORY.

THE Report of the Astronomer-Royal to the Board of Visitors of the Royal Observatory was read at the annual visitation on June 2.

One of the first points touched on in the Report is the threatened railway invasion of the Observatory.

The subject of approaching railways has again, after a lapse of many years, engaged our serious attention. Early in March notice was received from the Home Office of a proposal to carry a railway (in extension of the authorized Bexley Heath Railway) in a tunnel across Blackheath, the nearest point being 840 yards from the Observatory. As there was reason to believe that this railway might injuriously affect the Observatory, preliminary observations of the effect produced by trains on the existing Greenwich and Maze Hill Railway were at once commenced, the observations being made on six nights with the transit-circle, and the disturbance in the image of the wires, as seen by reflection from the trough of mercury, being noted. It resulted from these experiments that trains on this railway caused great disturbance during their passage, not only on the section between Greenwich and Maze Hill, the nearest point of which is 570 yards from the transit-circle, but also on the line beyond Greenwich on the London side and beyond Maze Hill on the Woolwich side. The distances of the Greenwich and Maze Hill stations from the Observatory are about 970 and 670 yards respectively. There was also evidence of disturbance caused presumably by trains on the Lewisham, Blackheath, and Charlton line, at a distance of about a mile from the Observatory, but we could only infer the times of passage of these trains from the published time-tables.

In order to establish conclusively the connection between definite disturbances and trains, arrangements were made to note the times of arrival and departure of trains on the Greenwich line and at Blackheath, facilities for doing this having been courteously given by Mr. Myles Fenton, the Manager of the South-Eastern Railway. Observations were made on this plan on five nights, one observer being stationed at the transit-circle to record all disturbances of the reflected image, while another observer travelling up and down the Greenwich line, and a third observer at Blackheath, noted the times of arrival at and departure from the stations. It was found that the disturbance was very great during the passage of trains between Greenwich and Maze Hill, the reflected image being invisible while the train was in the tunnel, at a minimum distance of 570 yards, and that there was considerable disturbance during the passage of trains through the Blackheath-Charlton tunnel, at a distance of a mile, the reflected image becoming occasionally invisible. As the tunnel of the proposed railway would be similar in character to this, but at half the distance, it was concluded that it would cause so great a disturbance as to make delicate observations impossible. On my notifying this to the Admiralty, the Bill was opposed on the part of the Government, and as a consequence of this the clauses authorizing the construction of the railway across Blackheath were abandoned.

I may here mention that the extension of the London, Chatham, and Dover Railway from Blackheath Hill to Greenwich, which was authorized in 1881, is now in course of construction. I hope that, though the terminus of this line is distant only 620 yards from the Observatory, the tremor from trains will not have sufficient time to produce the full accumulated effect in the short interval between Blackheath Hill station and the terminus. But if at any future time a further extension of this line should be proposed, the question would require very careful consideration in the interests of the Royal Observatory.

The following statement shows the number of observations made with the transit-circle in the period of 356 days ending May 10, 1888:—

Transits, the separate limbs being counted as separate observations	5304
Determinations of collimation error	294
Determinations of level error	351
Circle observations	5067
Determinations of nadir point (included in the number of circle observations)	331
Reflection-observations of stars (similarly included) ...	503

About 350 transits (included in the above number) have been observed with the reversion-prism, to determine personality depending on the direction of motion.

The very bad weather in the first four months of this year has seriously affected the number of observations with the transit-circle.

The total number of observations made with the altazimuth is as follows, the observations having been as usual restricted to the first and last quarters in each lunation, except during the winter, when, in the absence of suitable objects for equatorial observations, the moon was observed throughout the lunation.

Azimuths of the moon and stars	354
Azimuths of the azimuth mark	114
Azimuths of the collimating mark	116
Zenith distances of the moon and stars	209
Zenith distances of the collimating mark......	116

In consequence of the building operations for the extension of the computing-rooms the collimating mark was dismounted on November 9, and the view of the azimuth mark has been obstructed by the new building from the beginning of December. Since then the collimation and azimuth errors have been determined entirely by observations of high and low stars. It is proposed, when the work on the new building is completed, to select two azimuth marks, one distant and the other sufficiently near to be seen in the foggy weather of the winter months. For distinct vision of the latter a lens of very long focus would be required, and it would thus be available strictly as a collimating mark.

All will regret to hear that no progress has been made since the date of the last Report in the construction of the new 28-inch refractor, owing to difficulty in obtaining the crown disk. The flint disk made by Messrs. Chance seems to be satisfactory, but up to the present neither that firm nor M. Feil's successor has succeeded in making a crown disk.

Attempts have been made to show if anything is gained in sidereal photography by using curved plates. For this purpose a 4-inch photographic object-glass by Dallmeyer, belonging to one of the photoheliographs, was mounted at the end of June in a light wooden tube, and firmly attached to the side of the telescope tube and parallel to it, to carry out experiments on the extent of field available on plane and curved plates respectively, the latter being moulded by Messrs. Chance to a radius of 22 inches, corresponding to the curvature of the field, if the circle of least confusion be taken for the image. We read:—

Forty-one photographs have been taken of the Pleiades and other objects with different exposures and in different parts of the plate, 13 of these being on curved plates. In these experiments the Sheepshanks refractor was used as directing telescope, the image of a star being kept on its cross-wires during the exposure of a plate by means of the slow motions. The plates measure 6 inches × 6 inches, representing $5\frac{3}{4}^\circ \times 5\frac{3}{4}^\circ$, and it is found that on the flat plates the star images are sensibly circular to a distance of nearly 2° from the centre of the field, while micrometric measures of these plates show that for some distance beyond this limit the relative places of stars can still be measured with an accuracy exceeding that of meridian observations, and with no sensible systematic error depending on magnitude or duration of exposure. Comparison of the results on flat and curved plates respectively indicates that the advantages of using the latter are doubtful. As the Dallmeyer object-glass is peculiar in having the flint outside, it was reversed in the cell in the course of the experiments, and some photographs were taken with it in this position, the flint being inside. It appeared on comparing the results that a somewhat better field is obtained with the flint outside. A photographic object-glass of 6 inches aperture and 6 feet focal length, made by Sir H. Grubb for experiment, was mounted at the end of April in place of the 4-inch object-glass, and some trial photographs of stars have been taken with it.

Special arrangements were made for observing occultations during the total eclipse of the moon on January 28, observers being stationed at nine instruments, but clouds covered the moon almost continuously during totality. Various devices were adopted with a view to facilitating the observation in rapid succession of the faint stars occulted during the eclipse. In the case of two instruments the eye-piece was mounted excentrically at the distance of the radius of the moon's image from the axis, so that without disturbing the position of the telescope any point of the limb could be brought into the centre of the field. For setting the position-circles rapidly in the dark, cardboard circles with notches at important points or with the figures indicated with luminous paint, were found very useful.

The spectroscopic observations of motions of stars in the line of sight have been continued. The recent observations of Algol confirm the previous results indicating orbital motion, but further observations are required to establish the fact. At the request of Mr. Lockyer, the spectra of α Orionis, α Herculis, γ Cassiopeiæ, and β Lyræ have been examined on several occasions.

That the daily record of the solar surface is gradually getting more complete is clearly shown by what happened in the year 1887, in which Greenwich photographs are available on 188 days; photographs from India or Mauritius filled up the gaps in the series on 173 days, thus making a total of 361 days out of 365 on which photographs have been measured in this year.

The sun has been free from spots on 106 days in the year 1887, and the areas of both spots and faculæ have diminished since the date of the last Report. With the exception of a fine group seen during three rotations in May, June, and July, and of three other groups, one in July and two in December, all of these being in the southern hemisphere, there has been a complete absence of conspicuous spots. The entire spotted area has rarely amounted to 1/2000 of the sun's visible hemisphere, and the mean is less than one-sixth of that recorded in 1883, being intermediate between those for the years 1875 and 1876.

In view of the diminution of the current work as the minimum of sunspots approaches, the further discussion of the results of former years has been commenced, and arrangements have been made through the Solar Physics Committee to complete the Greenwich results as far as practicable by the measurement of photographs taken elsewhere, particularly at Ely and Cambridge, U.S. From the beginning of 1882 the photographic record is practically complete, the measurement of Indian photographs to fill up gaps in the Greenwich series having been undertaken from December 22, 1881. The further discussion of results has, therefore, been commenced from that date, and the projected areas of spots (uncorrected for foreshortening) have been formed to May 29, 1885, and from the beginning of 1886 to the end of 1887. The ledgers in which the areas and positions of the spots of a group are collected and the mean area and position of the group, deduced for each day and for the whole period of visibility, have been formed for 1886 and 1887, and their completion for the years 1882 to 1885 will now be taken in hand. Two new forms have been prepared to exhibit the distribution of spotted area on each day for every degree of latitude and for every 10° of longitude, mean results being taken for each rotation and for each year.

With regard to magnetic observations we read that the only important change is the substitution, since October last, of a wooden bar loaded with lead, of the same size and weight as the declination-magnet, for the brass bar hitherto used for determination of the torsion of the suspending skein, a very weak trace of magnetism having been detected in the brass bar.

The earth-current observations have been attended with some difficulties. We read:—

The earth-current wires, which were damaged by the snowstorm of 1886 December 26, were not completely repaired till August 1887, when it was found that the earth-plate at Angerstein Wharf had been stolen, another earth-plate being then supplied. A renewal of the earth-current wires concurrently with the telegraph wires on this portion of the South-Eastern Railway was arranged, in concert with Mr. Leonard, but this has not been carried out owing to a rise in the price of copper. Five measures of resistance of the earth-current wires have been made since the last Report, but the results are not satisfactory, owing presumably to the bad condition of the wires. On the line from Angerstein Wharf to Ladywell, $7\frac{1}{2}$ miles in length, the measures of resistance range from 220 to 285 ohms, and on the Blackheath to North Kent East Junction line, 5 miles long, the measures range from 230 to 262 ohms. Under these circumstances it seems hopeless to attempt to express the measures of ordinates on the earth-current sheets in terms of the electrical units until the conditions of the circuits have been improved. A further difficulty arises in discussing the small diurnal inequality on the earth-current registers in consequence of the circumstance (to which attention was first drawn by Mr. A. J. S. Adams, of the Post Office Telegraphs) that there is a slight dislocation in the Angerstein Wharf to Ladywell trace shortly after sunset with sudden return to the original position shortly before sunrise, representing an increased current from Ladywell to Angerstein Wharf, or a diminished potential at Angerstein Wharf during the night hours. Possibly this may be connected with the electric lighting in the vicinity of the earth plate. It appears to have commenced in 1883, becoming more pronounced in 1884.

The following are the principal results for the magnetic elements for 1887:—

Approximate mean declination . .	17° 47′ W.
Mean horizontal force . .	3·9419 (in British units) 1·8175 (in Metric units)
Mean dip	67° 25′ 45″ (by 9-inch needles) 67 26 20 (by 6-inch needles) 67 27 13 (by 3-inch needles)

In the year 1887 there were only three days of great magnetic disturbance, but there were also about twenty other days of lesser disturbance for which tracings of the photographic curves will be published, as well as tracings of the registers on four typical quiet days.

The mean daily motion of the air in 1887 was 275 miles, being 9 miles below the average of the preceding twenty years. The greatest daily motion was 829 miles on March 23; and the least, 59 miles on November 16. The only recorded pressure exceeding 20 pounds on the square foot was 20·5 pounds on April 6.

During the year 1887, Osler's anemometer showed an excess of about 17 revolutions of the vane in the positive direction N., E., S., W., N., excluding the turnings which are evidently accidental.

The number of hours of bright sunshine recorded during 1887 by Campbell's sunshine instrument (Prof. Stokes's improved pattern) was 1401, which is about 190 hours above the average of the preceding ten years. The aggregate number of hours during which the sun was above the horizon was 4454, so that the mean proportion of sunshine for the year was 0·315, constant sunshine being represented by 1.

The rainfall in 1887 was 19·9 inches, being 4·8 inches below the average of the preceding forty-six years.

There has been no failure in the automatic drop of the Greenwich time-ball, but on four days the ball was not raised on account of the violence of the wind.

The automatic drop of the Deal time-ball failed on six days owing to interruption of the telegraphic connections, and on two days high wind prevented the raising of the ball. There has been no case of failure of the 1 p.m. signal to the Post Office Telegraphs.

There have been twenty-three failures in the automatic signals from the Westminster clock since the date of the last report. The error of the clock was insensible on 25 per cent. of the days of observation, 1s. on 38 per cent., 2s. on 20 per cent., 3s. on 15 per cent., and 4s. on 2 per cent.

Provision has been made in the estimates for the expense of a re-determination of the difference of longitude between Greenwich and Paris, and correspondence has been carried on with the French authorities on the subject. The regretted death of General Perrier occurred before any definite plan had been settled; but his successor, M. le Commandant Bassot, has taken the matter up warmly in concert with Admiral Mouchez, and the French Bureau des Longitudes has approved the scheme, which is to include a determination of the longitude of Dunkirk. Three French delegates (M. Lœwy, M. Bassot, and M. Defforges) propose to visit Greenwich very shortly to settle the details of the plan of operations which it is intended to carry out in the autumn. In preparation for the work, Mr. Turner and Mr. Lewis have observed for practice, by eye and ear, a number of galvanometer signals sent by another observer and automatically registered on a chronograph, five sets of ten signals having been recorded on each of seven days.

The Report concludes as follows:—

In my last Report it was suggested that the instrumental equipment of the Observatory should be supplemented by a photographic refractor of 13 inches aperture (equatorially mounted) to enable Greenwich, as the National Observatory, to take its share in the scheme for forming a photographic map of the heavens, and for thus extending our knowledge of the places of the fixed stars. Consequent on the resolution of the Board of Visitors at the last visitation, I brought this question of the insufficiency of our instruments for the present wants of astronomy to the notice of the Admiralty and of the Chancellor of the Exchequer, and the matter is still under the consideration of the Government. If the Royal Observatory is to take part in this work of carrying out one of the principal objects for which the Astronomer-Royal was appointed, it appears to be essential that a decision should be arrived at without delay, in view of the circumstance that thirteen Observatories (including those of Melbourne and Sydney in our own colonies) have already ordered their instruments, which are to be completed by the end of the present year.

Allusion was made in the last Report to the increased demands made on the Observatory in recent years both by the scientific and the general public, and in view of the consequent development of work it now becomes necessary to review the position of the establishment, which was constituted many years ago, when the conditions were very different. In order to understand the difficulty of the present situation it is necessary to bear in mind the following facts:—In 1835 there were five assistants (excluding the chief assistant), having no computers to superintend, no extraneous work beyond the care of a relatively small number of chronometers for the Navy, no magnetic and meteorological observations, no altazimuth observations, no spectroscopic and photographic observations. At the present time there are eight assistants (excluding the chief assistant) having fifteen computers to superintend, and of this staff two assistants are absorbed by the magnetic and meteorological branch, one by the altazimuth, and two by the spectroscopic and photographic branch, leaving only three assistants to do the astronomical work, which in 1835 required five assistants, and in addition to perform all the extraneous duties which the Astronomer-Royal has felt it desirable to undertake in the public interest.

Under these circumstances it becomes a matter for serious consideration whether, unless adequate provision be made for the primary objects of the Observatory, extraneous work, such as the supply of time-signals, may not have to be dropped. The service of hourly time-signals throws considerable work on myself and the staff of the Observatory, and, as it is purely voluntary, it appears to me that a condition of its maintenance must be that arrangements shall be made to enable the proper work of the Observatory to be carried on and suitably developed.

INDUSTRIAL TRAINING.

At a meeting held at the Mansion House on Friday last, in support of the scheme for establishing Polytechnic Institutes in South London, an able and interesting speech was delivered by Lord Salisbury. Having pointed out that of late years much had been done for primary education, he went on to show that a sound system of secondary education for the great mass of the people was not less necessary. Secondary education, as we know it at present, had been established for the benefit of classes who in the main had not to work for their living. Plainly, therefore, it was not adapted to the needs of the working classes. "What we have now to do," he continued, "is to provide an education which will develop for each man the faculties that Nature has given him in such a manner that he may be as active, profitable, and prosperous a member of the community as possible." Lord Salisbury then passed in review the efforts which have been made in London to meet the demand for technical instruction, and concluded as follows:—

"I have only one more word to say, just to call your attention to another aspect of this case and to commend it to your efforts. We live in a time when men multiply fast, but apparently the means of supporting them do not multiply as rapidly; when there is vehement competition and occasionally intervals of deep depression. And if you should look more closely, you will find that one cause at least of this phenomenon is that man, as the mere owner of muscle, is being edged out by another and more powerful competitor. Merely as an agent of physical force, as the possessor of the power of labour, the steam-engine is a competitor which drives him easily out of the market. And more and more the mere unskilled labour is being made unnecessary by the development of the forces which mechanical science has discovered. And as the world goes on, you must expect this tendency to increase. You must expect mechanical force to become more varied and more powerful and more cheap, and the competition with human arms and limbs to become more hopeless. But there is one region where the machine can never follow the human being, and that is in the exercise of thought. In skill, in cultivated mind, in the power to adapt the processes of thought to the laws of Nature, in all that we call 'skilled labour' of the highest kind, in that man must always have a monopoly, and need fear no encroachment from the competition of the steam-engine. It is to the development of his powers in that respect that the increase in the means of subsistence and the opening of new paths of self-support must be found. On all of us, in whatever position we are, is pressing, as one of the most anxious subjects of public care, the discovery of methods by which the teeming millions of this country shall be able to maintain themselves in a prosperous, decent, and comfortable condition. We cannot find in their unskilled labour a satisfaction of that want. The difficulties are enhanced by the fact that our neighbours in other countries have been sensible of the superiority which skilled education can confer, and have not been slow to take advantage of it. If we will not be left behind in the race, if we desire to find any satisfactory solution for the deepest and the most inscrutable problem of our time, if we wish our complex community and high civilization to be maintained secure from all the dangers which the presence of unfed, unprosperous, untaught millions must bring upon them, we shall do our utmost to give a healthy and a rapid development to the secondary education of the working classes."

The *Times*, commenting on the meeting addressed by Lord Salisbury, says:—

"The Prime Minister spoke of the occasion as marking an era in the development of secondary education. The expression is scarcely too emphatic. Many of those present at the Mansion House have been for years labouring for that cause, and often with little confidence that they would ever see the produce of the seed which they sowed. Now, however, the husbandman's hopes rise, for he discerns everywhere lusty shoots flourishing, and he knows that a harvest is at hand. It is no small matter to find Government recognition of the importance of manual or technical education in a Bill which will enable any School Board to promote it. What London has done other cities will do, and here much has been done, and still more is imminent. The Polytechnic and the Beaumont Institutes are admirable pioneers. The projected Institutes for South London will soon, we should hope, be established; and the Charity Commissioners have promised to grant £50,000 in aid of an Institute for the south-west parishes north of the river on condition that the same amount is contributed by the district. What limits are there to the possible benefits from a network of such institutions over London and other great cities? Even if they fail to sharpen the wits of our workers, and to prepare them for their part in that struggle which the Prime Minister eloquently described as the course of civilization, if the foreign clerk continues to oust our own youth, we may count with certainty on deep and far-extending good from institutions mingling instruction with recreation, uniting many of the good points of clubs and schools, serving to some as ladders for ambition to climb with, to others as refuges from the public-house, and introducing intellectual light into the dark places of our cities. For many a man and woman, especially at the outset of life, narrow means would lose all terror if there were open of an evening an Institute such as was described yesterday; and it would be the best palliative of that dull monotony which in some walks of life is more injurious, as it is immensely more common, than downright viciousness."

For many a day, as our readers know, we have been urging the necessity for the establishment of a proper system of technical instruction. The subject is one of such pressing importance that we have returned to it again and again, seeking to present it in many different aspects; and Lord Salisbury's speech and the article in the *Times* may be taken as indications that large classes of the community have at last begun to understand that the nation has no time to lose in setting about a task which ought long ago to have been most seriously undertaken. Even if the question had little direct relation with economic interests, it would be for many reasons desirable to secure for manual training a place among our educational methods. Attention has hitherto been too exclusively devoted in schools to such knowledge as may be derived from books. It is necessary, from the strictly educational point of view, that teachers should aim at a wider, more direct, and more practical development of the mental powers of their scholars. But other and even more fundamental interests are also concerned. The leading nations of the world, our rivals in industry and trade, have already perceived the benefits to be secured from a thorough mastery, on the part both of employers and employed, of the principles of science as applied to agricultural and manufacturing processes. The result is that in many of the best markets, where our supremacy as a trading people was formerly unquestioned, we find ourselves at a disadvantage; and it is certain that unless we place ourselves on a level with our competitors we shall have to pass through some very bitter national experiences. The question is really one of life and death for England. It is a question whether in the near future there are or are not to be sufficient employment and remuneration for the vast and growing masses of her population.

WEISMANN ON HEREDITY.[1]

THE fundamental property of all living matter is assimilation and consequent growth; and reproduction is merely discontinuous growth. This is most apparent in the Protozoa, where the primitive form of reproduction—division into two parts—is common. Each part exactly resembles the other part, and both the parent. Heredity in them merely means identity of bodily substance, and consequent identity of vital phenomena. In Metazoa there is a sharp distinction between reproductive cells and body cells. In many cases it is certain that the reproductive cells of each new organism arise directly from the reproductive cells of the parent. Here there is as manifestly a continuity or identity of the germ-plasma as in the Protozoa. As has already been explained by Prof. Moseley in this paper, Weismann extends this phylogenetic continuity of germ-cell, or at least of germ-plasma—the essential constituent of the germ-cell—to all the Metazoa.

In the Metazoa, the germ-cells, instead of remaining single, give rise to the vast number of somatic cells which compose the adult structure. The form, arrangement, and succession of these depend on the germ-plasma; and as there is continuity of this from generation to generation it follows that the structures derived from it are identical in each generation. Obviously this view excludes the possibility of the inheritance of acquired characters. But this inheritance has been proved neither by observation nor by experiment, and it has been impossible to conceive any satisfactory mechanism by which it could be accomplished.

Weismann believes that the theory of the inheritance of acquired characters is not required to explain the phenomena of the organic world. In the production of an acquired character two forces are at play, and these forces in relation to the organism may well be called *centripetal* and *centrifugal*. The centrifugal forces are ultimately referable to the molecular constitution of the germ-plasma, and are transmitted with the other properties of the germ-plasma from generation to generation. Changes in the centrifugal forces due to that mixing of plasmata which is the object of amphigonic reproduction constantly occur. Adaptation and differentiation result from the action of the environment (centripetal forces) on these continual changes in the possibilities of the organism. Not acquired characters, but the internal possibilities of them, are transmitted: not the results, but the centrifugal causes of them, are transmitted and accumulated by natural selection. An example will make this clear. Giraffes are certainly descended from short-necked forms. According to the old theory, during life their ancestors, by constantly stretching to reach higher and higher branches of the acacias, &c., on which they fed, elongated their necks a

[1] "Ueber die Vererbung," von Dr. August Weismann. (Jena, 1884.)

little. Each addition to the neck so acquired was transmitted to the descendant, and by accumulation of the changes thus produced the modern long-necked condition was attained. According to Weismann, what happens is this. In each generation slight variations in the length of the neck, as in the other parts of the body, occur. These variations are due to constitutional causes which are transmitted. When greater length of neck became important to the animal, those animals with necks a little longer or capable of being stretched out a little further, would have the advantage, would survive longer, and leave more offspring. The offspring, inheriting the constitution of their parents, would inherit this tendency to have longer necks. By the continual elimination in many generations of the short-necked forms, and by the seizing hold of each naturally-occurring variation, the long-necked condition would finally appear.

As variations are constantly occurring, natural selection must constantly be at work to maintain the standard of any organ. Whenever an organ ceases to be of use, or even when it becomes merely of subordinate utility, this selective maintenance falls into abeyance. A state that Weismann calls *Panmixia* results. Variations below the standard cease to be eliminated, and the organ slowly degenerates. In this way is explained degeneration through disuse: degeneration from conditions that are not harmful but merely unnecessary. In many cases organs that are not used degenerate very much during individual lives, but this occurs through failure of nutrition. Weismann believes such effects not to be transmitted. Were these effects inherited, useless organs must inevitably disappear very much more rapidly and completely than there is evidence for.

Instincts are elaborated, not by the accumulation of transmitted individual experience, but by continual selection of mental variations in the required direction. For instance, the instinct to avoid enemies arose not by accumulation of experience, for experience of the inconvenience of being devoured could hardly be transmitted, but by the naturally more timid forms surviving, and leaving more offspring than their less wary brethren.

Talent and even genius often run through several generations; and certainly mental powers can be much increased in individual lives. But the exhibition of talent and genius depends on a combination of many physical and mental conditions in which constitutional variation is ever present, and these variations are undoubtedly inheritable. Moreover, the history of families of conspicuous ability (as, for instance, that of the musical family Bach) shows that the highest development often occurs in the middle of the series, while the theory of the transmission of acquired characters would demand to find it at the end.

Selection of variations best explains cases of adaptation to new climates. But the immense influence of climate conditions on nutrition in each ontogeny must be taken into account.

Qualitative changes at first present some difficulty, but it must be remembered that qualitative changes are nearly always at bottom quantitative. A surface appears naked, though covered with many small hairs; or light-coloured, though scattered pigment-cells are present. Quantitative variations in such conditions certainly occur, and are certainly transmitted, and natural selection can readily change the number or size of hairs or pigment-cells, and produce so-called qualitative results.

It is not claimed as yet that the inheritance of acquired characters can be excluded in every case. But increasing knowledge of the conditions of life and of the functions of organs causes ever a larger and larger part of the phenomena of the organic world to be explained by the selection of naturally-arising variations.

P. CHALMERS MITCHELL.

IMPERIAL GEOLOGICAL UNION.

REFERRING to my letter on the above subject, published in NATURE, vol. xxxvi. p. 146, I beg to communicate, for the information of those interested in the matter, the substance of a report made to the Royal Society of Canada at its meeting on May 22, in Ottawa.

The Committee reported that it had, as directed, printed the letter of Sir William Dawson to the President of the Royal Society, and the first report of the Committee, and had circulated these extensively, sending them especially to geologists and Societies in Great Britain and the colonies and dependencies of the Empire. A large number of replies had been received, testifying to a somewhat general wish for union and co-operation.

The matter was then laid before the Council of the Royal Society, with the view of holding a Conference in London under its auspices. The subject was taken up by the Council in October last, and a resolution was passed and communicated to the Committee to the effect that, having regard to the existing condition of the question of scientific federation, and the various contingencies that may occur during the next few years, they do not see their way to summon such a Conference as that recommended.

In view of this resolution it was felt to be useless for the present to attempt any farther action. Still, as the desire for and appreciation of the benefits of the union contemplated seemed to be very general, and as opportunities may occur later for giving it a practical form, it was thought best by the Royal Society of Canada to continue its Committee, with power to correspond with other bodies and with persons interested. The undersigned will therefore be glad to receive any communications on the subject.

Some misconception appears to exist as to the relations of the intended movement to the International Geological Congress which is to meet in London in September next. They have in reality no connection, except that, under certain contingencies, they might be mutually helpful.

A Union of British Geologists might exercise an influence for good in connection with the plans for unification of classification, nomenclature, and mapping, which have occupied the attention of the Congress; but its function would rather be the positive one of uniting workers throughout the wide area occupied by the British Empire, and enabling them more effectually to co-operate in the extension of actual knowledge, in giving mutual aid, in enlarging the mental vision of local and special workers, in making accessible to isolated labourers the common stock of knowledge, and in preventing the interference and discordance which result from disunited effort.

That there are difficulties in the way of the realization of such a plan as applied to British and colonial geologists in the first instance, and ultimately to all English-speaking geologists, there can be no doubt; but they are continually diminishing, in consequence of greater facilities for intercourse and the rapid growth of scientific work in the various outlying parts of the Empire. The idea is thus a fruitful one, certain to be realized in the future; and possible even at present if a central nucleus could be secured for an Imperial organization. It is not impossible that the large gathering of English-speaking geologists in London in September may afford opportunity for further discussion of the plan; and if the invitation which it is understood will be given by our friends of the United States to hold the next meeting in America be accepted, this may constitute another step in the same direction.

Montreal, May 31.

J. WM. DAWSON.

NOTES.

THE Laboratory of the Marine Biological Association at Plymouth is now approaching completion, and, after the opening ceremony on the 30th inst., it will be, in all essential respects, ready for work. The salt-water reservoirs have, after several delays, been filled, and the water is now circulating freely in the tanks of the aquarium. The fittings of the main laboratory are complete on the north side, and will give accommodation for seven naturalists, besides the Resident Director. In addition to this there are the physiological and chemical laboratories, all the fittings of which are now in place, and the library is in process of formation. The Association stands very much in need of presents of books, and it is hoped that those who are interested in its work, and have duplicate copies of biological works on their shelves, will be disposed to present them to so deserving an institution. At the opening ceremony on the 30th, upwards of a hundred members and their friends are expected to be present. The fact that Parliament is in session will keep away many of those who take a liberal interest in the Association, but it is hoped that Sir Lyon Playfair, Sir Edward Clarke, and Sir Edward Birkbeck will be present to represent the Parliamentary interest. Prof. W. H. Flower will be the presiding zoologist, and with him will be many well-known men of science, including Profs. Ray Lankester, Milnes Marshall, McIntosh, C. Stewart, Dr. Günther, Mr. Adam Sedgwick, and many others. The Hydrographer has stated his intention to be present, and the naval and military element will be fully represented by the commanding officers of both services at Plymouth. The Fishmongers' Company, which has been so munificent a patron of the Association, will be fully represented by its Prime Warden, Sir James Clarke Lawrence, and several members of the Court. They have kindly undertaken the hospitable duties of the occasion, and there can be no doubt that the *déjeûner* at the Grand Hotel, and the speeches that may be expected to be made there, will form a most important part of the day's proceedings.

THE annual meeting for the election of Fellows of the Royal Society was held at the Society's rooms in Burlington House on June 7, when the following gentlemen were elected: Thomas Andrews, F.R.S.E., James Thomson Bottomley, M.A., Charles Vernon Boys, Arthur Herbert Church, M.A., Prof. Alfred George Greenhill, M.A., Lieut.-General Sir William F. D. Jervois, R.E., Prof. Charles Lapworth, LL.D., Prof. T. Jeffery Parker, Prof. John Henry Poynting, M.A., Prof. William Ramsay, Ph.D., Thomas Pridgin Teale, F.R.C.S., William Topley, F.G.S., Henry Trimen, M.B., Prof. Henry Marshall Ward, M.A., William Henry White, M.I.C.E.

DR. S. H. VINES, F.R.S., Fellow of Caius College, Cambridge, has been elected to the Sherardian Professorship of Botany at Oxford.

THE King of Sweden, who was elected an Honorary Member of the Linnean Society at the centenary anniversary meeting of that Society held at Burlington House on May 24 last, gave an audience on Friday afternoon to the President (Mr. W. Carruthers, F.R.S.), Secretaries (Messrs. B. D. Jackson and W. P. Sladen), and Librarian (Mr. Harting), and inscribed his name in the album wherein the names of all Fellows and Honorary Members have been inscribed since 1788. The Royal signatures include those of George IV., William IV., Queen Victoria, Prince Albert, the Prince of Wales, the King of the Belgians, the King of Saxony, and now the King of Sweden.

THIS week the University of Bologna is celebrating the eighth century of its existence. A congratulatory Greek ode has been written by Prof. R. C. Jebb, who represents the University of Cambridge as its senior delegate at Bologna. The verses, which are composed in the metres of Pindar's eighth Olympian ode, are suggested by the circumstance that the University of Glasgow, in which Prof. Jebb holds the Chair of Greek Literature, is the only University in this country of which the model was taken directly and exclusively from Bologna.

THE second annual *soirée* of the Middlesex Natural History and Science Society was held at the Society's rooms, 11 Chandos Street, Cavendish Square, on Thursday evening last. Lord Strafford, the Lord-Lieutenant of the county, President of the Society, was in the chair. Many objects of scientific interest were exhibited.

THE Hon. J. Collier has undertaken to paint the portrait of Prof. Williamson, which is to be presented to University College.

THE Conferences convened by the London Chamber of Commerce to consider the question of commercial education led to the appointment of a Committee for the full discussion of the subject. This Committee nominated a sub-Committee, among the members of which were Sir John Lubbock, Sir Henry Roscoe, and Sir B. Samuelson. A scheme for the improvement of commercial education has now been drawn up by the sub-Committee and sent to various business men, schoolmasters, and other authorities on education, with a request for practical suggestions. The scheme, as it stands, proposes as obligatory subjects for examination for a commercial certificate: (1) English; (2) Latin; (3*a*) French; (3*b*) German, Spanish, or Italian; (4) history of British Isles and colonies, general and modern history, including commercial history; (5) geography, physical, political, commercial, and industrial; (6) mathematics; (7) drawing. Proficiency is also required in at least one of the following: physics, chemistry, natural history, commerce, and political economy.

PROF. LÜTKEN, Director of the Zoological Museum of Copenhagen, has addressed a strong appeal to country people in Denmark to protect the sand grouse. He points out that the only countries in which the birds nested in 1863 were Denmark and Holland, but that owing to people gathering and eating the eggs no birds were hatched. He trusts that this wanton conduct may not now be repeated. The Professor feels sure that the bird can be acclimatized in Denmark, as the sandy cliffs and shores of that country are particularly suited to its breeding. The Zoological Gardens in Copenhagen have obtained a live specimen of the bird, caught in the Island of Fünen. Flocks upwards of a hundred in number have of late been seen in many parts of Denmark.

ONE of the largest pine-trees ever grown in Sweden was felled the other day in Lapland. It measured over 120 feet in height, and was 12·5 feet in diameter 2 feet from the ground.

ON the evening of May 14, about 10 p.m., a brilliant meteor was seen at Kalmar, in Sweden. It was about the size of an ordinary plate, the colour being pale yellow, and it had a train about 100 feet in length. It went in a north-westerly direction, apparently only some little height above the ground, and exploded some distance from the town with a noise like that of burning gunpowder. During its progress a whizzing sound was distinctly heard.

IN vol. iv. Part 4, of the *Indian Meteorological Memoirs*, Mr. J. Elliot gives a list and brief account of the south-west monsoon storms generated in the Bay of Bengal during the years 1882–86. This list, which contains Nos. 47–101 of the series of storms, is a continuation of that given in the sixth paper of the second volume of the *Memoirs*, and is accompanied by yearly and monthly track charts. Some of the principal storms have been fully discussed in previous parts of the *Memoirs* and in the Journal of the Bengal Asiatic Society. The retreat of the south-west monsoon in October 1866 was followed by the occurrence of three cyclones at intervals of about a fortnight. They presented such marked peculiarities that they have been specially investigated. All were generated in the immediate neighbourhood of the Andamans. The first, which began to form on November 2, is an example of a class of storms, of occasional occurrence, which pass across Southern India into the Arabian Sea, and it lasted for a fortnight. It is the first example of its kind which has been fully worked out. The second, which was also a very violent storm, was formed on November 13, and affords a marked illustration of the effect of a mass of land in modifying the motion of a cyclonic disturbance. The third storm formed on December 7, and was in many respects exactly similar to the first, excepting that it was comparatively feeble at sea and short-lived on land.

AT the meeting of the French Meteorological Society on May 1, M. Poincaré presented calculations and synoptic charts showing mean barometric heights at latitude 30° and 10° N., for every day from December 9, 1882, to December 15, 1883, and on the parallels of 40°, 50°, and 20°, for a number of selected days, and pointed out certain relations which he considered existed between the barometric movements at these latitudes, and the positions of the sun and moon, and the effect of these on the displacements of the region of the trade winds. M. Renou made a communication upon the unsatisfactory condition of actinometry, and showed that the values obtained varied according to the instruments used, the force of the wind, &c., and he submitted some of the observations made during seven years at the Observatory of the Parc Saint-Maur. The Secretary presented, on the part of M. Pictre, of Pau, a plan for the graphical representation of local observations, in connection with general weather charts, with the view of facilitating local predictions. M. d'Abbadie urged the desirability of developing the study of earthquakes, and offered to give particulars as to an inexpensive form of seismograph, and as to the observations required, to persons willing to undertake such investigations.

THE Committee of the Association for the Oral Instruction of the Deaf and Dumb have issued their Report for 1887. They express much regret that in a great many instances the children are too early removed from the school established by the Association. Parents and guardians appear to think that as soon as a fair amount of speech and lip-reading has been acquired there is no longer any need for special training. Notwithstanding this drawback, the Committee feel assured that in each year the friends of oral instruction increase in numbers, and that the time is not far distant when the manual alphabet and sign language, if retained at all, will exist only as a special requirement for cases of imperfect vision and semi-imbecility. At the Training College two grades of certificates are now granted—first-class for head, second-class for assistant teachers. During the year 1887 eleven female teachers attended the Training College, of whom six obtained first-class, and two second-class certificates.

A NEW edition of Sir Walter Buller's "History of the Birds of New Zealand" has been issued. Without going over identically the same ground, the author gives in this edition a more thoroughly complete account of the birds of a country which is second in interest to none in the world as regards its natural history. A melancholy interest attaches to the avifauna of New Zealand, where so many of the indigenous birds, remains of a most ancient fauna, are either extinct or on the verge of extinction. Sir Walter Buller deserves well of every naturalist for the wonderful pains and energy he has shown in getting together the facts for the life-histories of many of these birds, which in a few years no one will be able to procure, and he has accomplished his task ably. The scientific portion of the work and the full descriptions of the species are as well written as the accounts of the habits. The plates have been done by Keulemans, and produced by chromolithography, but, like all illustrations of birds produced by this process, they are not quite satisfactory. Insects appear to us to be capable of illustration by chromolithography, but birds do not lend themselves so readily to this method. The delay in production is excessive, and the cost very considerable, while the efforts to produce a striking plate result in some loss of exactness in the colouring of the bird, this being not strictly accurate in many cases. That this should result when the best lithographic draughtsman of birds in the world has been employed, and unlimited money been spent on the production of the plates, clearly shows us that chromolithography is, and ever will be, inferior to hand-colouring.

THE fifth monthly part of the "Cyclopædia of Education" (Swan Sonnenschein) has now been issued. The complete work will include about twelve parts.

A SECOND edition of Mr. S. R. Bottone's "Electrical Instrument-making for Amateurs" (Whittaker and Co.) has just been issued. In compliance with the request of several correspondents, the author has added a short article on the telephone.

SIR DAVID SALOMON'S useful "Management of Accumulators and Private Electric Light Installations" (Whittaker and Co.) has already reached a fourth edition. The author has thoroughly revised the work and made some additions, including the "Rules and Regulations for the Prevention of Fire Risks," as laid down by the Committee of the Society of Telegraph-Engineers and Electricians.

MESSRS. GUY AND CO., at Cork, and Messrs. Simpkin, Marshall, and Co., London, have published a "Guide" to what the enthusiastic author calls "the most picturesque tour in Western Europe." By this he means a tour in the south-west of Ireland. The little volume is illustrated.

MR. LELAND'S work on "Practical Education" has reached a second edition. He will now follow up the ideas set down in this book by a series of illustrated hand-books on the minor arts and industries. The series will begin with a manual on "Drawing and Designing."

"A FRESH-WATER YARN," an illustrated account of a boat-voyage up the River Avon, is announced for immediate publication by Mr. Elliot Stock.

MR. T. FISHER UNWIN is about to publish a second edition of Mr. Edward Newman's "Birdsnesting and Bird-skinning." The work has been revised, and practically re-written, with, in addition, directions for the collection and preservation of birds, and a new chapter on bird-skinning, by Miller Christy.

MESSRS. E. AND F. N. SPON have in preparation "The Drainage of Fens and Low Lands by Gravitation and Steam Power," by W. H. Wheeler; "Practical Notes on Pipe Founding," by James W. Macfarlane; and "A System for the Construction of Crystal Models on the Type of an Ordinary Plait," by John Gorham.

THE administrators of the schools of the Caucasus have just brought out the first volume of the works of the late General Uslar. No explorer of the Caucasus has done so much as Uslar did for the ethnography of the region, yet his works are little known. In 1862 he published his remarkable researches into the Abkhazian language, and laid the foundations for a rational, most appropriate, and easy transcription of this and other Caucasian languages. Later on, he brought out similar works on the languages of the Tcherkesses, Avarians, Lakhes, and so on. He did not merely compile more or less perfect vocabularies of each language, but thoroughly learned each in turn, with the help of natives, and he considered his work worthy of publication only when he could bring out an elaborate grammar. Unhappily all his works were merely lithographed in a limited number of copies. Now the first volume has appeared at Tiflis under the title of "Ethnography of the Caucasus." It contains Uslar's work on the Abkhazian language, and several smaller articles on the principles of transcription of the Caucasian languages; on the languages of the Tcherkesses and Ubykhes; and on the grammar of the Svanetian language.

THE sporadic geographical distribution of the *Aldrovandia vesiculosa*—an aquatic plant of the family of Droseraceæ—long ago attracted the attention of botanists. Grisebach and De Candolle discussed it, and Caspary made it the subject of two well-known monographs, trying to explain the strange distribution of the *Aldrovandia*, a few individuals of which had been discovered, after much hunting for them, in localities so far apart as Arles, Bordeaux, and a very few other places in France; at isolated spots in Italy, Tyrol, and Hungary; in Silesia; about Pinsk in Lithuania; and at Calcutta. Since Caspary wrote, it has been discovered also in Brandenburg, South Bavaria, and at two other spots in Prussia. Schweinfurth discovered it in Central Africa, and Ferd. Müller in Australia; and Russian explorers have found it on the Lower Amu-daria, and in the delta of the Volga. Taking up again the whole question as to the causes of its sporadic extension, in the *Trudy* of the Kazan Society of Naturalists (vol. xvii. fasc. 1), M. Korzchinsky shows that in the delta of the Volga it grows especially in thickets of rushes. There, in the most inaccessible parts of the thickets, the water is covered with flowers of the *Aldrovandia*, while in open places it is very scarce, and the few individuals discovered rarely flower. MM. Herbich and Berdan noticed the same circumstance on the Tiniecki Lake about Cracow; and M. Korzchinsky concludes that the *Aldrovandia vesiculosa* is a feeble plant which cannot compete with other aquatic plants, and is thus compelled to seek for refuge in the shaded spots amidst the rushes where no other aquatic plants grow. The spots where the *Aldrovandia* grows now must be regarded as a few remnants of a wide region over which it formerly extended, and M. Korzchinsky compares it in this respect with the *Trapa natans*, which is also disappearing.

How far north did the Caspian Sea extend during the post-Pliocene period? This question has often been considered by geologists and geographers. Marine deposits, undoubtedly Caspian—that is, containing a fauna which is now characteristic of the Caspian Sea—have been recently found as far north as the Samara bending of the Volga; so there can be no doubt that during the post-Pliocene period a gulf of the great Aral-Caspian basin penetrated north, up the present valley of the Volga, as far as the 54th degree of north latitude. A few years ago Prof. Golovkinsky raised the question whether the post-Pliocene sediments which fill up the great depression on the middle Volga at its junction with the Kama, were not also deposited in a great lake which stood in connection with the Caspian; and this question is now answered by M. Netchayeff, who has investigated these deposits. He communicates to the Kazan Society of Naturalists (*Trudy*, vol. xvii. fasc. 5), that the brown-yellow sandy clays on the Kama about Tchistopol (55° 20′ N. lat.), contain the following fossils: *Dreyssena polymorpha*, most characteristic of the Aral-Caspian deposits all over the Trans-Caspian region, *Pisidium fontinale*, *Paludina achatina*, *P. impura*, *Limnæus fuscus*, *Helix pulchella*, and the *Hydrobia caspia* (Eichwald). The latter, according to Grimm, is one of the forms now in the Caspian Sea which are found only in that sea. We must therefore conclude that the Kazan depression of the Volga, now about 150 feet above the sea-level, *i.e.* 235 feet above the level of the Caspian, was a part of that sea at a period so close to our own as the post-Pliocene.

THE cod and whale fisheries in the north of Norway have entirely failed this spring, and it is suggested that the non-appearance of the former is due to the low temperature of the sea this season. Thus the Russian naval officers stationed on the Murman coast found in May only a surface temperature of from 1° to 2° C., and along the Norwegian coast it has been lower still. As to the whale-fishing, only 40 animals had been captured by the end of April against 200 last year. It is maintained that the present wholesale slaughter carried out by Norwegian and Russian steamers equipped with harpoon guns will eventually extirpate these animals, and some measure for their preservation is contemplated. Advices from the Arctic regions state that there was an enormous mass of drift-ice in those waters during this spring. Two sealers, the *Hekla* and the famous *Vega*, were imprisoned for more than a month in the ice to the north-east of Norway.

IN the very useful scientific methods whereby movements record themselves in curves, photography and a point moving on a smoked surface are perhaps those forms which yield the most delicate curves. In the French Société d'Encouragement, M. Mascart has called attention to a useful modification by M. Fénon, in which a bent tube of tempered steel forms a siphon, dipping at one end in a reservoir of ink, and at the other being shaped like a pen point, which is brought near the moving paper (the sloped section outwards). Capillary force prevents outflow when the apparatus is at rest. A fine trace is produced by this pen, without interruption by the most rapid displacements, and without sticking when at rest. M. Wolf, of the Paris Observatory, has used the system for getting records of air-pressure, temperature, wind, &c., with the best results. The reservoir needs charging only once a week; and using inks mixed with glycerine a single charge has been found to suffice for a barometer record of more than six months.

IN a recent interesting lecture, opening his course at the Collège de France, M. Ribot gave a sketch of contemporary psychology. The science in France might be characterized by one expression—"the era of monographs." There was no comprehensive work like that of Wundt in Germany; such were certainly very useful, but, like vast cathedrals, they always needed repair at some point. In psychology proper, the part belonging to logical operations, to reasoning, as principle of the unity of perceptions, had been well studied; and perhaps the most important results had been reached in the study of the nature and physical conditions of the image. The psychology of movements, especially those expressing thought, had yielded a rich harvest; while the great amount of experimentation in hypnotism, and the foundation in 1885 of a Society of Physiological Psychology (impossible twenty years ago), showed the vitality of French studies. In England, the principal contributions were in comparative psychology, represented chiefly by the work of Lubbock and Romanes. Germany was the centre of psycho-physics. Wundt's laboratory at Leipzig, founded in 1879, had acquired great renown, and, last year, had twenty students of different nationalities working in it. M. Ribot justified those studies, which had been rather depreciated in France. The predominating tendency in Italy was criminal psychology (better known as criminal anthropology)—the three chiefs of the school being Lombroso, mainly a biologist; Ferri, a sociologist and statistician; and Garofals, a jurist. It had gained several adherents in France, and there were symptoms of its invading Spain. In the United States, as in Germany, public instruction had almost alone played the part of initiation in the psychological movement; in England, the work had been chiefly done by books (Mill, Bain, Spencer, &c.). Four American Universities now gave special teaching in physiological psychology, and had laboratories, psycho-physics being the dominant study. A journal devoted to experimental psychology was started at the Johns Hopkins University, last November, by Prof. Stanley Hall. The work of James at Harvard was also referred to. Allusion was further made to Russia, which might be expected to take a good place in the psychology of the future.

THE additions to the Zoological Society's Gardens during the past week include five Pea-fowls (*Pavo cristatus*, 2 ♂, 3 ♀) from India, presented by Her Majesty the Queen; a Pagoda Owl (*Syrnium sinense*), a Horsfield's Scops Owl (*Scops lempiji*) from Penang, presented by Mr. C. B. Ricketts; three Grey-breasted Parrakeets (*Bolborhynchus monachus*) from Monte Video, presented by Mrs. Macnab; a —— Gull (*Larus* ——) from Massowah, presented by Mr. D. Wilson-Barker; a Chilian Skunk (*Conepatus mapurito*) from Chili, a Black-necked Swan (*Cygnus nigricollis*) from Australia, a White-throated Monitor (*Varanus albogularis*) from South Africa, purchased; a West Australian Great Kangaroo (*Macropus ocydromus* ♂) from West Australia, two Wandering Tree Pies (*Dendrocitta vagabunda*) from India, received in exchange; a Japaneese Deer (*Cervus sika* ♀), a Burrhel Wild Sheep (*Ovis burrhel* ♀), born in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JUNE* 17–23.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on June 17

Sun rises, 3h. 44m.; souths, 12h. 0m. 43·7s.; sets, 20h. 17m.: right asc. on meridian, 5h. 45·4m.; decl. 23° 25′ N. Sidereal Time at Sunset, 14h. 3m.

Moon (at First Quarter June 17, 7h.) rises, 12h. 1m.; souths, 18h. 33m.; sets, 0h. 52m.*: right asc. on meridian, 12h. 19·2m.; decl. 2° 56′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	5 37	13 43	21 49	7 27·5	21 58 N.
Venus ...	3 19	11 32	19 45	5 16·3	22 57 N.
Mars ...	13 43	19 13	0 43*	12 58·5	6 38 S.
Jupiter ...	17 36	21 59	2 22*	15 45·3	18 54 S.
Saturn ...	6 52	14 43	22 34	8 28·0	19 45 N.
Uranus...	13 23	19 3	0 43*	12 49·3	4 35 S.
Neptune..	2 27	10 12	17 57	3 56·0	18 44 N.

* Indicates that the setting is that of the following morning.

June.	h.	
18	13	Mars in conjunction with and 5° 48′ south of the Moon.
20	15	Uranus stationary.
21	0	Sun at greatest declination north; longest day in northern latitudes.
21	12	Jupiter in conjunction with and 3° 51′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	June 20,	23 14	*m*
R Virginis	12 32·8	7 36 N.	,, 23,		*m*
δ Libræ	14 55·0	8 4 S.	,, 22,	2 28	*m*
U Ophiuchi...	17 10·9	1 20 N.	,, 23,	2 6	*m*
			,, 23,	22 14	*m*
Z Sagittarii...	18 14·8	18 55 S.	,, 19,	1 0	*M*
			,, 23,	0 0	*m*
η Aquilæ	19 46·8	0 43 N.	,, 20,	1 0	*M*
X Cygni	20 39·0	35 11 N.	,, 22,	22 0	*m*
δ Cephei	22 25·0	57 51 N.	,, 20,	22 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near β Ursæ Majoris	169°	55° N.	
,, ζ Cygni	318	32 N.	
Between δ and ε Cephei	335	57 N.	Swift.

GEOGRAPHICAL NOTES.

THE paper read at Monday's meeting of the Royal Geographical Society was on Hudson's Bay and Hudson's Strait as a navigable channel, by Commander Markham. It was really a brief sketch of a much larger memoir on Hudson's Bay which Commander Markham has prepared, and which will ultimately be published by the Society. For some years investigations have been carried on with a view to discover whether the navigation of Hudson's Bay could be so depended on as to justify its acceptance as a regular trade route, in conjunction with a railway, to the more northerly parts of Canada. Commander Markham briefly sketches the history of navigation in Hudson's Bay, and concludes with the results of his own visit in the summer of 1886 on board the *Alert*. The result, he states, of all the experience gathered from voyages during two centuries, and from observations at the stations, is that Hudson's Strait is perfectly navigable and free from ice in August and later in the season. It must be remembered that this passage has been successfully accomplished nearly every year for the last two centuries, while the vessels that have been employed on the service have been ordinary sailing-ships, dependent entirely on wind and weather. It is very rare indeed that they have failed to get through, and still more rare that any of them have been destroyed by the ice. It appears from the official records of the Hudson's Bay Company that Moose Factory, on the southern shore of the bay, has been visited annually by a ship since 1735, with but one exception, namely in 1779, when the vessel for once failed to achieve the passage of the strait. The percentage of losses by shipwreck among these vessels employed in Hudson's Bay is far less than would have to be recorded in a like number of ships engaged in general ocean traffic. Commander Markham pointed out that until quite recently only sailing-vessels attempted to navigate Hudson's Bay, and maintained that with a properly constructed steam-vessel, there need be neither difficulty nor danger. The establishment of new routes for commerce, Commander Markham concluded, is always a gain to the science of geography. In come cases new regions have to be discovered and explored. In others the physical aspects of an already known region must be more carefully studied, and many points of interest relating to the action of climates, or of winds and currents, may be ascertained. The proposed Winnipeg and Hudson's Bay Railroad is a striking instance. The objections of opponents to the route have had to be carefully examined. All former experience had to be collected, maturely considered, and passed in review. Observatories had to be established at several points, to make certain whether the historical records actually coincided with physical facts as they now exist. The route itself had to be sailed over and explored. All these various researches have been as great a gain to geography as to commerce. They have enriched our science with a fresh stock of information, have revised previous conceptions, and confirmed or rejected, as the case may be, the theories and views which may have been put forward. From this point of view, and from this point of view alone, can commercial or political questions receive consideration here. The study of the Hudson's Bay route involves a problem for which physical geography alone can furnish a solution.

DR. F. H. H. GUILLEMARD has been recommended, by the joint Committee of the Royal Geographical Society and the University, as Lecturer on Geography at Cambridge.

THE *Bollettino* of the Italian Geographical Society for May publishes the map of the Massawa district (Massawa to Saati) prepared to the scale of 1 : 80,000, by Prof. P. Durazzo, with the materials which have been supplied by the Italian Staff officers during the recent military operations in that region. Prof. Durazzo has also now completed his large map in two sheets, scale 1 : 800,000, of all the Italian possessions and protectorates in East Africa. These cartographic works embody the results of all the latest surveys, and contain several new features, as well as some important corrections of existing maps.

OUR ELECTRICAL COLUMN.

THE beautiful illustrations of stress in a dielectric in an electric field, due to Dr. Kerr, have been modified and amplified by Messrs. Rücker and Boys, and were shown to a large audience at the Institution of Electrical Engineers on March 22, and again at the *soirée* of the Royal

Society. The dielectric they used was carbon bisulphide (CS_2), and the beam of light passed through about four inches of the liquid. The presence and intensity of the electric field was evident to all by the brightness of the screen. They showed experiments to illustrate the fact that the repulsion of similarly electrified bodies may be regarded as an attraction between each of them and surrounding objects. They have devised an experiment visible to a large audience to show that in an electric field the structure of the CS_2 becomes crystalline—that is, the optical properties along and transverse to the electric lines of force are different; in other words, the velocities of propagation of light vibrations differ when parallel and perpendicular to the lines of force, contrary to the view formerly held on the Continent that the effect is due to unequal expansion. They were able to increase the stress so that the liquid displayed colours even to the green of the second order; and by observing the spectrum of the light passing through the field, black bands enter at the violet end and traverse its whole length as the potential rises. Faraday's experiments and speculations, Maxwell's mathematics and theories, are rapidly becoming acknowledged facts; and the apparatus of Messrs. Rücker and Boys will materially assist in spreading a knowledge of the confirmation which those theories receive from the work of Kerr and Quincke.

BLONDLOT (*Comptes rendus*, January 30, 1888) has been working in the same direction, but with vibratory discharges from a Leyden jar, in order to test the existence or non-existence of retardation in the optical effects. He could see no retardation.

COWLES's process for the production of aluminium from its ores by the direct action of an electric current of 5000 amperes in an electric furnace has now become an industry. Works have been started near Stoke, and bronzes of wonderful quality are supplied at comparatively cheap prices.

THERE is a fashion in experimental investigation as in everything else. Self-induction is played out, and now the counter E.M.F. of the arc is passing through the same phase. Uppenborn (*Beiblätter*, No. 1, 1882, p. 83) is the last inquirer. He finds for a current of 7·7 amperes and 10 mm. carbons, that $a = 35{\cdot}4$ to $45{\cdot}4$; $b = 1{\cdot}74$ to $3{\cdot}2$ in Edlund's formula—

$$E = a + bl.$$

Since a decreases both for an increase of current and for an increase in the section of arc, he leans to a resistance hypothesis rather than an E.M.F.

KLEMENCIC (*Beiblätter*, No. 1, 1888, p. 57) finds the specific inductive capacity of mica to be 6·64; Cohn and Arons (*Ann. der Physik*, No. 1, 1888, p. 13) that of distilled water 76, ethyl alcohol 26·5, amyl alcohol 15, and petroleum 2·04.

PALMIERI (March 1888) has observed that in a bright clear sky, with a high and steady barometer, and every indication of continued fine weather, the electrometer will give an indication of change long before the barometer.

W. KOHLRAUSCH (*Electrotechnische Zeitschrift*, March 1888) has estimated the current and quantity of electricity in a lightning-flash. He calculates that it will take 9200 amperes to melt a copper rod of 2·5 centimetres diameter. Preece's constant (Proc. R.S., March 1888) makes it 10244. Such a current concentrated in a flash would contain from 52 to 270 coulombs, which would decompose from 5 to 25 milligrammes of water, and from 9 to 47 cubic centimetres of explosive gas. If this energy were stored up and distributed for electric lighting, it would require from 7 to 35 such flashes to keep one glow lamp alight for an hour.

VOGEL (*Electrotechnische Zeitschrift*, January 1888) had previously calculated the relative value of copper and iron as lightning-protectors, giving iron a section 2·5 times that of copper to act with equal efficiency. Preece's constants give the relative efficiency—

Iron	3148
Copper	10244

for equal diameters—that is, an inch rod will fuse with the above currents in amperes; or, if we take the same current, say 300 amperes—

Iron	0·2086
Copper	0·095

are the diameters in inches of the wires such currents will fuse, or in the ratio 2·2 to 1; Vogel's ratio being 13·54 to 9·6. Vogel did not consider the emissivity of the surface, and therefore his results are not so accurate as Preece's experimental figures.

THAT patient worker, H. Tomlinson, has proved that the temperature at which nickel begins to lose its magnetic properties is between 300° and 320° C.; but that the rate of decrease of magnetic permeability, and the temperature at which permeability practically vanishes, vary with the magnetizing force, and hence the widely different results by different observers. Faraday made the former point 330° to 340°; Becquerel 400°; Pouillet 350°; Chrystal 400°. Iron behaves in the same way: permeability vanishes between 750° and 770° according to Ledeboer.

PROF. EWING AND MR. COWAN have been examining the magnetic qualities of nickel on the same lines as the former examined iron. They confirm Sir W. Thomson's observation that longitudinal pull diminishes magnetism to a surprising extent. Their paper in the Philosophical Transactions will be looked forward to with much interest.

S. ARRHENIUS (*Wiener Berichte*, xcvi. p. 831) has shown that the electrical conductivity of chloride and bromide of silver was influenced by the intensity of the rays of light which fell upon the salts. It was most intense at G of the spectrum, and is therefore an effect of light, and not of heat.

F. KOHLRAUSCH (*Wiedemann's Annalen*, No. 4, 1888) has shown that the electric conductivity of

Hard steel	is 3·3
Soft steel	,, 5·5
Wrought iron	,, 7·6

mercury being 1; while their thermal conductivities in C.G.S. units were—

Hard steel	0·062
Soft steel	0·111
Wrought iron	0·152

the ratios being the same. Hence the conditions that determine the conduction of heat and electricity are the same.

MR. C. V. BOYS's interesting magnetic and electric experiments with soap-bubbles, and his wonderful manipulative skill, remind old *habitués* of the Royal Institution how exquisitely Faraday handled soap-bubbles blown with oxygen to illustrate the magnetic character of that gas. Mr. Boys blows one bubble inside another, and, on bringing the two into an electric field, the perfect indifference of the inner one to any change of potential clearly shows that electrification is confined to the absolute surfaces of a conductor, and that it is not felt at any depth within it, however small.

WHEAT CULTIVATION.[1]

THE most interesting sections of this number of the Journal are those bearing upon the subject of wheat cultivation. The permanent wheat and barley experiments at Woburn, reported upon by Sir John Lawes, Bart., is followed by a paper upon the condition of wheat-growing in India by Dr. George Watt, Reporter upon Economic Products to the Government of India. Next comes an article by Mr. W. E. Bear upon the Indian wheat trade. Lastly, in this connection, comes a highly interesting account of modern improvements in corn-milling machinery. These four papers occupy one-third part of the volume, and taken in connection with each other throw considerable light upon the difficulties under which the English wheat-grower is struggling. Dr. Watt and Mr. Bear both show the extraordinary extent of the wheat-producing area of our Indian Empire, and the rapidity with which this vast field is being opened up. With reference to the latter point men in middle life are scarcely likely to realize the fact that in 1853 there were in all only 20½ miles of railway in India, that in 1873 there were 5695 miles of railway, while in 1887 there were 13,386 miles. Telegraphic communication with India was first opened in 1865, and the opening of the Suez Canal in 1869 was scarcely of less importance in developing her trade—first, by shortening the passage, and secondly, by mitigating the risk from wheat weevil. Another agency has been the development of irrigation works.

[1] The Journal of the Royal Agricultural Society of England, vol. xxiv. (second series), part 1. (John Murray, Albemarle Street.)

We read that "only" 30,000,000 acres have up to date been artificially irrigated, but the appropriateness of the qualifying adverb is rendered evident when it is employed in contrast with the total area of 200,000,000 acres of cultivated ground, and the vast tract of 868,314 square miles which include British India. The normal area under wheat is 26,000,000 acres, and the degree to which this area is likely to be increased depends entirely upon demand and price. Dr. Watt informs us that the Indian cultivator is at all times ready to adapt his courses of cropping to circumstances, and that he will increase or abandon the cultivation of wheat, cotton, or any other crop according to its comparative profitableness.

Dr. Watt comes to the conclusion that the Indian wheat trade up to the present time is a perfectly natural one. "The people are exporting only what they specially cultivate for that purpose. The moment better profits can be realized on another crop they will turn from wheat, without being in the least degree incommoded." If this is the case, the English farmer may well look with envy upon his Indian brother, as he is in the unfortunate position of being compelled to carry on wheat-growing from sheer inability to find a substitute for it in his agricultural economy. Natural though the course of the ryot may be from his point of view, the actual bounty upon wheat, or what amounts to a bounty, consequent upon the fall in value of the rupee, can scarcely be described as natural. This great advantage to the Indian cultivator is clearly brought out by Mr. Bear by the following considerations. First, the Indian ryot gets as much for a quarter of his wheat now as he obtained in 1872. He gets as many rupees, and his rupees are worth as much to him as they were then! In 1871-72 the average exchange value of the rupee was 1*s.* 11·12*d.*, whereas recently it has been under 1*s.* 5*d.* The price of No. 2 club wheat in Calcutta in 1872 averaged only 2*rs.* 3*a.* 1*p.* per maund, whereas it has for some time past been over 2*rs.* 10*a.*! Taking 16*rs.* per quarter (6 maunds) as the price for both periods, then reckoning the exchange value of the rupee for both periods, it is clear that the exchange value of 16*rs.* in 1872 was equal to 30*s.* 8*d.* per quarter, whereas the exchange value of the same sum in 1888 is only 22*s.* 8*d.* The fact is that the Indian ryot gets as much for a quarter of wheat now as he did in 1872, in spite of the fall in prices. He gets as many rupees, *and his rupees are worth as much to him.* This seems to settle the question as to the encouragement given to the ryot as a competitor in wheat-growing with the English farmer. Another point, in all respects discouraging to the cultivation of wheat in England, is found in the complete revolution during the last ten years in corn-milling machinery described by Mr. W. Proctor Baker, of Bristol. There has been in fact not a mere substitution of one machine for another, or of one series of machines for another, but there has been a change of the principle and mode of procedure. The old system of "low grinding" by mill-stones, so well calculated for producing flour from soft, tender wheats, such as are produced by us, has been entirely superseded by the Hungarian and American "gradual reduction" process by "roller mills." Not only does this system require the wheat to be dry, hard, and brittle, so as to secure the requisite cracking and gradual reduction, but anything in the form of a soft or moist wheat is most injurious to the machinery and the products. It rolls into a paste, steam is generated, and the flour works into balls, becomes attached to the rollers, turns sour, and, in fact, throws the entire process out of gear. "It is because of these troubles that owners of mills on a large scale will not employ native wheats in damp seasons. No concession of price is sufficient inducement to them to risk the disorganization of the mill, and probable loss of reputation, by turning out inferior or irregular flour." There are, however, two modes in which these wheats may be used. First, by submitting them to an artificial drying process; and secondly, by mixing them with some description of very brittle wheat, and allowing the mixture to lie for some weeks, until the brittle wheat absorbs some of the moisture of the native wheat, to the mutual advantange of both.

One of the most serious points at issue between science and agricultural practice at present appears to be the comparative values of farm-yard manure and artificial fertilizers. So far as absolute experiment goes, the evidence seems to be in favour of the application of the latter, while, on the other hand, the preponderating opinion among farmers is on the side of farm-yard manure. In the Report on the Field and Feeding Experiments at Woburn, by Dr. J. Augustus Voelcker, applications of dung appear somewhat at a disadvantage when contrasted with applications of salts of potash, phosphates, and nitrates direct. Mr. Vallentine, in his paper upon the practical value of dung as compared with artificial manures, declares in favour of the latter, and labours to show the extravagant cost at which farm-yard manure is produced. "For years past," he says, "my main reliance has been placed on artificial manures. Some dung is made and some bought, but it is found to answer best, as a rule, to sell hay and straw and to purchase manures." This may answer on some classes of soil; but what would be the effect upon our high-lying and thin chalk downs if we were to relinquish sheep-farming and depend upon "artificials?"

Many more valuable papers well repay perusal, among others one upon recent experiences in laying land down to grass, by Mr. James A. Caird. The remainder are mostly official in character, being the usual Reports upon implements, prize farm competitions, shows, experiments, and the Annual Reports of the Consulting Chemist, Botanist, and Entomologist, which, however, are none the less valuable for being official.

Downton. JOHN WRIGHTSON.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The following are the speeches delivered on June 9 by the Public Orator, Dr. Sandys, Fellow and Tutor of St. John's College, in presenting for the honorary degree of Doctor in Science, Prof. G. G. Stokes, Lord Rayleigh, Sir Frederick Abel, Prof. Cayley, and Prof. Adams:—

(1) Salutamus deinceps Regiae societatis praesidem, professorem nostrum Lucasianum, senatorum nostrorum omnium consensu Britanniae senatoribus additum; quem in munere illo triplici Newtoni nostri in vestigiis insistere gloriamur. Atqui ipse, qua est morum suavitate et modestia, vix tali sese honore dignatur, sed a plausu populari remotus et seclusus, templum quoddam serenum occupat, ubi reverentia debita rerum naturae miracula perscrutatur, ubi "in statione tranquilla collocatus" lucis leges obscuras observat, observatas ingenii sui lumine illustrat. Viro tali rerum naturam contemplanti crediderim apparere nonnunquam sedes illas quietas,

> "quas neque concutiunt venti, nec nubila nimbis
> aspergunt, neque nix acri concreta pruina
> cana cadens violat, semperque innubilus aether
> integit, et large diffuso lumine ridet."

Duco ad vos virum illustrem, PROFESSOREM STOKES.

(2) Venio ad nomen physicorum professoris quem non sine desiderio nuper amisimus, viri cum Cancellarii nostri munificentia haud ita pridem consociati. Ex illo velut fonte, liberalitatis flumen amplum professoris nostri in provinciam defluxit inque alias Academiae partes redundavit. Ipse fontium exsilientium et aquarum destillantium naturam quam feliciter exploravit; caeli colorem illum caeruleum quam dilucide explicuit; quicquid audiendi quicquid videndi ad rationes intimas pertinet, quam sapienter interpretatus est; quotiens in rerum natura eventis specie quidem inter sese diversis causas easdem subesse ostendit. Quam profundam rei mathematicae θεωρίαν, ut aiunt, cum quanta in experimentis instituendis sollertia coniunxit; quam subtilem denique scientiae cognitionem cum sensu illo communi consociavit qui non in magna tantum fortuna sed in omni vitae condicione rerum omnium est revera ra rissimus.

Duco ad vos IOANNEM WILELMUM STRUTT, BARONEM RAYLEIGH.

(3) Scientiam Chemicam et in bello et in pace utilem esse, quis negabit? Heri in hoc ipso loco virum hunc insignem docentem audivistis, quo potissimum modo scientia illa populi saluti consulere et pericula pacis in artibus suscepta possit avertere. Idem Martis fulmina illa antiquis ignota quam familiariter tractat: pulverem illum formidolosum quo Bellona gaudet, quot experimentis vexat: quam admirabilem in modum velut Olympius ille Aristophanis, fulgurat, tonat, omnia permiscet. Atqui non minus quam Pericles ille Atheniensis, qui tot insularum imperium civitati suae conciliavit, inter ipsa tonitrua audit tot coloniarum Britannicarum uno in imperio coniunctarum vocem, et illorum consiliis pro virili parte op'tulatur qui in ipsa μητροπόλει artium et scientiarum templum quoddam tanto imperio dignum consecrare voluerunt. Templi illius e sacerdotibus unum, cuius praeceptor coram Principe nostro in hoc senaculo quondam

laudatus est, hodie coram eodem, templi illius praeside illustrissimo, titulo nostro libenter ornamus.

"sunt hic etiam sua praemia laudi;
sunt lacrimae rerum et mentem mortalia tangunt."

Duco ad vos Hofmanni discipulum, Faradai successorem, FREDERICUM AUGUSTUM ABEL.

(4) Perveni tandem ad Professorem nostrum Sadlerianum, virum non modo in recentioris quae dicitur Algebrae provincia, sed etiam studiorum mathematicorum in toto regno inter principes numeratum; qui, quamquam iuris peritia honores summos adipisci potuisset, maluit sese scientiae illi dedicare, quae verbis quam paucissimis, quam illi quae verbis quam plurimis, rerum veritatem exprimere conatur. Quantum tamen prudentia eius Academiae profuerit, et senatus totius concilium et Collegium plus quam unum testantur; neque Cami tantum prope ripas sed etiam in ipsa Europa atque adeo trans aequor Atlanticum fontes eius aliis patuerunt. Idem, velut alter Socrates, ipsi rerum pulchritudini et veritati mentis oculis contemplandae sese consecravit, arbitratus illa sola quae studiorum suorum in puro velut caelo sint, revera esse, illorum autem imagines quas φαινόμενα vocamus, velut specus εἴδωλα videri; ipsam vero pulchritudinem percipi quidem posse sed non omnibus explicari. Quam dilucide tamen regnum suum quondam non campo deserto comparavit sed regioni cuidam pulcherrimae primum e longinquo prospectae, cuius partem unamquamque posse deinde peragrari, cuius et clivos et valles, et rivos et rupes, et flores et silvas posse propius maxima cum voluptate aspici. Diu, inter numina silvestria, regionem illam laetam feliciter pererret Professor noster insignis, ARTHURUS CAYLEY.

(5) Extra ipsas Athenas, stadiis fere decem ab urbe remotus, prope ipsam Platonis Academiam, surgit Coloneus ille tumulus Sophocleo carmine olim laudatus, Neptuni templo quondam ornatus, astronomi magni Metonis cum memoria consociatus. Et nos Colonum nostrum iactamus, clivum illum spatio a nobis eodem distantem, locum arboribus obsitum, avibus canorum, ubi in templo quodam stellis observandis dedicato vivit Neptuni ipsius inventor. Quid si Colono nostro deest Cephisus? sed aqua de clivo illo antiquitus deducta, Collegii Herscheliani sub hortis transmissa, Newtoni in Collegio in fontem exsilit. Quid si Neptuni inventi gloria cum altero participatur? sed, gloriae illius geminae velut imago perpetua, Geminorum in sidere est stella quaedam quae caeli totius inter stellas duplices prae ceteris fulget. Idem neque stellarum geminarum cursus, neque Saturnum neque Uranum inexploratum reliquit; neque faces illas caelestes, Leonides vocatas, quas ter in annis fere centenis orbes suos magnos conficere ostendit; neque motum illum medium lunae qui cum motu diurno terrae collatus per saeculorum lapsus paullatim acceleratur. Talium virorum laudibus non debet obesse quod inter nosmet ipsos vivunt; pravum enim malignumque foret "non admirari hominem admiratione dignissimum, quia videre, alloqui, audire, complecti, nec laudare tantum, verum etiam amare contigit."

Tot insignium virorum nominibus hodie velut cumulus accessit vir illustris, PROFESSOR ADAMS.

The Senior Wrangler of the year is Mr. Orr, of St. John's; the Second Wrangler Mr. Brunyate, of Trinity. No woman is placed with the Wranglers; but one, Miss H. F. Ashwin, of Girton, is bracketed with the first Senior Optime.

The Rede Lecture was delivered in the Senate House on Friday, by Sir F. A. Abel, on the applications of science to the protection of human life.

The Report on Local Lectures gives particulars of a large number of science lectures given in local populous centres. At evening lectures on astronomy at Northampton, Mr. J. D. McClure had a regular audience of 277, and 250 at Aylesbury. The formation of Students' Associations, for mutual aid between the lectures, has been very useful. Several students from Northumberland came up to Cambridge in the Long Vacation, and did practical work in chemistry and biology.

The Syndicate appointed to report on Sir Isaac Newton's manuscripts in the possession of the Earl of Portsmouth, the scientific portion of which he offered to present to the University, have prepared a detailed catalogue of the whole, which is to be published.

Prof. Thomson announces that students who receive permission may work in the Cavendish Laboratory in the Long Vacation. There will be a special course for those who have passed the Mathematical Tripos, and intend taking the Natural Sciences Tripos.

In the Long Vacation, Mr. Fenton will give a general course on Chemistry, Mr. Potter will lecture on Systematic Botany with practical work, Prof. Macalister will lecture on Osteology, and Mr. Wingfield will give a revision course of Practical Physiology for Dr. Foster; Prof. Roy will lecture on the Elements of Pathology, and will hold a practical course on three days a week.

Prof. Lewis will lecture on Crystallography during July, and Mr. Solly will give elementary demonstrations in Mineralogy during July and August.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. x. No. 3 (Baltimore, April).—The number opens with an article by M. E. Goursat, "Surfaces telles que la somme des rayons de courbure principaux est proportionnelle à la distance d'un point fixe au plan tangent" (pp. 187–204), in which are discussed some surfaces of a somewhat more general character than those treated of by M. Appell in the last number of the *Journal*. The title sufficiently indicates the scope of the memoir, which in part touches upon work accomplished by Riemann.—"Remarks on the Logarithmic Integrals of Regular Linear or Differential Equations" (pp. 205–24), by Karl Heun, follows up Fuchs's investigations (*Journal für Mathematik*, lxviii. p. 376). The author has elsewhere shown that the Fuchs equations are not independent of each other when the differential equation is of a higher order than the second, and in this paper he deduces, from elementary considerations, the minimum number of conditions on which the existence of logarithms depends. In addition he gives several theorems concerning the pseudo-singular points.—Mr. C. H. Chapman, in his article "On Some Applications of the Units of an n-fold Space" (pp. 224–42), obtains a proof of the rule for multiplying two determinants of the nth order by the principles of quaternions.—In "A Problem suggested in the Geometry of Nets of Curves and applied to the Theory of Six Points having Multiply Perspective Relations" (pp. 243–57), Mr. E. H. Moore discusses matters treated of by Von Staudt, Clebsch, Klein, and others.—Adopting the definition of *orientation* given by Laguerre, M. G. Humbert generalizes results previously obtained by Laguerre and himself in a memoir entitled "Sur l'orientation des systèmes de droites" (pp. 258–81), and also brings together some interesting properties of the hypocycloid given already by Cremona and Darboux.

Bulletin de l'Académie Royale de Belgique, April.—Contribution to the study of the albuminoid substances in the white of an egg, by MM. G. Corin and E. Bérard. It was recently shown by Halliburton that the albumen of the serum is a mixture of two or of three albumens, according to the nature of the animal, which coagulate under different degrees of temperature. Applying the same process of research to the albuminoids of the white of eggs, the authors find that five different albuminoid substances are present in this liquid: two globulines, coagulating at $+57°$ and $+67°$ C. respectively, and three true albumens, coagulating at $+72°$, $+76°$, and $+82°$. Besides these new facts, they also offer some interesting remarks on the general character of the relations existing between the albumens and the globulines, and on the opalescence observed when these substances begin to coagulate under the action of heat.—M. F. Folie describes a new method of determining the constant of aberration by means of a series of observations of one and the same star in right ascension. For this method he claims great simplicity, and exemption from the numerous sources of error to which other processes are liable.—To this number of the *Bulletin*, A. F. Renard contributes an exhaustive memoir on the prevailing geological formations of the Cape Verd Islands.

Rendiconti del Reale Istituto Lombardo, May.—On an old theory regarding the climate of Quaternary times, by Prof. T. Taramelli. Reference is made to the theory announced in 1840 by Lombardini, who considered that the Quaternary climate was simply a continuation of those of previous epochs, modified by the appearance of more elevated lands upheaved in post-Tertiary times. This anticipates by twenty years Frankland's remarks on the physical causes of the Glacial epoch, and leads the author to formulate a vulcanico-glacial theory based on the views of these physicists and of Charpentier.—Meteorological observations made at the Brera Observatory, Milan, during the month of April.

Rivista Scientifico-Industriale, May 15.—Remarks on the earthquake at Florence on November 14, 1887, by Prof. P. G. Giovannozzi. Following the system adopted by Serpieri, the author has collected data from various quarters showing that the disturbance was of a purely local character. The chief shock, although so violent as to have been heard by the deaf, passed through the city with such velocity that very little damage was done. It presented all the characters of a true gaseous explosion, taking a vertical direction from a moderate depth below the crust of the earth, and absolutely unconnected with any volcanic phenomena. It is noteworthy that the earthquake followed a long and exceptional period of wet weather, during which a rainfall of 225mm. was recorded within the zone of disturbance.

SOCIETIES AND ACADEMIES.

London.

Royal Society, May 17.—"On Æolotropic Elastic Solids." By C. Chree, M.A., Fellow of King's College, Cambridge. Communicated by Prof. J. J. Thomson, F.R.S.

On the multi-constant theory of elasticity, the equations connecting the strains and stresses contain 21 constants. As shown by Saint-Venant, these reduce for one-plane symmetry to 13, for three-plane symmetry to 9, and for symmetry round an axis perpendicular to a plane of symmetry to 5.

Part I. of this paper deals with one-plane symmetry. A solution is obtained of the internal equations of equilibrium complete so far as it goes. It is employed in solving the problem, already treated by Saint-Venant, of a beam, whose length is perpendicular to the plane of symmetry, held at one end, and at the other acted on by a system of forces, whose resultant consists of a single force along the axis of the beam, and of a couple about any line in the terminal section through its centroid. The case when the cross-section is elliptical, and the beam exposed to equilibrating torsional couples over its ends is also treated. Results are obtained confirmatory of Saint-Venant's. They are also extended to the case of a composite cylinder, formed of shells of different materials whose cross-sections are bounded by concentric similar and similarly situated ellipses, the law of variation being the same for all the elastic constants of the solution. The limiting case of a continuously varying structure is deduced.

When a beam of circular section is exposed to torsion, it is proved that warping will ensue proportional to the moment of the twisting couple. Only two diameters in the cross-section, and these mutually at right angles, remain perpendicular to the axis of the beam.

Part II. treats of a material symmetrical round an axis, that of z, and having the perpendicular plane one of symmetry. A general solution of the internal equations of equilibrium is obtained, supposing no bodily forces to act. The solution involves arbitrary constants, and consists of a series of parts, each composed of a series of terms involving homogeneous products of the variables, such as $x^l y^m z^{n-l-m}$, where l, m, n are integers, and n is greater than 3. The terms involving powers of the variables, the sum of whose indices is less than 4, are then obtained by a more elementary process, and these alone are required in the applications which follow.

The first application of the solution is to "Saint-Venant's problem" for a beam of elliptical cross-section. The problem is worked out without introducing any assumptions, and a solution obtained, which is thus directly proved to be the only solution possible if powers of the variables above the third be neglected.

Part III. consists of an application of the second portion of the solution of Part II. to the case of a spheroid, oblate or prolate, and of any eccentricity, rotating with uniform angular velocity round its axis of symmetry, which is also the axis of symmetry of the material. The surface of the spheroid is supposed free of all forces.

The limiting form of the solution, when the polar axis of the spheroid is supposed to diminish indefinitely, is applied to the case of a thin circular disk rotating freely about a perpendicular to its plane through its centre. The solution so obtained is shown to satisfy all the conditions required for the circular disk, except that it brings in small tangential surface stresses. According to this solution the disk increases in radius, and diminishes everywhere in thickness, especially near the axis, so as to become biconcave. All, originally plane, sections parallel to the faces become very approximately paraboloids of revolution.

Again, by supposing the ratio of the polar to the equatorial diameter of the spheroid to become very great, a surface is obtained which differs very little from that of a right circular cylinder. The corresponding form of the solution obtained for the spheroid, when the ratio of the polar to the equatorial diameter becomes infinite, may thus be expected to apply very approximately to a long thin cylinder. This is verified directly, and it is shown that this solution is in all respects as approximately true as that universally accepted for Saint-Venant's problem. According to the solution the cylinder shortens, and every cross-section increases in radius but remains plane.

Part IV. treats of the longitudinal vibrations of a bar of uniform circular section and of material the same as in Part II. Assuming strains of the form—

$$\text{radial} = r\psi(r)\cos(pz - \alpha)\cos kt,$$
$$\text{longitudinal} = \phi(r)\sin(pz - \alpha)\cos kt,$$

$\phi(r)$ is found in terms of $\psi(r)$ by means of the equations established in Part II. From these equations is deduced a differential equation of the fourth order for $\psi(r)$, and for this a solution is obtained containing only positive integral even powers of r. A relation exists, determining all the constants of the solution in terms of the coefficients a_0 and a_2 of r^0 and r^2. In applying this solution to the problem mentioned, terms containing powers of r above the fourth are neglected, and it is shown to what extent the results obtained are approximate.

On the curved surface, the two conditions that the normal and tangential stresses must vanish lead to the following relation between k and p—

$$k = p\left(\frac{M}{\rho}\right)^{\frac{1}{2}}\{1 - \tfrac{1}{4}p^2a^2\sigma^2\}.$$

Here ρ denotes the density and a the radius of the beam, while M is Young's modulus, and σ the ratio of lateral contraction to longitudinal expansion for terminal traction. This agrees with a result obtained by Lord Rayleigh ("Theory of Sound," vol. i. § 157) on a special hypothesis.

Proceeding to the terminal conditions, it is shown how p is determined from the conditions at the ends. Since a_0 depends only on the amplitude of the vibrations, we are left with no arbitrary constant undetermined. If the bar be so "fixed" at its ends that the radial motion is unobstructed, this leads to no difficulty, but if an end be "free" a difficulty arises. At such an end the solution requires the existence of a radial stress $\propto (2i + 1)^3 r (a^2 - r^2)/l^3$, where i is an integer depending on the number of the harmonic of the fundamental note, and l denotes the length of the bar. There will thus be a difference in these cases between the results of experiment and those of the accepted theory, even as amended by Lord Rayleigh. This divergence will increase rapidly with the order of the harmonic, and, though very small for a long thin bar, will increase rapidly as the ratio of the diameter to the length is increased. Since, in dealing with the conditions at the curved surface, terms of the order $(a/l)^5$ were neglected, the same remarks apply, though to a smaller extent, in the case of the "fixed-fixed" vibrations.

May 31.—"Investigations on the Spectrum of Magnesium. No. II." By Profs. Liveing and Dewar.

Since our last communication on this subject, we have made many additional observations on the spectrum of magnesium under various circumstances, and have arrived at some new results. Speaking generally, we find that differences of temperature, such as we get in the flame of burning magnesium, in the arc, and in the spark, produce less differences in the spectrum than we had before attributed to them. For instance, the lines which previously we had observed only in the spark discharge, we have since found to be developed in the arc also, provided the discharge occur between electrodes of magnesium.[1] In making these experiments we used thick electrodes of magnesium, and brought them together inside a glass globe about 6 inches in diameter, fitted with a plate of quartz in front

[1] Compare the appearance of the lines of hydrogen in the arc discharge. Roy. Soc. Proc., vol. xxx. p. 157; and vol. xxxv p. 75.

and filled from time to time with various gases. The arc was an instantaneous flash which could not be repeated more than twice without rendering the sides of the vessel opaque with a complete coating of magnesium. It was therefore analogous to an explosion of magnesium vapour. The strong blue line λ4481, two pairs about λ3895, 3893, and λ3855, 3848, the strong pair about λ2935, 2927, and the two weaker lines of the quadruple group, namely λ2789·9 and 2797, all come out in the arc given by a Siemens dynamo between magnesium electrodes in air, in nitrogen, and in hydrogen. We have observed most of them also when the arc is taken in carbonic acid, in ammonia, in steam, in hydrochloric acid, in chlorine, and in oxygen. The observations render doubtful the correctness of the received opinion that the temperature of the spark discharge is much higher than that of the arc. Heat, however, is not the only form of energy which may give rise to vibrations, and it is probable that the energy of the electric discharge, as well as that due to chemical change, may directly impart to the matter affected vibrations which are more intense than the temperature alone would produce.

The Bands of the Oxide.

The set of seven bands in the green, beginning at about λ5006·4 and fading towards the violet side of the spectrum, which we have before attributed to the oxide of magnesium, have been subjected to further observation, and we have no reason to doubt the correctness of our former conclusion that they are due either to magnesia or to the chemical action of oxidation. On repeating our experiments with the spark of an induction coil between magnesium electrodes in different gases at atmospheric pressure, we could see no trace of these bands in hydrogen, nitrogen, or ammonia, whether a Leyden jar was used or not. Nor could we see them at all in carbonic oxide, but in this case the brightness of the lines due to the gas might prevent the bands being seen if they were only feebly developed. On the other hand, the bands come out brilliantly when the gas is oxygen or carbonic acid, both with and without the use of a Leyden jar. In air and in steam they are less brilliant, but may be well seen when no jar is used. When a jar is used they are less conspicuous, because in air the lines of nitrogen come out strongly in the same region, and in steam the F line of hydrogen becomes both very bright and much expanded.[1] It seems, therefore, that it is not the character of the electric discharge, but the nature of the gas which determines the appearance of the bands; and the absence of the bands in the absence of oxygen, and their increased brilliance in that gas, leave little room for doubt that they are due to the oxide, or to the process of oxidation. If a very small piece of magnesia, such as a fragment of the ash of burnt magnesium ribbon, be held in an oxy-hydrogen jet, most of the spectrum of burning magnesium is developed in the flame for a short distance from the piece of magnesia. Under these circumstances, the flame shows the *b* group and the magnesium-hydrogen series close to it, the bands in the green, the triplet near L, the triplet near M of the flame of burning magnesium, with the group of bands in that region, and the line λ2852. It is remarkable that the proportions in which the oxygen and hydrogen are mixed affect the relative intensities of different parts of the spectrum. In general, both the metallic lines of the *b* group and the bands of the oxide are easily seen; but if the oxygen be in excess the bands of the oxide come out with increased brightness, while the *b* group fades or sometimes becomes invisible. On the other hand, if the hydrogen be in excess the bands fade, and the *b* group shows increased brilliance. There can hardly be much difference in the temperature of the flame according as one gas or the other is in excess, but the excess of oxygen is favourable to the formation and stability of the oxide, while excess of hydrogen facilitates the reduction of magnesium and its maintenance in the metallic state. As regards temperature, it should be observed that while substances merely heated by the flame, and not undergoing chemical change, are not likely to rise to a temperature above the average temperature of the flame, it will be otherwise with the materials of the flame itself and other substances in it which are undergoing chemical change, and have at the instant of such change the kinetic energy due to the change.

[1] Neither the arc of a Siemens dynamo, nor that of a De Meritens magneto-electric machine, when taken in a crucible of magnesia, shows these bands, even if metallic magnesium be dropped into it. A stream of hydrogen led into the crucible with a view to cool it does not elicit them. When the arc is taken in the open air, and metallic magnesium dropped through it, the bands appear momentarily, but that is probably the result of the burning of the magnesium vapour outside the arc. (May 23.)

In fact, when chemical changes are occurring in a flame it cannot be taken for granted that the temperatures of the molecules are all alike, or that the vibrations which they assume are the result of heat alone. On the other hand, the temperature of the metal separated from magnesia by the oxyhydrogen flame cannot, we suppose, be at a temperature higher than that of the hottest part of the flame. We are therefore inclined to think that the metallic lines (*b*) are manifested at a lower temperature than the bands of the oxide; and the appearance of a line in the position of the first band without any trace of the second band (which is nearly as bright as the first), and without any trace of the *b* group, is quite sufficient to create a suspicion of mistaken identity when Mr. Lockyer ascribes the sharp green line in the spectrum of nebulæ to this band of magnesia. This suspicion will be strengthened when it is noticed that the line in question is usually in the nebulæ associated with the F line of hydrogen, if it be borne in mind that the spark of magnesium in hydrogen does not give the bands, and that the oxyhydrogen flame hardly produces them from magnesia when the hydrogen is in excess.

In Mr. Lockyer's map of the spectrum of the nebula in Orion (Roy. Soc. Proc. vol. xliii. p. 134), he has represented three lines in the position of the edges of the first three of these bands. If these three lines were really seen in the nebula, there would be less room to doubt the identity of the spectra; but the authorities quoted for the map (*loc. cit.*, p. 142) mention only a single line in this position.

When the flame of burning magnesium is viewed with a high dispersion, these bands are resolved into series of fine, closely set lines. Seven such series may be counted, beginning at the approximate wave-lengths 5006·4, 4995·6, 4985·4, 4973·6, 4961·6, 4948·6, 4934·4, respectively. When a condensed spark is taken between magnesium electrodes in oxygen mixed with a little air, the pair of strong nitrogen lines may be seen simultaneously with the bands, and lying within the first band, the bright edge of the band being somewhat less refrangible than the less refrangible of the two nitrogen lines.

When the bands are produced by the spark discharge between magnesium electrodes in oxygen or other gas, we have not been able to resolve them into lines, but the whole amount of light from the spark is small compared with that from the flame, and besides it is possible that the several lines forming the shading may be expanded in the spark, and thus obliterate the darker spaces between them.

Triplet near M and adjacent Bands.

Our former account of the spectrum of the flame of burning magnesium included a description of a triplet near the solar line M, and a series of bands extending from it beyond the well-known triplet near L. As we had not observed these features in the spectrum of the spark or arc, and could not trace their connection with any compound, we concluded that they were produced by magnesium only at the comparatively low temperature of the flame. We have since found that they are not produced by the metal at that temperature only, but are exhibited as strongly, or even more strongly, in the arc between electrodes of magnesium. In the latter case they appear concurrently with the line at 4481 and other lines which seem to belong to high temperatures. We must therefore regard them as not only produced at the temperature of flames, but as persistent at temperatures very much higher.

The different circumstances under which we have observed this triplet are as follow:—

In the oxyhydrogen flame when a very small piece of magnesia is held in it. In this case the outer two lines of the triplet are much stronger than the middle line (λ3724 about), which in some of our photographs does not show at all. It should be noticed that the least refrangible of the three lines (λ3730 about) is in general more diffuse and not quite so bright as the two more refrangible lines. Magnesia in the oxyhydrogen flame also gives rise to some bands close to and more refrangible than the triplet, and to another still more refrangible but less bright triplet, in which the lines are set at nearly equal distances from each other, with the approximate wave-lengths 3633·7, 3626·2, 3620·6. These additional bands and triplets are not really absent from the flame spectrum, for traces of them may be seen in some of our photographs of the magnesium flame, but they seem relatively brighter in the oxyhydrogen flame with magnesia, and the longer exposure of the photographic plate in the latter case helped to bring them out. They seem to come out more strongly under the conditions which make both the green bands of the oxide and the *b* group show well.

We have not noticed the more refrangible triplet (λ3633·7 to 3620·6 about) under other circumstances, but the triplet near M is produced when magnesia is held in the flame of cyanogen burning in oxygen, in the flash of pyroxylin with which magnesium filings have been mixed, or which has been treated with an alcoholic solution of magnesium chloride.

It is not only very strongly developed, but shows strongly reversed on our photographic plates, in the spectrum of the arc from a Siemens dynamo taken between electrodes of magnesium in oxygen; and most of the accompanying ultra-violet bands of the magnesium flame spectrum are at the same time reversed. It is less strongly, but distinctly, reversed in the spectrum of the same arc taken in air, in carbonic acid gas, and in sulphurous acid gas. It appears also if the arc is taken in ordinary nitrogen unless great precautions are taken to exclude all traces of oxygen or carbonic acid, when it completely disappears. It is developed also in the flash produced when a piece of magnesium ribbon is dissipated in air by the discharge through it of the current from 50 cells of a storage battery. Also in the spark in air at atmospheric pressure between magnesium electrodes connected with the secondary wire of an induction coil when the alternating current of a De Meritens magneto-electric machine is passed through the primary.

In two cases, but only two, we have found this triplet, or what looks like one or both of the more refrangible of its lines, developed in vacuous tubes. In both tubes the gas was air. One had platinum electrodes and a strip of magnesia from burnt magnesium disposed along the tube; the other had fragments of the Dhurmsala meteorite attached to the platinum electrodes. The discharge was that of an induction coil worked in the usual way without a Leyden jar. In each case it is only in one photograph of the spectrum that the lines in question appear. In other photographs taken with the same tubes they do not show.

On the other hand, this triplet does not make its appearance in the arc from a dynamo between magnesium electrodes in hydrogen, coal gas, cyanogen,[1] chlorine, hydrochloric acid, or ammonia; nor in the arc from a De Meritens machine in hydrogen or nitrogen. It does not show in the spark between magnesium electrodes of an induction coil used in the ordinary way, either with or without a Leyden jar, in hydrogen or in air at atmospheric pressure; nor in the glow discharge in vacuous tubes with magnesium electrodes when the residual gas is either air, oxygen, hydrogen, carbonic acid gas, or cyanogen. Nor does it appear, except in the one instance above-mentioned, in the glow discharge in highly rarefied air in a tube containing either magnesia or a strip of metallic magnesium.

A review of all the circumstances under which the triplet near M and its associated bands appear, and of those under which they fail to appear, leads pretty conclusively to the inference that they are due not to merely heated magnesium but to the oxide, or to vibrations set up by the process of oxidation.

We have expended a vast amount of time and trouble over vacuous tubes, and our later experiments do but confirm the opinion which we had previously formed that there is an uncertainty about them, their contents and condition, which makes us distrustful of conclusions which depend on them. Photographs of the ultra-violet spectra given by such tubes tell tales of impurities as unexpected as they are difficult to avoid. Every tube of hydrogen which we have examined exhibits the water spectrum more or less, even if metallic sodium has been heated in the tube, or the gas dried by prolonged contact with phosphoric oxide. Indeed the only tubes which do not show the water spectrum have been filled with gases from anhydrous materials contained in a part of the tube itself; and even when tubes have been filled with carbonic acid gas from previously fused sodium carbonate and boracic anhydride the water spectrum is hardly ever absent. The last traces of the ultra-violet bands of nitrogen are almost as difficult to be rid of with certainty. Frequently, unknown lines or bands make their appearance, and the same tube will at different times exhibit wholly different spectra. This is especially the case with tubes of rarefied gases which oppose much resistance to the passage of the electric discharge, such as oxygen.

[1] In taking the arc in this way in cyanogen our photographs show the whole of the five bands of cyanogen between K and L well reversed. We have before noticed (Roy. Soc. Proc., vol. xxxiii. p. 4) the reversal of the more refrangible three of these bands against the bright background of the expanded lines of magnesium when some of that metal was dropped into the arc between carbon electrodes, but in taking the arc between magnesium electrodes in an atmosphere of cyanogen the bright wings of the expanded magnesium lines near L extend beyond the cyanogen bands, and the whole series of the latter are well reversed. (May 23.)

The ultra-violet magnesium lines which we have observed in vacuous tubes with magnesium electrodes, when the induction coil, without jar, is employed, are the triplets at λ3837, and the lines λ2852, 2802, and 2795. These appear whether the residual gas be air, oxygen, hydrogen, or carbonic acid. When a jar is used we have obtained also the triplets at P and S, the pair about λ2935 and 2927, and the quadruple group near λ2802 and the quintuple group beyond, and *in one case only*, in oxygen, the group near *s*, described below, and the flame-triplet near M. When no jar is used sometimes only λ2852 is to be seen, sometimes λ2852 and the strong pair near λ2802, and sometimes also the triplet near L. We infer, therefore, that this is the order of persistency of these lines under the circumstances.

Group near "s."

In their list of lines in the spectrum of magnesium (Phil. Trans., 1884, p. 95) Messrs. Hartley and Adeney have given two lines, λ3071·6 and λ3046·0, which we had not heretofore observed either in the spectrum of the flame, arc, or spark of magnesium; but in our recent observations we have noticed in many cases a well-marked line which, by interpolation between neighbouring iron lines, appears to have a wave-length about 3073·5, and a pair of narrow bands sharply defined on their less refrangible sides at wave-lengths about 3050·6 and 3046·7, and fading away on their more refrangible sides.

The circumstances under which this group is seen and is not seen, do not seem to indicate that its emission is connected with any particular temperatures so much as with the character of the electric discharge, and perhaps also with the density of the magnesium vapour.

Royal Microscopical Society, May 9.—Dr. C. T. Hudson, President, in the chair.—The President said that on the occasion of his taking the chair for the first time, he desired, before beginning the business of the evening, to thank the Fellows very heartily for the honour which they had done him in electing him their President.—Mr. Crisp exhibited a form of camera lucida by M. Dumaige, of Paris, fitted in a box with a cover, which, when closed, kept the prism and mirror free from dust; also by the same maker, an adapter with spiral springs, for rapidly changing objectives, and a portable microscope in which the foot and stage were in one piece.—Dr. Kibbler exhibited and described a new stand and camera, which, he believed, would be found very useful for photomicrography. It had been made to his design by Mr. Bailey, his idea being that it was best not to take negatives upon a large plate, but on a quarter-plate first, and afterwards to enlarge the pictures from the original negatives. The great advantage of this method was in the amount of light gained for the purpose of focussing. The quarter-plate size was also the proper one for lantern slides. The ordinary diaphragm plate placed immediately below the stage he had found entirely useless, but by removing it a certain distance from the object it then ceased to cut off the field, and began to reduce the light and to improve the penetration and definition. With high powers this answered very well, but it would not work with low powers unless the diaphragm was removed to a distance too great to be convenient in practice. He had therefore devised the plan of introducing a short 1½-inch condenser behind the stage, and about 3 inches in front of the diaphragm plate, in this way throwing it out of focus. The effect of this was that the same improvement in penetration and definition was obtained, but on a much shorter distance. Attention was also called to a method of clamping the object in position when the focus had been obtained; also to a plan for obtaining a fine adjustment by a tangent screw.—Mr. Mills's note on a sponge with stelliform spicules was read.—Mr. Crisp referred to some comments which had recently been made in America upon the advantages of the method of tilting the stage of the microscope as a means of obtaining a very economical and simple fine adjustment, on which some discussion took place.—Dr. A. C. Stokes's paper on new Infusoria Flagellata from American fresh-waters, containing descriptions of twenty new species, was read.—A paper on the Foraminifera of the Red Chalk, by Messrs. H. W. Burrows, C. D. Sherborn, and Rev. G. Bailey, was also read.

PARIS.

Academy of Sciences, June 4.—M. Janssen, President, in the chair.—On the equilibrium of a heterogeneous mass in rotation, by M. H. Poincaré. This is a generalization (worked out on a fresh basis) of M. Hamy's theorem of fluids in rotation. If all the surfaces of the several liquid layers in contact were ellipsoids, then all these ellipsoids would be homofocal, which is impossible unless all the layers be assumed of equal density.—On the rainbow, by M. Mascart. The results are here published of the author's researches on this phenomenon in connection with M. Boitel's recent communication on the supernumerary arcs of the rainbow.—Experimental researches on the action of the brain, by M. Brown-Séquard. The experiments with rabbits here described tend to show that the so-called motor centres and the other parts of one hemisphere of the encephalon may determine movements in both sides of the body through the influence of gravitation alone. This conclusion, while opposed to the generally accepted doctrines, is in harmony with the views advocated by M. Brown-Séquard in previous communications to the Academy. It is evident, he remarks, that the motor zone of each side of the brain is capable of producing movements in the corresponding members on either side, and not, as is commonly supposed, on that side alone which is opposed to the centre of irritation.—Observations of Sawerthal's comet made at the Observatory of La Plata with the Gautier 0·217m. equatorial, by MM. Beuf, MacCarthy, Salas, and Delgado. These observations cover the period from March 9 to April 2, 1888, and the position of the Observatory is given at lat. $-34^\circ 54' 30''\cdot 3$, long. W. of Paris 4h. 0m. 58s.—Determination of the ohm by M. Lippmann's electrodynamic method, by M. H. Wuilleumier. The true value of the ohm as worked out by this process is given by the relation $\frac{106 R'}{R}$, the resistance of the conductor between two given points A and B being $R = 0\cdot 301889 . 10^9$. The value thus obtained is represented by the resistance at 0° of a column of mercury with section 1mmq. and length 106·27cm.—On electro-chemical radiophony, by MM. G. Chaperon and E. Mercadier. By the method here adopted, the authors have succeeded in obtaining an electro-chemical radiophone whose effects are analogous to those of the selenium electric instruments, possessing equal intensity and being capable of like applications.—On the action of the alkaline phosphates on the alkaline earthy oxides, by M. L. Ouvrard. The author has made a comparative study of baryta, lime, and strontian, for the purpose of determining the nature of the compound substances that may be obtained by fusion of these bases and some of their salts with the alkaline phosphates.—On some new gaseous hydrates, by M. Villard. To those already known the author now adds analogous hydrates of methane, ethane, ethylene, acetylene, and protoxide of nitrogen. They are generally less soluble, less easily liquefied, than those previously obtained, and are decomposed at the respective temperatures of 21°·5, 12°, 18°·5, 14°, and 12°. It is shown in the case of methane and ethylene that a gas may form a hydrate above its critical temperature of liquefaction, and that these two gases have a critical temperature of decomposition considerably higher than the others.—Contribution to the study of the ptomaines, by M. Oechsner de Coninck. Having recently obtained a ptomaine in $C_8H_{11}N$, the author here determines by analysis a certain number of salts, and describes the preparation of the chloromercurates and iodomethylate.—On the development of the grain of wheat, by M. Balland. It results from these studies that wheat may be advantageously reaped eight or ten days earlier than is customary. During this latter period the grain ceases its independent growth, and may continue to complete its development just as well in the cut ear as on the standing stalk. The point is obviously of great importance to growers, who have thus so much more time to harvest their crops.—Influence of the organic temperature on convulsions produced by cocaïne, by MM. P. Langlois and Ch. Richet. Some experiments are described tending to show that the higher the temperature of the animal the more susceptible it becomes to the toxic effects of cocaïne. It is inferred that refrigeration should be a general method apt to diminish the effects of toxic substances causing convulsions.—On the chemical action and vegetative alterations of animal protoplasm, by M. A. P. Fokker. Continuing his already-described experiments, the author here shows that, besides the property of producing fermentations, protoplasm possesses that of undergoing vegetative changes, thus confirming his already expressed opinion that the formation of hematocytes is a case of heterogenesis.

STOCKHOLM.

Royal Academy of Sciences, June 6.—A review of the researches on the electricity of the air, by Prof. Edlund.—Researches on the elasticity and tenacity of metallic wires, by Dr. Isberg.—On the probability of finding large numbers in the development of irrational decimal fractions and of continued fractions, by Prof. Gyldén.—Researches on a non-linear differential equation of the second order, by the same.—On the forms and varieties of the common herring, by Prof. F. A. Smitt.—On the integration of the differential equations in the N body, problem iv., by Prof. Dillner.—New remarks on the genus Williamsonia, by Prof. A. G. Nathorst.—Contributions to the knowledge of the hydroids of the western coast of Sweden, by M. Segerstedt.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Poems in the Modern Spirit: C. Catty (Scott).—Rural Water Supply: C. L. Hett (Spon).—Contribution à la Météorologie Electrique, Notes: Prof. J. Luvini (Turin).—Natural History Transactions of Northumberland, Durham, and Newcastle-upon-Tyne, vol. ix. Part 2 (Williams and Norgate).—Morphologisches Jahrbuch, 13 Band, 4 Heft: C. Gegenbaur (Williams and Norgate).—Bulletin of the New York State Museum of Natural History, No. 3 (Albany).—Rapport Annuel sur l'Etat de l'Observatoire de Paris, 1887 (Gauthier-Villars, Paris).—Archives Italiennes de Biologie, Tome 9, Fasc. 3 (Loescher, Turin).—Zeitschrift für Wissenschaftliche Zoologie, 46 Band, 3 Heft (Williams and Norgate).—Botanische Jahrbücher, Neunter Band, 5 Heft (Williams and Norgate).—Geological Magazine, June (Trübner).—Journal of the Society of Telegraph-Engineers and Electricians, No. 73 (Spon).—Proceedings of the Bath Natural History and Antiquarian Field Club, No. 3, vol. vi. (Bath).—Hand-book of the Amaryllideæ: J. G. Baker (Bell).—Elementary School Atlas: J. Bartholomew (Macmillan).—A Season in Sutherland: J. E. Edwards-Moss (Macmillan).—The Encyclopædic Dictionary, vol. vii. Part 1 (Cassell).—Teoría Elemental de las Determinantes: F. Amorétti and C. M. Morales (Biedma, Buenos Ayres).—The Clyde from its Source to the Sea: W. J. Millar (Blackie).—General Physiology: Dr. J. G. M'Kendrick (MacLehose, Glasgow).—An Illustrated Manual of British Birds, Part 3: H. Saunders (Gurney and Jackson).—Die Natürlichen Pflanzenfamilien, 18 and 19 Liefg.: Engler and Prantl (Leipzig).—Ueber Kern- und Zelltheilung im Pflanzenreiche (Heft 1 of Histologische Beiträge): E. Strasburger (Fischer, Jena).—Sea-side and Way-side Nature Readers, No. 2: J. M. Wright (Heath, Boston).—Report on a Part of Northern Alberta and Portions of Adjacent Districts of Assiniboia and Saskatchewan: J. B. Tyrrell (Dawson, Montreal).—The Forest Flora of South Australia, Part 8: J. E. Brown (Adelaide).—Journal of the Chemical Society, June (Gurney and Jackson).

CONTENTS.

THURSDAY, JUNE 21, 1888.

THE STEAM-ENGINE.

The Steam-Engine. By G. C. V. Holmes. (London: Longmans, 1887.)

THIS treatise is intended as an elementary text-book for technical students. In many respects it fulfils its purpose, at least better than any book of moderate size with which we are acquainted. It is clearly written; its arrangement, if not the best possible, is orderly; it is so far practical that problems arising in the actual design and use of steam-engines are not ignored, but attacked in a sufficiently elementary way; and the *rationale* of processes involved in the use of steam is explained adequately and correctly on the whole. The woodcuts represent fairly good examples of construction, with the exception of one or two, like those of the injector and exhaust-ejector, which are antiquated, and one or two others so bad that they are obviously mere imaginary sketches. Nevertheless the book fails of being what a really good elementary text-book of the steam-engine might easily be—what, indeed, anyone of Mr. Holmes's competence would make it, if some experience in teaching had shown him the needs and difficulties of engineering students. It is a little to be feared that Mr. Holmes's book is marred by an attempt in part to adapt it to the requirements of some existing examinations on the steam-engine, which are more scrappy and less scientific than the worst of existing text-books. If only a really adequate practical and elementary text-book were written, it would control the examinations instead of needing to be adapted to them.

The treatise includes the mechanics, the thermodynamics, and rules for the design of steam-engines. The portions included under the last head are by far the weakest portions of the book. The scattered discussions of the strength of some portions of engines and boilers are too vague and general to be of practical value. The rules for the strength of fly-wheels at p. 246, and that for area of steam passages at p. 204, are examples of the kind of useless rules which stop short of encountering any one of the actual difficulties of ordinary designing. It is just these portions of the book which seem designed to meet the exigencies of a student cramming for an examination, and the book would be improved by their omission. An elementary treatise on the steam-engine might well leave questions of design on one side, and confine itself to a descriptive account of engines and boilers, with theory enough to explain the actions involved. But then it is neither necessary nor useful in such a treatise to introduce elementary physics and mechanics. A technical student may be assumed to know elementary science. "I have not assumed," says the author, "the slightest acquaintance on the part of the reader with the sciences of heat and motion, and have consequently devoted many pages to the explanation of such parts of these sciences as are necessary for the proper understanding of the working of engines." Hence we find a chapter on the nature of heat, including a discussion of the melting of ice, and the graduation of thermometers. There are definitions of mass, weight, force, and velocity, and arithmetical examples of the laws of motion. Surely all this would only be justifiable in an age when elementary books were scarce and dear. An ordinary student finds it a tiresome obstruction, when the way to the subject of the book is barred by such repetition. On the other hand, a brief but clear and critical account of the methods by which Regnault determined the fundamental constants for steam would have been very useful. It would have shown both the meaning of the terms used, and the probable trustworthiness of the determinations. In place of this, we find only verbal definitions and formulæ.

The thermodynamical portion of the book is probably its best and clearest part, and that in which it is most in advance of any quite elementary book of a similar kind. It must be understood that in criticizing this portion we do not ignore the fact that the author has done a service to elementary technical students.

On p. 67 a diagram is copied from Maxwell, and called a diagram of isothermals of dry saturated steam. It has escaped the author that for dry saturated steam there is no isothermal. At a point in the curve, say at 212°, the steam is saturated: on one side of this it is a mixture of steam and water, or conventionally wet steam; on the other side it is superheated steam. The saturation curve so useful in steam-engine calculations is nowhere mentioned. Further, in any modern treatment of the steam-engine it ought to be recognized that the engineer is always or almost always dealing not with dry saturated steam but with wet steam. The algebraical expressions for the total heat, &c., of wet steam should be introduced along with those for dry steam. Curiously, nowhere in this book can we find an expression for the latent heat of steam, though no quantity is so often required. The total heat is given, and so the latent heat can be inferred, but surely the ordinary approximate expression for latent heat is also useful.

In Chapter III. the theory of perfect engines is given. Following the precedent of treatises of wider scope, the author begins with the laws of expansion of permanent gases. Next the theorem about a reversible engine is given, but in a form in which it is restricted to the case of an air-engine. The efficiency of the reversible engine so obtained is afterwards spoken of as the efficiency of perfect heat-engines in general. But the independence of the result on the nature of the fluid employed is nowhere indicated. The diagram for a perfect steam-engine is given on p. 113. But no elementary student will perceive why the efficiency of this is the same as that of the air-engine, at least without explanation. The only idea ordinary students get from the theorem about the Carnot engine is that the efficiency of *any* engine is proportional to the range of temperature in the cylinder. In the case of the actual steam-engine this is so wrong as to be nearly the reverse of the truth, and the misconception is hardly anywhere adequately guarded against. It is very doubtful whether the Carnot engine ought to be introduced into the elementary theory of the steam-engine. An ordinary indicator-diagram can be taken, and the relation of the heat expended to the heat utilized determined. From the feed measurement and indicator-diagram the steam liquefied at the end of admission and at exhaust can be ascertained. The heat expenditure corresponding to work of admission, expansion, and expulsion can be calculated. From the condenser measurement the heat abandoned can be found, and an estimate formed of the loss by radiation. Repeating the calculation for different degrees of expansion, and perhaps for cases of a jacketed and unjacketed cylinder, really clear notions will be formed of the relative importance of the processes going on in the engine. All this can be done in a perfectly elementary way, and the student will soon perceive that it is in the direct study of the losses of heat, and not in attempts to realize the conditions of a Carnot engine, that improvement is to be sought.

We fail to see the use of reviving the antiquated empirical treatment of Navier and de Pambour given in Chapter IV. De Pambour's equations involve so many assumed quantities that they are practically useless. The author might have remembered that contrary to de Pambour's view the friction of an engine is not proportional to the load, but very nearly independent of it.

Chapter V. deals with the mechanics of the engine. But the simplest graphic methods for finding curves of crank pin effort and acceleration are not given. The next chapter, on slide-valve diagrams, is one of the clearest and most useful in the book.

In Chapter XI. the very difficult question of cylinder condensation is treated on the whole clearly and with insight. But the obscurities of this difficult part of the explanation of the steam-engine are, as might be expected, not quite removed. The author probably attaches much too great importance to radiation from the cylinder sides to the steam, and too little to conduction from the cylinder sides to the water lying on its surface. The following passage will certainly puzzle a student :—

"The second cause—excess of condensation over re-evaporation—is a most fruitful source of waste, and should be most carefully guarded against. It results in the continuous accumulation of water in the cylinder, and consequently causes an amount of waste which goes on increasing with each stroke."

Of course, if the accumulation is continuous the cylinder must get full, which is impossible. In steady working, initial condensation must exactly equal re-evaporation and water carried mechanically to the condenser. Priming and condensation during expansion may for the argument be neglected. What is prejudicial is not excess of condensation over re-evaporation, but retention of water in the cylinder after exhaust.

THE ANIMAL ALKALOIDS.

On the Animal Alkaloids, the Ptomaïnes, Leucomaïnes, and Extractives in their Pathological Relations. By Sir William Aitken, Knt., M.D., F.R.S., Professor of Pathology in the Army Medical School. (London: H. K. Lewis, 1887.)

A Treatise on the Animal Alkaloids, Cadaveric and Vital; or, The Ptomaïnes and Leucomaïnes chemically, physiologically, and pathologically considered in Relation to Scientific Medicine. By A. M. Brown, M.D. With an Introduction by Prof. Armand Gautier, of the Faculté de Médecine of Paris, &c. (London: Baillière, Tindall, and Cox, 1887.)

THE advancement of modern chemistry has increased our knowledge of the alkaloids occurring in the vegetable kingdom—bodies which are of great importance both in a therapeutical and a toxicological aspect. Since the year 1872, a new mode of natural origin of alkaloids has been discovered, viz. from animal sources, and the knowledge and investigation of these bodies have proved of great service in the study of both physiological and pathological chemical processes.

Ptomaïnes were first discovered in decomposing animal tissues, as their pseudonym of "cadaveric alkaloids" implies. Their presence in these dead tissues introduced a new factor in the *post-mortem* search for poisons in suspected cases—a factor, however, the importance of which has been somewhat exaggerated. A more important result of their discovery has been the explanation of the cases of poisoning by decayed animal foods, such as sausages, tinned and putrid meats, in which they have been found.

Further researches have, moreover, brought to light the fact that similar bodies of an alkaloidal nature may be produced within, and by, the living organism. In this case they may be considered as of "vital" origin, the products, that is, of the metabolism of protoplasm; or they may, in some cases, be the result of the decomposition of albuminoid bodies: in both cases, the term "leucomaïnes" has been used to designate them. A leucomaïne—peptotoxin—has, for example, been found by Brieger as a product of artificial peptic digestion; another has been discovered in the body of the sea-mussel (*Mytilus edulis*), and to its presence were ascribed the symptoms of poisoning which occurred in Wilhelmshaven, in many people who had eaten the shell-fish. These facts, of the origin of poisonous alkaloids by the decomposition of albuminoid bodies, and also in the living animal tissues, open out a wide field of research in pathology, and have perhaps led to more speculation than our present knowledge warrants.

The two books before us deal with the whole subject of poisonous alkaloids. Sir W. Aitken's small work owes its origin to an introductory lecture delivered by him at the Army Medical School at Netley. It is chiefly a short *résumé* of the work done on the subject. The second part of the *brochure* will be found of interest to medical men, as it gives the direction in which modern thought is tending with regard to the part played by poisonous alkaloids in the production of disease. The conclusions drawn can, in the present state of our knowledge, be considered merely as suggestions: many more facts must come to light before the *rôle* played by the "vital" alkaloids in pathological processes can be adequately, or even reasonably, discussed.

Dr. Brown's work is of a more ambitious nature, and purports to be a treatise on the subject of animal alkaloids generally. After commencing with a short history of the subject, the author proceeds to give an account of the methods for extraction of the alkaloids, and of the chemical and physiological properties of ptomaïnes; the "vital" alkaloids, leucomaïnes, being treated in a similar manner. The account of the methods of extraction might, we think, be made more practical by being considered a little more fully, as it is to this part of the book that workers in this field will turn for information.

Of the chemical and physiological properties of these alkaloids, a fairly complete account is given: our knowledge of these properties is, however, up to the present so imperfect, that the researches carried on during the last sixteen years only serve as a basis for future work. Much has yet to be done regarding the physiological action of these bodies; and no progress can be made in this respect until the alkaloids have been extracted in a pure state. It is almost useless, in the interests of science, to speak of the action of alkaloids extracted by various reagents; though, in certain cases of poisoning, the investigation of such an action may be of immediate utility. Dr. Brown has devoted much space to the consideration of the part played by the vital alkaloids in physiological or pathological conditions. In his account he has closely followed the views of M. Gautier, whose researches have thrown great light on the subject.

Dr. Brown's work may be recommended as giving a general account of the present state of our knowledge regarding these alkaloids. S. H. C. M.

PRACTICAL FORESTRY.

Practical Forestry: its Bearing on the Improvement of Estates. By Charles E. Curtis, F.S.T., F.S.S., Professor of Forestry, Surveying, and General Estate Management at the College of Agriculture, &c. (London: *Land Agent's Record* Office, 1888.)

THE present work is described as a reprint of a series of papers on "Practical Forestry," which appeared in the *Land Agent's Record*, and the author's object in republishing his ideas on practical forestry is to promote and encourage the study of true forestry among the British land-owners and land agents, and especially to impress upon students the necessity of acquiring a sound practical knowledge of a branch of land economy so long neglected and ignored. So far so good; but when the author says, "I trust this publication will be the means of spreading this object more widely" (*sic*), we fear that he will be grievously disappointed.

To begin with: the book is written in doubtful English. Though the correct use of the English language is not absolutely essential, yet in order to be a really useful work, a book should be written in language which complies with the ordinary grammatical rules, and which is also intelligible to the class of readers expected to profit by its perusal. The whole book is conceived in a very narrow spirit, and the expressed views of the author are frequently open to question. Take for instance the following passage (p. 40):—

"The great and true principle of thinning is to encourage the growth of those trees which are left, and not to secure a financial present return. This, though important, is quite a secondary consideration, and should at all times be ignored."

We beg to say that the great and true principle of thinning is nothing of the kind. In every instance the owner, or his manager, must consider what the objects of his management are. They may be:—

(1) To produce material of a certain description.

(2) To produce the greatest possible number of cubic feet per acre and year.

(3) To secure the highest possible money return from the property.

(4) To secure the highest possible interest on the invested capital.

(5) To improve the landscape, or to affect the climate, &c.

In each of these cases the method of thinning will be different.

Again, the description given of a true forester (p. 12) is somewhat illusory. If the author thinks that a man who has studied botany, vegetable physiology, geology, entomology, &c., is also able to wield the axe, and use with skill the pruning saw or knife, he is likely to be disappointed in nine cases out of ten. Such ideas are theoretical speculations, and not the result of practical experience.

The chapter on "Soil and Site" is of a very hazy description whenever the author attempts to rise above ordinary platitudes. He promises to describe clearly in future sections the nature of the soils and sites in which the individual trees most delight, but, as far as we can see, he has got over the difficulty by omitting to redeem his promise.

To sum up, the book is not likely to further the object which the author seems to have at heart. The experienced forester will find nothing new in it, and the beginner will only meet with badly arranged statements which are frequently not in accordance with the teaching of science or of practice. Sw.

OUR BOOK SHELF.

Tropical Africa. By Henry Drummond. (London: Hodder and Stoughton, 1888.)

THIS is a brightly-written and most interesting sketch of Mr. Drummond's experiences during a recent journey in East Central Africa. He has no very surprising or exciting adventures to describe, but in the course of his narrative, which is written with a vigour and grace unusual in books of travel, he contrives to convey a remarkably vivid impression of the country through which he passed. Going up the valley of the Shiré River, he visited Lake Shirwa, of which little has hitherto been known; then he went on to Lake Nyassa, and to the plateau between Lake Nyassa and Lake Tanganyika. During the whole of his journey he was a close observer, not only of the physical features of the districts he visited, but of the various classes of phenomena which interested him as a geologist, an ethnographer, and a student of natural history. In one admirable chapter he gives a full and striking account of the white ant, which he had frequent opportunities of studying; in another he brings together many curious illustrations of the well-known fact that among numerous species of animals mimicry is one of the means of self-protection. Before going to Africa, Mr. Drummond had mentally resolved not to be taken in by "mimetic frauds," yet he was "completely stultified and beaten" by the first mimetic form he met. This was an insect—one of the family of the *Phasmidæ*—exactly like a wisp of hay. Another insect, which he often saw, closely resembles a bird-dropping, and the consequence is that "it lies fearlessly exposed on the bare stones, during the brightest hours of the tropical day, a time when almost every other animal is skulking out of sight." Mr. Drummond has of course much to say about the chances of a great future for Africa, and in this connection he presents a good deal of valuable information as to the capacity of the natives for work and as to the wrongs inflicted upon them by vile gangs of slave-traders.

Plotting, or Graphic Mathematics. By R. Wormell, D.Sc., M.A. (London: Waterlow and Sons, Limited, 1888.)

THIS book is intended chiefly for those who have mastered the beginnings of algebra and Euclid, and so is very elementary. The method employed throughout is that of using squares, and preparatory exercises are first given to show the student the different purposes to which they may be applied with facility. Proportion and the determination of areas are the subjects of the first two chapters, followed by a chapter on the tracing of paths of projectiles, with various data. The sections of the cone, such as the parabola, ellipse, and hyperbola, are next described, with various methods of tracing them. The book contains a great number of numerical examples, and concludes with a chapter on the higher graphs and curves of observation.

The Elements of Logarithms. By W. Gallatly, M.A. (London: F. Hodgson, 1888.)

IN this little book of thirty-one pages the various rules and methods of treating logarithms are stated and explained in a simple and precise way, and those beginning the subject would do well to read through these few pages. Numerous examples are put in here and there, and at the end the author has added a collection of questions taken from the Woolwich and Sandhurst examination papers for the years 1880-87.

LETTERS TO THE EDITOR.

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Thunderstorms and Lightning Accidents.

As the season of thunderstorms and lightning accidents is now approaching, I hope you will kindly allow me to make known through your columns the fact that, in the interests of science, the Institute of Medical Electricity is very desirous of obtaining authentic information concerning lightning accidents, whether fatal or otherwise. I should therefore esteem it a favour if some of the many friends of humanity among your readers will assist us to investigate these phenomena by sending me such particulars of accidents of this nature as they may have personal or trustworthy knowledge of as soon after they occur as possible.

Of course, electrical and physiological details are what we most require, but trustworthy general information is often valuable, and will be gratefully received.

24 Regent Street, S.W. H. NEWMAN LAWRENCE.

Nose-Blackening as Preventive of Snow-Blindness.

I ONLY read Prof. Ray Lankester's letter the other day on the above, which appeared in NATURE of May 3 (p. 7). I have made inquiries among travellers in the snow regions of North America, and find the practice to be quite common and well known, but have met with no one who can explain it. I may say, however, that when I visited New Zealand in 1884 there were in one of the canoes which came off to our ship several naked natives, who had disfigured their faces by blackening their noses and eyes, and running a black fillet round the face, which gave them a villainous aspect; and I, in that insolent ignorance which seems to prevail with all pious people who have dealings with "the heathen of the isles," believed they had got themselves up in this way in order to frighten us. But it may well have been for other reasons. Certainly the sun's heat, reflected from the still waters of the sea, was quite as painful as any I ever felt in the regions of the silver snow. I subsequently found that the black used by these people, who are of a pale complexion, was the oxide of manganese, called in their tongue *labán*.

A. J. DUFFIELD.

The Delaware, Keweenaw Michigan, U.S.A., June 4.

The *Lethrus cephalotes*.

THE beetle which is described in your issue of June 7 (p. 134), by the British Consul at Varna, is probably the *Lethrus cephalotes*, which has proved so destructive to vineyards in East and South-East Europe. It is a dull black beetle, easily recognized by the swollen truncated ends of the antennæ; its length is about 21 mm. It lives chiefly in dry and sandy soil, and during dry weather the beetles leave their holes generally between nine and eleven in the morning and after three in the afternoon, to attack the tender parts of the vine, as Mr. Brophy describes.

Taschenberg is of the opinion that the buds, &c., of the vine which are dragged back to the holes of the beetles serve as food for the larvæ. As the beetles show a marked aversion to water, it is possible that the pest might be lessened by copiously watering the infected areas. ARTHUR E. SHIPLEY.

Christ's College, Cambridge, June 16.

Proposed Fuel-testing Station for London.

WILL you allow me to put before your readers the following proposition for the establishment of such a station, the desirability of which has been much impressed upon me within the last few years? So far as I know, there does not exist anything of the kind in this country where, as on the Continent, coals can be tested for their evaporative power, the gases of combustion analyzed, and all the results carefully reported on by experts. I subjoin a few details of the proposed station, with probable cost. It should, I consider, be placed on a perfectly independent footing, and managed by experts, under a small committee appointed by those who assist with money or otherwise. It might follow generally the lines of existing coal-testing stations, but with all modern improvements.

In this country it is remarkable that neither the sellers of coal take the trouble to find out how much heat they are offering, nor the purchasers how much they are getting for their money, and this notwithstanding the hundreds of millions of tons of coal changing hands yearly. Colliery-owners and coal-merchants, as well as the large consumers, know very little about coal calorimeters, although the former sell so much heat, and the latter try to utilize it to the best advantage. How few of the latter weigh their coal regularly, or keep any weekly record of the quantities of ashes and clinkers, to find out how much dirt and incombustible matter they are paying for! How few know what it costs them in fuel to evaporate one thousand gallons of water into steam, which is one of the best standards of comparison in a given district!

Locality.—The station might be in close proximity to a river, canal, or railway-station, so that the coals could be delivered easily and cheaply, and the steam allowed to escape under pressure without causing annoyance. A small piece of land doubtless could be obtained in such a situation at a low rent. The boiler-shed should be about 35 × 20 feet, with a small additional shed for storing the fuel.

Cost.—It would be desirable to allow at least £700 for a start, to cover the cost of the boiler-shed, chimney, 20 horse-power boiler (if such were considered large enough), and the special arrangements for measuring the feed-water with tanks, scales, feed-pump, injector, gas and coal analyzing apparatus, calorimeters, &c. Seeing that until the objects of the station become known it would probably not pay expenses, the help of guarantors would no doubt be necessary.

Yearly Expenses.—The charge for testing and reporting upon each combustible would probably more than cover eventually the salaries of a technical manager, his assistant, and the stoker. Some arrangement might possibly be made by which the manager and his assistant should only attend when required, at any rate at first, in order to diminish expenses.

The station would require to be advertised and made known in various ways. Colliery-owners would no doubt find it to their advantage to have their different kinds of coal tested and reported upon, so as to offer them to their customers with their ascertained heating value or evaporative power. Large consumers of coal (railway companies, water-works, and others) should know the heating value of the coal they are paying for, and the percentage of incombustibles.

I add a few notes on the temporary and permanent experimental heat stations known to me.

(1) The earliest fuel-testing station was established in 1847 at Brix, in Germany.

(2) Sir H. de la Beche and Dr. Lyon Playfair made a series of

experiments before the year 1851 with different coals suitable for the Navy. These trials were conducted near London, under a small marine boiler at atmospheric pressure.

(3) At the English Government dockyards, various interesting experiments have been made under small marine boilers, and the results published in Blue-books.

(4) Messrs. Armstrong, Longridge, and Richardson published in 1858 an account of some valuable experiments they had made with the steam-coals of the Hartley district of Northumberland, under a small marine boiler, for the Local Steam Colliery Association.

(5) At Wigan many excellent experiments were made by Messrs. Richardson and Fletcher about 1867, to test the value of Lancashire and Cheshire steam-coals for use in marine boilers. The water was evaporated under atmospheric pressure from a small marine boiler. This station was afterwards abolished.

In none of the above do the gases of combustion appear to have been analyzed.

(6) A fuel-testing station was worked at Dantzig in 1863.

(7) An important station was opened at Brieg, on the Oder, by the colliery-owners of Lower Silesia in April 1878, with the primary object of testing the value as fuel of the important coal-seams of that province. After working with the most satisfactory results for two years, and establishing the superiority of the Lower Silesian coal, the experiments terminated in 1880. The testing boilers had each 40 square metres of heating surface. Gases and coals were analyzed.

Existing Continental Stations.—(8) The Imperial Naval Administration Coal-testing Station at Wilhelmshaven, Germany, was established in 1877.

(9) Dr. Bunte's coal-testing station, erected at Munich about 1878, particulars of which have been published in the Proceedings of the Institution of Civil Engineers, vol. lxxiii. Here some hundreds of trials have been reported on and published; much valuable work has been done, and many fuels tested, including coals of the Ruhr valley, Saar basin, Saxon and Bohemian coal-fields, and those of Silesia and Upper Bavaria. The boiler of the station has about 450 square feet of heating surface. The gases and coals are analyzed, and all particulars carefully noted. It is one of the most complete stations I have seen.

(10) In Belgium, near Brussels, there is a Government station for testing fuels, under the administration of the Belgium State railways; locomotive boilers are used. The establishment has been at work for the last two years, but no results are published, as they are considered the property of the Government. Private firms can, however, have their coals tested and reported upon.

(11) The Imperial Marine Station, Dantzig.

(12) Boiler Insurance Company at Magdeburg.

The above is a slight outline of the work already done in this direction.

With the view of obtaining the opinions of those interested in starting a fuel-testing station, I ask you kindly to give this letter publicity. If the necessary sum can be raised, we may hope to have before long a practical and useful establishment in London, and to gain from it many interesting practical results respecting the combustion of fuels. BRYAN DONKIN, JUN.

Bermondsey, S.E., June 11.

The Geometric Interpretation of Monge's Differential Equation to all Conics—the Sought Found.

THE question of the true geometric interpretation of the Mongian equation has been often considered by mathematicians. In the first place, we have the late Dr. Boole's statement that "here our powers of geometrical interpretation fail, and results such as this can scarcely be otherwise useful than as a registry of integrable forms" ("Diff. Equ.," pp. 19-20). We have next two attempts to interpret the equation geometrically. The first of these propositions, by Lieut-Colonel Cunningham, is that "the eccentricity of the osculating conic of a given conic is constant all round the latter" (*Quarterly Journal*, vol. xiv. 229); the second, by Prof. Sylvester, is that "the differential equation of a conic is satisfied at the sextactic points of any curve" (*Amer. Journ. Math.*, vol. ix. p. 19). I have elsewhere considered both these interpretations in detail, and I have pointed out that both of them are irrelevant; the first of them is, in fact, the geometric interpretation, not of the Mongian equation, but of one of its five first integrals which I have actually calculated (Proc. Asiatic Soc. Bengal, 1888, pp. 74-86); the second is out of mark as failing to furnish such a property of the conic as would lead to a geometrical quantity which vanishes at every point of every conic (Journal Asiatic Soc. Bengal, 1887, Part 2, p. 143). In this note I will briefly mention the true geometric interpretation which I have recently discovered.

Consider the osculating conic at any point, P, of a given curve; the centre, O, of the conic is the centre of aberrancy at P, and as P travels along the given curve, the locus of O will be another curve, which we may conveniently call the *aberrancy curve*. Take rectangular axes through any origin; let (x, y) be the given point P, and α, β the co-ordinates of the centre of aberrancy. Then it can be shown without much difficulty that

$$\alpha = x - \frac{3qr}{3qs - 5r^2},$$

$$\beta = y - \frac{3q(pr - 3q^2)}{3qs - 5r^2},$$

whence

$$\frac{d\alpha}{dx} = \lambda \mathrm{T}, \quad \frac{d\beta}{dx} = \mu \mathrm{T},$$

where

$$\lambda = \frac{r}{(3qs - 5r^2)^2}, \quad \mu = \frac{pr - 3q^2}{(3qs - 5r^2)^2},$$

$$\mathrm{T} \equiv 9q^2t - 45qrs + 40r^3,$$

p, q, r, s, t being, as usual, the successive differential coefficients of y with respect to x.

If $d\psi$ be the angle between two consecutive axes of aberrancy, ds the element of arc, and ρ the radius of curvature of the aberrancy curve, we have

$$\rho = \frac{ds}{d\psi}, \quad ds^2 = d\alpha^2 + d\beta^2,$$

whence

$$\rho = (\lambda^2 + \mu^2)^{\frac{1}{2}} \cdot \mathrm{T} \cdot \frac{dx}{d\psi}.$$

But it is easy to show that

$$\frac{d\psi}{dx} = \frac{q(3qs - 5r^2)}{r^2 + (rp - 3q^2)^2},$$

so that

$$\rho = \mathrm{T} \cdot \frac{\{r^2 + (rp - 3q^2)^2\}^{\frac{3}{2}}}{q(3qs - 5r^2)^3}.$$

Now, $\mathrm{T} = 0$ is Monge's differential equation to all conics, and when $\mathrm{T} = 0$ we have $\rho = 0$. Hence, clearly, the true geometric interpretation of the Mongian equation is:

The radius of curvature of the aberrancy curve vanishes at every point of every conic.[1]

This geometrical interpretation will be found to satisfy all the tests which every true geometrical interpretation ought to satisfy, and I believe that this is the interpretation which, during the last thirty years, has been sought for by mathematicians, ever since Dr. Boole wrote his now famous lines. I will not take up the valuable space of these columns with the details of calculation: they will be found fully set forth in two of my papers which will be read next month before the Asiatic Society of Bengal, and will in due course be published in the Journal.

Calcutta, May 18. ASUTOSH MUKHOPADHYAY.

PERSONAL IDENTIFICATION AND DESCRIPTION.[2]

I.

IT is strange that we should not have acquired more power of describing form and personal features than we actually possess. For my own part I have

[1] The differential equation of all parabolas,

$$3qs - 5r^2 = 0,$$

is also easily interpreted, viz. calling the distance OP between the given point and the centre of aberrancy the *radius of aberrancy*, and the reciprocal of this (= I) the *index of aberrancy*, we have, easily,

$$\mathrm{I} = \frac{3qs - 5r^2}{3q\{r^2 + (rp - 3q^2)^2\}^{\frac{1}{2}}},$$

so that the interpretation is that *the index of aberrancy vanishes at every point of every parabola.*

[2] The substance of a Lecture given by Francis Galton, F.R.S., at the Royal Institution on Friday evening, May 25, 1888.

frequently chafed under the sense of inability to verbally explain hereditary resemblances and types of features, and to describe irregular outlines of many different kinds, which I will not now particularize. At last I tried to relieve myself as far as might be from this embarrasment, and took considerable trouble, and made many experiments. The net result is that while there appear to be many ways of approximately effecting what is wanted, it is difficult as yet to select the best of them with enough assurance to justify a plunge into a rather serious undertaking. According to the French proverb, the better has thus far proved an enemy to the passably good, so I cannot go much into detail at present, but will chiefly dwell on general principles.

Measure of Resemblance.—We recognize different degrees of likeness and unlikeness, though I am not aware that attempts have been as yet made to measure them. This can be done if we take for our unit the *least discernible difference*. The application of this principle to irregular contours is particularly easy. Fig. 1 shows two such contours, A and B, which might be meteorological, geographical, or anything else. They are drawn with firm lines, but of different strengths for the sake of distinction. They contain the same area, and are so superimposed as to lie as fairly one over the other as may be. Now draw a broken contour which we will call C, equally subdividing the intervals between A and B; then C will be more like A than B was. Again draw a dotted contour, D, equally subdividing the intervals between C and A; the likeness of D to A will be again closer. Continue to act on the same

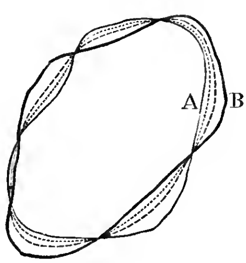


Fig. 1.

principle until a stage is reached when the contour last drawn is undistinguishable from A. Suppose it to be the fourth stage; then as $2^4 = 16$, there are 16 grades of least-discernible differences between A and B. If one of the contours differs greatly in a single or few respects from the other, reservation may be made of those peculiarities. Thus, if A has a deep notch in its lower right-hand border, we might either state that fact, and say that in other respects it differed from B by only 16 grades of unlikeness, or we might make no reservation, and continue subdividing until all trace of the notch was smoothed away. It is purely a matter of convenience which course should be adopted in any given case. The measurement of resemblance by units of least-discernible differences is applicable to shades, colours, sounds, tastes, and to sense-indications generally. There is no such thing as infinite unlikeness. A point as perceived by the sense of sight is not a mathematical point. but an object so small that its shape ceases to be discernible. Mathematically, it requires an infinitude of points to make a short line; sensibly, it requires a finite and not a large number of what the vision reckons as points, to do so. If from thirty to forty points were dotted in a row across the disk of the moon, they would appear to the naked eyes of most persons as a continous line.

Description within Specified Limits.—It is impossible to verbally define an irregular contour with such precision that a drawing made from the description shall be undistinguishable from the original, but we may be content with a lower achievement. Much would be gained if we could refer to a standard collection of contours drawn with double lines, and say that the contour in question falls between the double lines of the contour catalogued as number so-and-so. This would at least tell us that none of the very many contours that fell outside the specified limits could be the one to which the description applied. It is an approximate and a negative method of identification. Suppose the contour to be a profile, and for simplicity's sake let us suppose it to be only the portion of a profile that lies below the notch that separates the brow from the nose and above the parting between the lips, and such as is afforded by a shadow sharply cast upon the wall by a single source of light, such as is excellently seen when a person stands side-ways between the electric lantern and the screen in a lecture-room. All human profiles of this kind, when they have been reduced to a uniform vertical

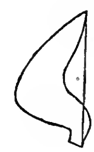


Fig. 2.

scale, fall within a small space. I have taken those given by Lavater, which are in many cases of extreme shapes, and have added others of English faces, and find that they all fall within the space shown in Fig. 2. The outer and inner limits of the space are of course not the profiles of any real faces, but the limits of many profiles, some of which are exceptional at one point and others at another. We can classify the great majority of profiles so that the whole of each class shall be included between the double borders of one, two, or some small number of standard portraits such as Fig. 3. I am as yet unprepared to say how near together the double borders of such standard portraits should be; in other words, what is the smallest number of grades of unlikeness that we can satisfactorily deal with. The process of sorting profiles into their proper classes and of gradually

Fig. 3.

building up a well-selected standard collection, is a laborious undertaking if attempted by any obvious way, but I believe it can be effected with comparative ease on the basis of measurements, as will be explained later on, and by an apparatus that will be described.

Classification of Sets of Measures.—Prisoners are now identified in France by the measures of their heads and limbs, the set of measures of each suspected person being compared with the sets that severally refer to each of many thousands of convicts. This idea, and the practical application of it, is due to M. Alphonse Bertillon. The actual method by which this is done is not all that could be theoretically desired, but it is said to be effective in action, and enables the authorities quickly to assure themselves whether the suspected person is or is not an old malefactor. The primary measures in the classification are four—

namely, the head length, head breadth, foot length, and middle-finger length of the left foot and hand respectively. Each of these is classified according as it is large, medium, or small. There are thus three, and only three, divisions of head lengths, each of which is subdivided into three divisions of head breadth; again, each of these is further subdivided into three of foot length, and these again into three of middle-finger length; thus the number of primary classes is equal to three multiplied into itself four times—that is to say, their number is eighty-one, and a separate pigeon-hole is assigned to each. All the exact measures and other notes on each criminal are written on the same card, and this card is stored in its appropriate pigeon-hole. The contents of each pigeon-hole are themselves sub-sorted on the same principle of three-fold classification in respect to other measures. This process can, of course, be extended indefinitely, but how far it admits of being carried on advantageously is another question. The fault of all hard-and-fast lines of classification, when variability is continuous, is the doubt where to find values that are near the limits between two adjacent classes. Let us take the case of stature, for

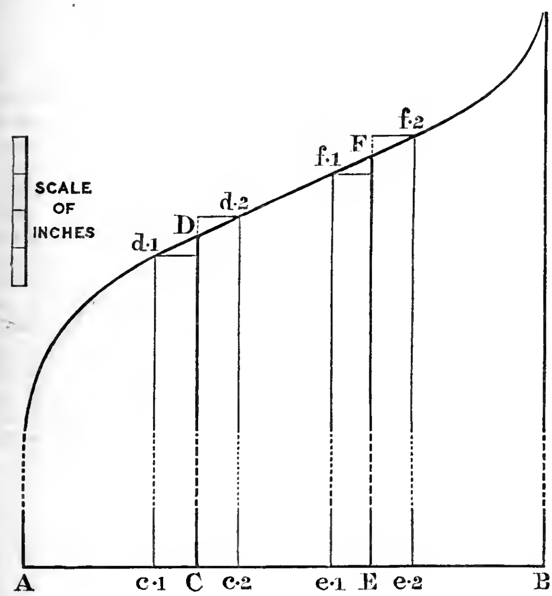


Fig. 4.

illustration of what must occur in every case, representing its distribution by what I have called a "scheme," as shown in Fig. 4.

Here the statures of any large group of persons are represented by lines of proportionate length. The lines are arranged side by side at equal distances apart on a base, A B, of convenient length. A curve drawn through their tops gives the upper boundary of the scheme; the lines themselves are then wiped out, having served their purpose. If the base A B be divided into three equal parts, and perpendiculars, C D, E F, be erected at the divisions between them, reaching from the base up to the curve, then the lengths of those perpendiculars are proportionate to the limiting values between the small and the medium group, and the medium and the large group, respectively. The difference between these perpendiculars in the case of stature is about 2·3 inches. In other words, the shortest and tallest men in the medium class differ only by that amount. We have next to consider how much ought reasonably to be allowed for error of measurement. Considering that a man differs in height by a full third of an inch between the time of getting up in the morning and lying down at night; considering also that measures are recorded to the nearest tenth of an inch at the closest, also the many uncertainties connected with the measurement of stature, it would be rash not to allow for a possible error of at least ± half an inch. Prolong C D, and note the points upon it at the distance of half an inch above and below D; draw horizontal lines from those points to meet the curve at $d.1$, $d.2$, and from the points of intersection drop perpendiculars reaching the base at $c.1$, $c.2$. A similar figure is drawn at F. Then the ratio borne by the uncertain entries to the whole number of entries is as $c_1c_2 + e_1e_2$ to AB. This, as seen by the diagram, is a very serious proportion. There is a dilemma which those who adopt hard and-fast lines of classification cannot avoid: either the fringe of uncertainty is dangerously wide, or else the delicacy with which measures are made is not turned to anything like its full account. If the delicacy is small, the fringe of uncertainty must be very wide; if the delicacy is great, the fringe will be narrow; but then the other advantages of possessing delicate observations are wasted through employing only a few classes. The bodily measurements are so dependent on one another that we cannot afford to neglect small distinctions. Thus long feet and long middle-fingers usually go together. We therefore want to know whether the long feet in some particular person are accompanied by particularly long, or moderately long, or relatively short fingers, though the fingers may in the two last cases be long as compared with those of the general population, and will be treated as long in M. Bertillon's system of classes. Certainly his eighty-one combinations are far from being equally probable. The more numerous the measures the greater would be their interdependence, and the more unequal would be the distribution of cases among the various possible combinations of large, small, and medium values. No attempt has yet been made to estimate the degree of their interdependence. I am therefore having the above measurements (with slight necessary variation) recorded at my anthropometric laboratory for the purpose of doing so. This laboraiory, I may add, is now open to public use under reasonable restrictions. It is entered from the Science Collections in the Western Galleries at South Kensington.

Mechanical Selector.—Feeling the advantage of possessing a method of classification that did not proceed upon hard-and-fast lines, I contrived an apparatus that is quite independent of them, and which I call a mechanical selector. Its object is to find which set out of a standard collection of many sets of measures, resembles any one given set within specified degrees of unlikeness. No one measure in any of the sets selected by the instrument can differ from the corresponding measure in the given set, by more than a specified value. The apparatus is very simple, it applies to sets of measures of every description, and ought to act on a large scale with great rapidity, and as well as it does on a small one, testing several hundred sets by each movement. It relieves the eye and brain from the intolerable strain of tediously comparing a set of many measures with each successive set of a large series, in doing which a mental allowance has to be made for a *plus* or *minus* deviation of a specified amount in every entry. It is not my business to look after prisoners, and I do not fully know what need may really exist for new methods of quickly identifying suspected persons. If there be any real need, I should think that this apparatus, which is contrived for other purposes, might, after obvious modifications, supply it.

The apparatus consists of a large number of strips of card or metal, *c1*, *c2* (Fig. 5), say 8 or 9 inches long, and having a common axis, A, passing through all their smaller ends. A tilting-frame, T, which turns on the same axis, has a front cross-bar, F, on which the tips of the larger ends of all the cards rest whenever the machine is left alone. In this condition a counterpoise at the other end of T suffices to overcome the weight of all the cards, and this heavy end of T lies on the base-board S. When the heavy end of T is lifted, as in Fig. 5, its front-bar is of course depressed, and the cards being individually acted on by their own weights are free to descend with the cross-bar unless they are otherwise prevented. The lower edge of each card is variously notched to indicate the measures of the person it represents. Only four notches are shown in the figure, but six could easily be employed in a card of eight or nine inches long, allowing compartments of 1 inch in length, to each of six different measures. The position of the notch in the compartment allotted to it, indicates the corresponding measure according to a suitable scale. When the notch is in the middle of a compartment, it means that the measure is of mediocre amount; when at one end of it, the measure is of some specified large value or of any other value above that; when at the other end, the measure is of some specified small value or of any other value below it. Intermediate positions represent intermediate values according to the scale. Each of the cards corresponds to one of the sets of measures in the standard collection. The set of measures of the given person are indicated by the positions of parallel strings or wires, one for each measure, that are stretched

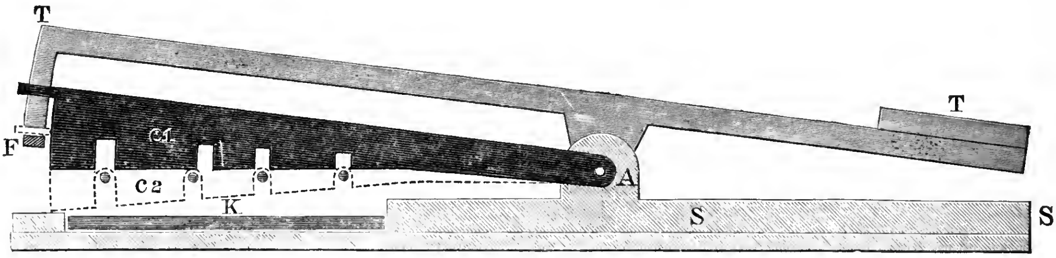


Fig. 5.—Section of the apparatus, but the bridge and rod are not shown, only the section of the wires.

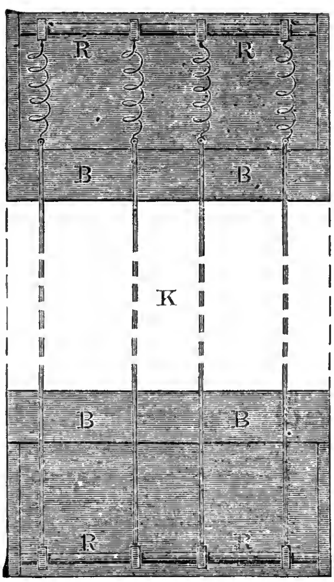


Fig. 6*a*.
Plan and section of the key-board K.

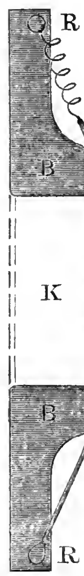


Fig. 6*b*.

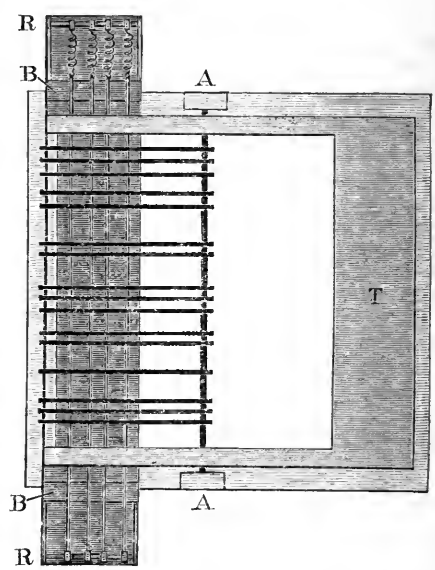


Fig. 7.—Reduced plan of complete apparatus.

Explanation:—A, the common axis; c_1, c_2, the cards; T, tilting-frame, turning on A (the cards rest by their front ends on F, the front cross-bar of T, at the time when the heavy hinder end of T rests on the base-board S); K, key-board, in which R, R are the rods between which the wires stretch; B, B, are the bridges over which the wires pass.

across bridges at either end of a long board set crossways to the cards. Their positions on the bridges are adjusted by the same scale as that by which the notches were cut in the cards. Figs. 6*a* and 6*b* are views of this portion of the apparatus, which acts as a key, and is of about 30 inches in effective length. The whole is shown in working position in Fig. 7. When the key is slid into its place, and the heavy end of the tilting-frame T is raised, all the cards are free to descend so far as the tilting-frame is concerned, but they are checked by one or more of the wires from descending below a particular level, except those few, if any, whose notches correspond throughout to the positions of the underlying wires. This is the case with the card c_2, drawn with a dotted outline, but not with c_1, which rests upon the third wire, counting from the axis. As the wires have to sustain the weight of all or nearly all the cards, frequent narrow bridges must be interposed between the main bridges to sustain the wires from point to point. The cards should be divided into batches by partitions corresponding to these interposed bridges, else they may press sideways with enough friction to interfere with their free independent action. Neither these interposed bridges nor the partitions are drawn in the figure. The method of adjusting the wires there shown is simply by sliding the rings to which they are attached at either end, along the rod which passes through them. It is easy to arrange a more delicate method of effecting this if desired. Hitherto I have snipped out the notches in the cards with a cutter made on the same principle as that used by railway guards in marking the tickets of travellers. The width of the notch is greater than the width of the wire by an amount proportionate to the allowance intended to be made for error of measurement, and also for that due to mechanical misfit. There is room for 500 cards or metal strips to be arranged in sufficiently loose order within the width of 30 inches, and a key of that effective length would test all these by a single movement. It could also be applied in quick succession to any number of other collections of 500 in each.

Measurement of Profiles.—The sharp outline of a photograph in profile admits of more easy and precise measurement than the yielding outline of the face itself. The measurable differences between the profiles of different persons are small, but they are much more numerous than might have been expected, and they are more independent of one another than those of the limbs. I suspect that measures of the profile may be nearly as trustworthy as those of the limbs for approximate identification—that is, for excluding a very large proportion of persons from the possibility of being mistaken for the one whose measurements are given. The measurement of a profile enables us to use a mechanical selector for finding those in a large standard collection to which they nearly correspond. From the selection thus made the eye could easily make a further selection of those that suited best in other respects. A mechanical selector also enables us to quickly build up a standard collection step by step, by telling us whether or no each fresh set of measures falls within the limits of any of those already collected. If it does, we know that it is already provided for; if not, a new card must be added to the collection. There will be no fear of duplications, as every freshly-added standard will differ from all its predecessors by more than the specified range of permitted differences. After numerous trials of different methods for comparing portraits successively by the eye, I have found none so handy and generally efficient as a double-image prism, which I largely used in my earlier attempts in making composite portraits. As regards the most convenient measurements to be applied to a profile for use with the selector, I am unable as yet to speak decidedly. If we are dealing merely with a black silhouette, such as the shadow cast on a wall by a small or brilliant light, the best line from which to measure seems to be B C in Fig. 8; namely, that which touches both the concavity of the notch between the brow and nose, and the convexity of the chin. I have taken a considerable number of measures from the line that touches the brow and chin, but am now inclined to prefer the former line. A sharp unit of measurement is given by the distance between the above line and another drawn parallel to it just touching the nose, as at N in the figure. A small uncertainty in the direction of B C has but a very trifling effect on this distance. By dividing the interval between these parallel lines into four parts, and drawing a line through the third of the divisions, parallel to B C, we obtain the two important points of reference, M and R. M is a particularly well-defined point, from which O is determined by dropping a perpendicular from M upon B C. O seems the best of all points from which to measure. It is excellently placed for defining the shape and position of the notch between the nose and the upper lip, which is perhaps the most distinctive feature in the profile. O L can be determined with some precision; O B and O C are but coarse measurements. In addition to these and other obvious measures, such as one or more to define the projection of the lips, it would be well to measure the radius of the circle of curvature of the depression at B, also of that between the nose and the lip, for they are both very variable and very distinctive. So is the general slope of the base of the nose. The difficulty lies not in selecting a few measures that will go far towards negatively identifying a face, but in selecting the best—namely, those that can be most precisely determined, are most independent of each other, most variable, and most expressive of the general form of the profile. I have tried many different sets, and found all to be more or less efficient, but have not yet decided to my own satisfaction which to adopt.

A closer definition of a profile or other curve, can be based upon the standard to which it is referred. Short cross-lines may be drawn at critical positions between the two outlines of the standard, and be each divided into eight equal parts. The intersection of the cross-lines with the outer border would always count as 0, that with the inner border as 8, and the intermediate divisions would count from 1 to 7. As the cross-lines are very short, a single numeral would thus define the position of a point in any one of them, with perhaps as much precision as the naked eye could utilize. By employing as

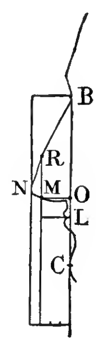


FIG. 8.

many figures as there are cross-lines in the standard, each successive figure for each successive cross-line, a corresponding number of points in the profile would be accurately fixed. Suppose a total of nine figures to be given, together with a standard collection of under a thousand doubly outlined portraits, each with six cross-lines. The first three figures would specify the catalogue number of the portrait to be referred to, and the remaining six figures would determine with much accuracy, six points in the outline of the portrait that it is desired to describe.

I have not succeeded in contriving an instrument that shall directly compare a given profile with those in a standard collection, and which shall at the same time act with anything like the simplicity of the above, and with the same quick decision in acceptance or rejection. Still, I recognize some waste of opportunity in not utilizing the power of varying the depths of the notches in the cards, independently of their longitudinal position.

I shall have next to speak of other data that may serve for personal identification, and especially on the marks left by blackened finger-tips upon paper.

(*To be continued.*)

SOAP-BUBBLES.

SOAP-BUBBLES fill the same happy position as do those charming books in which Lewis Carroll describes the adventures of Alice, in that they serve equally to delight the young and to attract the old. Clerk-Maxwell has mentioned the fact that on an Etruscan vase in the Louvre are seen the figures of children amusing themselves with bubbles, while to-day the same subject is being forced on the attention of the world by a strange development of modern enterprise. On the other hand, the bubble has occupied the minds of scientific men of all times. Sir Isaac Newton, Sir David Brewster, and Faraday, not to mention many others, devoted themselves to the soap-bubble as a means for investigating the subtleties of light. Plateau a few years ago delighted men of science with that wonderful book in which he, a blind man, expounded, in the clearest and most elegant manner, the result of years of labour on this one subject. Lately, Profs. Reinold and Rücker have employed the soap-film in investigations which tend to throw more light on the molecular constitution of bodies. These experiments will be remembered by all who saw them as being no less beautiful than instructive. The latest experiments with bubbles, which were shown by Mr. C. V. Boys to the Physical Society and at the Royal Society *conversazione*, and of which a full account is to be found in the May number of the *Philosophical Magazine*, depend upon no property which is not well known, and, unlike those referred to above, are not intended to increase our scientific knowledge; and yet no one would have ventured to predict that bubbles would submit to the treatment described in the paper, or would have expected such simple means to produce such beautiful results.

The first property of the soap-film turned to account is that strange reluctance of two bubbles to touch one another. Just as a bubble may be danced on the sleeve of a serge coat, or even embraced, without wetting the sleeve or being broken, so can two bubbles be pressed together until they are materially deformed without really touching one another at all. One bubble may be blown inside another, and if the heavy drops which accumulate at the bottom are removed, the inner one may be detached and rolled about within the outer one; or the outer one, held by two moistened rings of wire (Fig. 1),

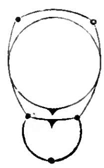


Fig. 1.

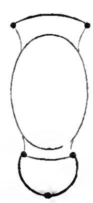


Fig. 2.

may be pulled out so as to squeeze the inner one into an oval form (Fig. 2), or may even be swung round and round, and yet the inner one remains free and independent, and when the outer is broken it floats gently away. If the inner one is coloured with the fluorescent material uranine, it shines with a green light, while the outer one remains clear as at first, showing that there is no mixture and no contact.

When the inner bubble is blown with coal gas, it rests against the upper side of the outer one (Fig. 3), pulling it

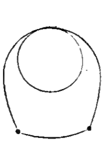


Fig. 3.

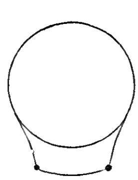


Fig. 4.

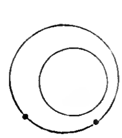


Fig. 5.

more and more out of shape as its size increases (Fig. 4). It can even be made to tear the outer one off the ring to which it was attached, after which the two bubbles rise in the air one inside the other. The outer bubble may be held by a light ring of thin wire to which thread and paper are attached, and then when an inner bubble of coal gas is blown, it will carry up the outer bubble, ring, paper, and all; and yet, in spite of this weight pressing them together, the inner bubble refuses to touch the outer one. If a little gas is let into the outer of two bubbles, the inner one will remain suspended like Mahomet's coffin (Fig. 5).

Diffusion of gas through a soap-film is shown by lowering a bell-jar of coal-gas over a bubble in which a second one is floating (Fig. 6). By degrees the gas penetrates the outer bubble, until the inner one, insufficiently buoyed up, gently sinks down.

The heavy and inflammable vapour of ether is made use of to show the rapidity with which the vapour of a liquid which will mix with the soap solution will penetrate through the walls of a bubble. A large

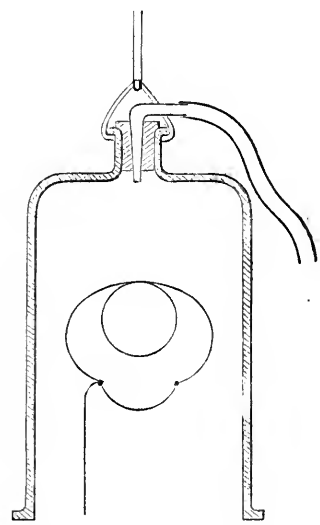


Fig. 6.

Fig. 7.

inverted bell-jar has some ether poured into it, after which bubbles blown with air in the usual way may be dropped into the jar, when they will float upon the vapour. They are then taken out and carried to a flame, when a blaze of light shows that the inflammable vapour has penetrated through the film. A bubble blown at the end of a wide tube and lowered into the vapour hangs like a heavy drop when removed; and if held in the beam of an electric light the vapour is seen oozing through the film and falling away in a heavy stream, while a light applied to the

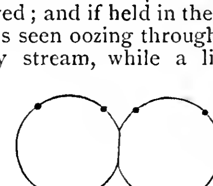


Fig. 8.

mouth of the tube fires the issuing inflammable vapour, and a large flame like that of a bunsen burner is the result (Fig. 7).

A variety of experiments are described in which bubbles are rolled along troughs made of soap-film—either straight, circular, or spiral—the prominent feature being that bubbles will roll upon or within one another as if they were made of india-rubber; they will even, where apparently in contact, take up the vibrations of a tuning-fork, and this will not force them to touch. There is one influence, however, which they cannot resist, and that is electrification. When two bubbles which are resting against one another (Fig. 8), provided that one is not within the other, are exposed to the influence of an even feebly electrified body, they instantly coalesce and become one (Fig. 9), and so act as a delicate electroscope. When one bubble is within the other, the outer one may be pulled out of shape by electrical action, and yet the inner one is perfectly screened from the electrical influence, thus showing in a striking manner that there is no electrical force within a conductor not even as near the surface as one side of a soap-film is near the other; for though the force outside is so great that the bubble is deformed, yet the fact that the inner one remains separate shows that the force within is too small to be detected. One of the experiments described shows at the same time the difference between the behaviour of two bubbles, one blown inside a third, and the other brought to rest against the third from the outside. Under these conditions, if electricity is produced

FIG. 9.

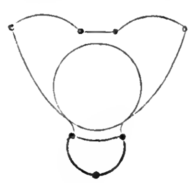


FIG. 10.

in the neighbourhood, the two outer bubbles become one, and the inner one, unharmed, rolls down and rests at the bottom of the now enlarged outer bubble (Fig. 10).

One experiment is described in which a cylindrical bubble is blown with oxygen gas between the poles of an electro-magnet. If the length is properly adjusted, the bubble breaks into two directly the exciting current is turned on, though the force due to the magnetic nature of oxygen is so feeble that not the slightest change of shape can be detected in a spherical bubble under the same conditions.

For other experiments and for details, readers are referred to the original paper in the *Philosophical Magazine*, the editor of which has kindly allowed us to reproduce the illustrations used in this article.

THE PARIS OBSERVATORY.

THE Annual Report of the Paris Observatory, which has recently appeared, draws special attention to the two events which have rendered the past year memorable, not merely in the history of the Observatory, but in that of astronomical science as a whole. The first of these was, of course, the meeting at Paris of the International Congress for the execution of the photographic chart of the heavens, and Admiral Mouchez gives the names of the members of the Congress, and the resolutions adopted by them. Of the Permanent Committee appointed by the Congress, Admiral Mouchez is himself the President, and he has already issued the first number of the *Bulletin de la Carte du Ciel*, future numbers of which will be brought out by the Committee as occasion may require. Twelve Observatories, including that of Paris, had definitely pledged themselves to join in the scheme, and five or six more expected to be able to do so shortly, so that there should be no difficulty in completing the chart within three or four years. The International Exhibition to be held at Paris next year would furnish a good opportunity for the reassembling of the Permanent Committee in order that the final decisions relating to the carrying out of this great scheme might be formed.

The other great event was the publication of the first two volumes of the great Paris Catalogue, the revision of the Catalogue of Lalande. This last work, which has already been referred to in NATURE (vol. xxxvii. p. 569), was commenced in 1855, but owing to many unfavourable circumstances has only been pushed forward vigorously during the last ten years, and now is all but completed. As the stars which still require observation have become fewer and more scattered, it has been found no longer necessary to devote more than one instrument to the work; the great meridian instrument has therefore been set apart for this work, and for the observation of minor planets and comparison stars, whilst the other meridian instruments have been left free for the careful study of the places of fundamental stars and for special researches. The "garden" circle has accordingly been used for the observation of circumpolars after M. Lœwy's plan, and the Gambey mural circle by M. Perigaud for the redetermination of the latitude of the Observatory. The value found for this latter by a series of *consecutive* observations of Polaris at upper and lower transit is 48° 50′ 12″·0, but Admiral Mouchez considers that despite the care and skill of M. Perigaud this determination falls short of the desired accuracy on account of the uncertainty of the corrections for refraction. This is partly due to the observations having all been made during midsummer, but chiefly to the bad position of the Observatory at the extreme south of Paris, the observations of Polaris therefore being made with the telescope pointed over the entire breadth of the city. It is hoped that the great Eiffel tower may render assistance to the study of refraction by affording much information as to inversions of the usual law of the variation of temperature with the height. The above value for the latitude still remains to be corrected for flexure of the instrument, and M. Perigaud is now undertaking the study of this error. The total number of meridian observations obtained during the year was 16,318, the highest monthly number having been secured in February, a most unusual circumstance. The observations of sun, moon, and planets amounted to 545.

The observations with the equatorials have been of the usual kind. M. Bigourdan has made 400 measures of nebulæ with that of the West Tower; and M. Obrecht, with the equatorial *coudé*, has made 720 measures of lunar craters referred to different points of the limb, in order to secure a better determination of the form of our satellite. But a yet more important work with this latter instrument has been the thorough examination of its theory by MM. Lœwy and Puiseux. In view of the success of the Paris telescope, of the number of similar instruments now under construction, and of the still wider popularity which the same form will probably have in the future, this was a work much to be desired.

The results, however, achieved in the field of astronomical photography are those in which, in view of the proposed chart, the greatest interest will be felt just now, and here the MM. Henry have further evidences of progress to present. Saturn and the moon have been photographed with a direct enlargement of 20 diameters. The phases of the lunar eclipse of August 3 have been recorded by the same means. With the smaller photographic instrument, aperture 4·3 inches, negatives have been obtained, one of which showed more than 30,000 stars on the single plate. Several curious new nebulæ have been discovered, one 1° in length near ζ Orionis; but the most remarkable have been those in the Pleiades. Two plates of this group, each with an exposure of four hours, have not only added much to our knowledge of the nebulæ round Electra, Merope, Maia, and Alcyone, these no longer appearing as mere faint clouds, but as well-marked nebulosities of intricate and complicated forms, but two new nebulæ are shown, both very narrow and straight, the longer one being some 40′ in length and but 2″ or 3″ in breadth, and threading together as it were no fewer than seven stars. The plate representing this photograph of the Pleiades, which is attached to the Report, shows 2326 stars, and comprises stars of the 18th magnitude, instead of the 1421 stars contained in the earlier photograph. MM. Henry have been likewise engaged in the study of the new instrument they have devised for the measurement of the stellar photographs, and in the preparation of tables of instrumental corrections, and of corrections for the effect of refraction; whilst M. Thiele has been inquiring into the degree of accuracy of which the measures are capable, with most encouraging results, and Admiral Mouchez considers that the precision thus attainable "will permit the carrying out under good conditions of the Catalogue of all the stars down to the 11th magnitude as decided by the Congress." It should be noted, however, that this interpretation of the resolution of the Congress has been challenged, and it has been urged that the Catalogue to be formed was to contain simply as many suitably placed stars as would be necessary as reference points for the great photographic chart, and that stars down to the 11th magnitude might be used for this purpose.

As to the publications of the Observatory, the first volume of the Catalogue, 0h.–6h. of R.A., is shortly to be followed by the second, 6h.–12h., the first sheets of which were already in the printers' hands. The volume of Observations for 1882 was published last August, that for 1883 was passing through the press, whilst the reduction of apparent to mean places was completely finished for 1884. The nineteenth volume of the Memoirs was in course of publication, and would contain, besides the works mentioned in the Report for 1886, a memoir on the theory of the figure of the planets, by M. Callandreau, and another on an allied subject, by M. Hamy. Amongst the works published by the individual members of the Observatory, the most important have been M. Lœwy's new method for the determination of the constant of aberration, and a work by M. Wolf, on the pendulum. M. Leveau is still engaged in his work upon Vesta, and M. Bossert is preparing for the determination of a definitive orbit of the Pons-Brooks comet. Under the head of "Matériel" the progress of the new equatorial *coudé* of 2 feet aperture and 60 feet focal length is referred to. Its completion is expected during the present year, but the building for it has not yet been begun.

The chief exception to the record of progress which Admiral Mouchez's Report supplies is found in the short paragraph which records the closing of the astronomical school, on financial grounds. The necessity for this step is to be most deeply regretted.

THE PHOTOGRAPHIC CHART OF THE HEAVENS.

WE lately reprinted from the *Observatory* (NATURE, May 10, p. 38) an article by the editors of that periodical on Dr. Gill's proposal that two million stars should be catalogued. The following is the reply of the editors, printed in the June number of the *Observatory*, to letters addressed to them on the subject by Admiral Mouchez and Mr. E. B. Knobel:—

We print above letters from Admiral Mouchez and from Mr. Knobel, concerning the remarks we made last month on Dr. Gill's proposition to catalogue 2,000,000 stars. There is a somewhat personal implication in both letters, to which we must at once reply before proceeding to treat of the real question at issue—a suggestion that we have been so emphatic in our disapproval of the scheme as to be discourteous to its supporters. We may perhaps venture to doubt whether either writer has done us the honour to read our remarks carefully enough. Admiral Mouchez "nous trouve bien sévère pour un projet aussi bien étudié et venant d'un savant aussi habile et compétent que le Directeur de l'Observatoire du Cap." We have not said a single word in disparagement of the skill and care with which Dr. Gill's paper has been written; we have vehemently objected to the question being raised at all; and that we have objected so vehemently may be taken as a full recognition of Dr. Gill's prominent position, which makes it a matter of necessity to bring all our forces to bear against a scheme which he chooses to advance. Mr. Knobel is perhaps more unjust to us. We have not in an unqualified manner characterized a catalogue of 2,000,000 stars as "an utter waste of time, labour, and money"; but we did use even stronger language about cataloguing stars "for the purpose only of getting their places written down," in order to call attention to the *reductio ad absurdum* of cataloguing towards which we very much fear there is some apparent tendency. And, finally, if we have been so emphatic as to be accused of exaggeration, let us again point out that a scheme, which we contend has not been assented to or even considered by the members of the Astrophotographic Conference, has been quietly launched, and is now so far under way that it is referred to by the President in the opening sentence of his letter as a matter already accepted by the "Comité permanent," and as only remaining to be discussed in detail. Surely it is time for those who have the welfare of the scheme really sanctioned by the Conference to raise their voices loudly in protest!

So much in explanation of the tone we have adopted in speaking of this proposal, and we now return to the letters. The main point of both is that this scheme of a catalogue of 2,000,000 stars has not been originated by Dr. Gill, but was really considered and approved by the Conference. As we have stated above, we hold the opposite opinion,—that although two resolutions of the Conference do mention *a* catalogue, this term cannot be supposed to sanction a catalogue of 2,000,000 stars without further specification. The Conference met to discuss the advisability of making a chart. With the invitations sent out to the various astronomers to attend this Conference there was sent a "programme provisoire" (which, it is to be very much regretted, was not that considered by the Congress). This first "programme provisoire" was dropped, and at the first *séance* of the Congress another was produced. In the first, in article 19 a *catalogue of reference stars* was mentioned, and properly so, but in the second there was no mention of any such catalogue. Mention was made in section 4 of a means of publishing the chart and the form of publication, but up to this time there was absolutely no question before the Conference of publication of a catalogue either of 2,000,000 or any other number of stars. There was no doubt a feeling amongst some astronomers present that a catalogue would be as useful, in their judgment, as the chart; and they took the opportunity of putting forward their views when the question of a second series of plates was brought forward. The taking of this second series of plates was proposed to meet an anticipated difficulty in photographing parts of the heavens where the stars differed greatly in magnitude. It was decided (Resolution 17) that a second series of plates should be taken, in order to insure the greatest precision in the micrometrical measurement of the stars of reference, and to render possible the construction of a catalogue. Here we have the first mention of a catalogue in the resolutions noted. A reference to the minutes of the Congress will show that this resolution was a compromise, for there had already been before the Congress a direct proposition (that of M. Tacchini) for a catalogue, which, however, was not voted upon. The resolution was in fact an endeavour to settle a question that was before the Congress, viz. whether the plates should be so taken as to be capable of accurate measurement; and this is decided by the specification that they shall render possible the construction of a catalogue. The next two resolutions speak of the second series of plates as *destinés à la construction du catalogue*, but nowhere is any direct resolution to be found as to the construction of a catalogue of all the stars.

If these resolutions need interpretation by the light of subsequent consideration at all, we may suggest a very different direction in which they might be modified in actual fact, and in which their spirit would yet be even better represented than by a literal fulfilment. It was pointed out that in taking the photographic plates of stars down to the 14th magnitude in parts of the sky where brighter stars existed, these with the exposure necessary to obtain the 14th magnitude would be very much over-exposed. And it was suggested that it would be advisable to take a second series of plates, as already mentioned (see Resolution 17). Now in some parts of the sky no second series of plates are, from this point of view, at all necessary; whilst in others not one or two, but many series of plates would be necessary in order to do justice to the various magnitudes in that particular part of the sky. For the present this is not the point at issue, but it may serve as an illustration of the sort of interpretation of the resolutions which we should consider legitimate.

In order to come to a proper judgment on the legitimacy of the derivation of Dr. Gill's proposal from the resolutions it is necessary to make some statements, which are not new, but of which the true significance does not seem to have been universally appreciated:—(1) When the plates are obtained they are actual representations of the stars as existing at a given time, and for every purpose except spectrum analysis are as good, if not better, than the visible heavens. If with these plates we have the absolute places of a certain small number of known stars, we have then all the data to make them valuable, either in the present or in the future. (2) The many questions concerning the stars which it is hoped a photographic chart of the heavens would do a great deal towards settling, such as their distribution, their proper motions, their changes of magnitude, and the presence of minor planets, of new stars and the like, can all be best treated by a direct comparison of plate with plate, in any of the various ways in which this can be done. (3) In order to obtain the best results from such an agent as photography it is necessary to use it in its own proper way; and astronomers must recollect that old methods of procedure adapted to other instrumental means may most probably be out of place. We might considerably enlarge on these statements, but for our present purpose it is sufficient to call attention to them.

Now, if Dr. Gill's catalogue were successfully constructed—and there are, alas! many difficulties in the way—its utility in the direction of comparison of our sky with that of the future is wholly limited by one condition, that in the future another exactly similar catalogue be constructed, occupying a similar time. Even then, if any changes were found by means of this comparison of catalogues which might very well be made in the course of fifty or one hundred years, the natural and indeed the proper thing to do would be to immediately compare the original plates. But can it be possible that any man or number of men really think of dealing with such a subject in such a way? If, on the other hand, the object of a catalogue be merely to allow of comets, minor planets, and other bodies being located, surely it would be better to measure the plates as occasion arises, and not to catalogue 2,000,000 stars on the off-chance of having some twenty or thirty positions to settle in the course of a year. And, further, such a catalogue would have this enormous disadvantage, that whilst in some parts of the sky stars of the 11th magnitude would be fairly well spread, in the Milky Way we should have stars clustered in such enormous quantities that it would be an extremely difficult thing to even identify them: in fact, speaking roundly, we should say that if such a catalogue were made, two-thirds of the stars catalogued would lie in the Milky Way. If, contrary to the opinion we have expressed, it is decided to form a very large catalogue, surely it would be better to determine the places of a certain number of stars, of such magnitudes as are found available, in each square degree, and make these the reference stars from which the positions of the other stars on the plate could be obtained.

We are therefore of opinion that, supposing limitless time and money available for such a purpose, the advantages of constructing this catalogue would be doubtful; but even if we waived all these objections and agreed that such a catalogue would be a "nice thing to have," or admitted that since men of the ability and reputation of Admiral Mouchez and Dr. Gill consider such a catalogue necessary it is heresy to inquire the why and wherefore, there would still be left the serious objection that to form a chart of the heavens is the first thing to do, and, take it in as simple a form as possible, it will quite possibly tax the energies of astronomers to their utmost; and that stellar photography being as yet in its infancy it is suicidal to attempt anything which will commit us to a course of action extending over more than a very few years. We could not give a better illustration of the dangers of the opposite procedure than has been supplied by Admiral Mouchez himself. In a recent article he has suggested that there have lately been such improvements in the sensitiveness of plates that we could now go to the 15th magnitude instead of the 14th. With a little ingenuity and less arithmetic it could easily be shown that the whole plan of operations would have become hopelessly futile and obsolete before half the time allowed by Dr. Gill for its completion had elapsed.

But not for one moment do we wish to appear lacking in sympathy with those who have spent and are spending so much time and thought on this subject; it is our great anxiety for the success of the work in which they are co-operating which makes us eager to protest as far as we can against the grand mistake of attempting too much.

THE INCURVATURE OF THE WINDS IN TROPICAL CYCLONES.

THE question of the incurvature of the winds in tropical cyclones is one of such importance to mariners, to enable them to judge their position in a storm, and to escape the hurricane around the central calm, that no apology is needed for adding my independent testimony to that of Prof. Loomis, whose conclusions, given at length in his recent well-known memoir, "Contributions to Meteorology," are quoted in Mr. Douglas Archibald's paper on M. Faye's work "Sur les Tempêtes" in last week's NATURE (p. 149).

In the preparation of a forthcoming work on the weather and climates of India and the storms of Indian seas, I have lately had occasion to re-investigate the above question in the case of cyclones in the Bay of Bengal, on the evidence afforded by the numerous original memoirs and reports prepared by Messrs. Willson, Eliot, Pedler, and other officers of the Indian and Bengal Meteorological Departments; my object being the practical one of determining directly the bearing of the storm-centre from a ship's position; and instead, therefore, of measuring the angle between the wind direction and the nearest isobar, as was done by Prof. Loomis, I have measured with a protractor the angle included between the former and its radius vector, in all cases in which the position of the storm's centre has been ascertained on sufficient evidence. In one other important condition I have also departed from the method pursued by Prof. Loomis. I have restricted the measurements to wind observations of ships at sea, within the influence of the storm, and to those of good observatories on the coast, subject to the same proviso; and have taken no account of those of inland observatories. This difference of procedure is probably the reason that the amount of the incurvature shown by these measurements is somewhat different from that obtained by Prof. Loomis, though the general fact of a great incurvature is thoroughly confirmed. My results are as follow:—

(1) The mean of 132 observations between lats. 15° and 22°, within 500 miles of the storm-centre, gives the angle 122° between the wind direction and its radius vector.

(2) The mean of 12 observations between the same latitudes, within 50 miles of the storm-centre, gives the angle 123°.

(3) The mean of 68 observations between N. lats. 8° and 15°, within 500 miles of the storm-centre, gives the angle 129°.

The observations within 50 miles of the storm-centre in the south of the Bay are too few to afford any trustworthy result.

For seamen's guidance, the following practical rules may be formulated:—

(1) In the north of the Bay of Bengal, standing with the back to the wind, the centre of the cyclone bears about five points on the left hand, or three points before the beam.

(2) In the south of the Bay, it bears about four points on the left hand, or four points before the beam.

(3) These rules hold good for all positions *within the influence of the storm*, up to 500 miles from the storm-centre. On the north and west the influence of the storm rarely extends to anything like this distance, but it does to the east and south.

Since much of this evidence, afforded by the Bay of Bengal cyclones, has been before the public for many years, it is incomprehensible to me how a man of M. Faye's scientific eminence can still assert that in the tropics "the wind arrows display an almost rigorous circularity." If, as may possibly be the case, he relies on the evidence of Mr. Piddington's memoirs, ignoring all subsequent work, it is only necessary to examine those memoirs to find that his data do not bear out that author's conclusions. In the charts which accompany Mr. Piddington's later memoirs, the wind observations are, as a rule, not shown, but only the ships' courses, and the author's interpretation of the positions and tracks of the storms. But the evidence is always fully given in the text, and it will be found that when the wind arrows are plotted therefrom, and are sufficiently numerous to allow of the position of the storm's centre being determined, which is far from being generally the case, they are reconcilable only with spiral courses, having a considerable incurvature.

I do not propose now to enter on a formal criticism of Mr. Piddington's work, the great merit of which, as that of a pioneer in the field of storm-science, no one more fully recognizes than myself; but so much seems necessary in explanation of the apparent glaring discrepancy between his results and those of modern workers in the same field.

The evidence of the cyclones of the Bay of Bengal, those tropical cyclones to which M. Faye appeals as authoritative on the validity of his views, is, then, conclusive against him. There is a strong influx of the lower atmospheric strata into a tropical cyclone, proving, in the most unquestionable manner, the existence of an ascending current over the vortex This fact is quite independent of any views that may be entertained as to any theory of cyclone origin and movement of translation, but any such theory must harmonize with the fact, and hence I conceive that it is fatal to M. Faye's views. With these, in so far as they are theoretical merely, I have no present concern, but it is obviously a matter of high importance to seamen that they should not be misled as to the facts of the wind's movement in cyclones, and it is because the promulgation of such views as M. Faye's tends to perpetuate an old and now exploded error of fact, that I have to put in my protest against them.

HENRY F. BLANFORD.

Folkestone, June 15.

NOTES.

It should have been stated in our paragraph last week relative to the opening of the Laboratory of the Marine Biological Association at Plymouth that the President, Prof. Huxley, who has given unremitting care to the affairs of the Association during the last three years, would be present if he were not prevented from taking part in any public proceedings by the state of his health. In the absence of the President, one of the Vice-Presidents of the Association, Prof. Flower, will preside. The Honorary Secretary, Prof. Ray Lankester, who founded the Association, and has conducted its affairs to the present issue, will also be present.

MR. J. J. H. TEALL, who now holds a foremost place among the petrographers of this country, has just been appointed to the Geological Survey. We understand that he will be specially charged with the study of the crystalline schists and the problems of regional metamorphism, and that he will be closely associated with the field officers who are mapping these rocks in different parts of Scotland. The Survey is to be heartily congratulated on this appointment. The staff is now remarkably strong, but the problems with which it is confronted are among the most difficult in geology. These problems have never been attacked by such a united force of field geologists and microscopists, who, working together with one common aim, will no doubt raise still higher the scientific reputation of the Survey, increase our knowledge of the history of the most ancient rocks, and throw light on some of the most puzzling questions in geological science.

THE electors to the Mastership of Downing College, Cambridge, have, by a unanimous vote, chosen Dr. Alexander Hill, Fellow of the College, to succeed Prof. Birkbeck. Dr. Hill's claim to the appointment sprang from his success as a teacher and worker in biology. No appointment to a Headship has been made on this ground alone since the revival of natural science at the Universities.

ON the 4th inst., Dr. Maxwell T. Masters was elected a corresponding member of the Institute of France, in the Botanical Section, in place of the late Prof. Asa Gray. Besides Dr. Masters, the following names appeared on the list of presentation: M. Treub, of Batavia; Mr. Triana, of Paris; M. Warming, of Lund; M. Wiesener, of Vienna. Dr. Masters obtained 39 votes; M. Triana, 5; M. Treub, 1.

THE Sorbonne, consulted as to the proposed creation of a Chair for the teaching of Darwinian theories, has not expressed disapproval of the scheme suggested by the Municipal Council of Paris. It has appointed a committee to report on the matter; and it is expected that no serious opposition will be offered to the proposal.

WE are glad to learn that a pension of £50 has been granted to Mrs. Balfour Stewart from the Civil List.

ON May 25, a complimentary dinner was given at the Queen's Hotel, Manchester, to Prof. Schorlemmer, of the Owens College, by his former pupils, to celebrate the occasion of the conferring of LL.D. upon him by the Senate of the Glasgow University, and to offer their congratulations. In the absence of Sir Henry Roscoe, who had been expected to take the chair, Mr. R. S. Dale, one of Prof. Schorlemmer's eldest pupils and friends, presided. Numerous congratulatory telegrams and letters were received by Dr. Schorlemmer, and early in the evening a letter was read from Sir Henry Roscoe, expressing regret that he could not be present, and testifying to his high appreciation of the ability of his old friend and colleague. Among those from whom congratulatory telegrams were received were Dr. Pauli, Director of the firm of Meister, Lucius, and Brüning, in Hoechst; Prof. Bernthsen, of the Badische Anilin und Sodafabrik, in Ludwigshafen; and Prof. Hermann Kopp, of Heidelberg, the historian of chemistry, who spoke of Prof. Schorlemmer's position as one of the principal pioneers of the science of organic chemistry and one of its foremost exponents, both as a teacher and a writer. Prof. Thorpe, F.R.S., proposed the health, long life, and prosperity of Dr. Schorlemmer, and referred to the fact that Glasgow, which had conferred honour on him, had produced such men as Black and Thomson, names familiar to all chemists.

DR. ASA GRAY left Harvard College in trust, to aid in the support of the Gray Herbarium of Harvard University, the copyrights of all his books, upon condition that proper provision should be made for the renewal and extension of these copyrights by new editions, continuations, and supplements, such as might be needed in the study of botany, and as might best enhance and prolong the pecuniary value of the bequest.

PROF. LOVERING has resigned the Chair of Mathematics and Natural Philosophy which he has held at Harvard for fifty years. In accepting his resignation, which takes effect in the autumn, the President and Fellows of the College have expressed warm appreciation of his services. Prof. Lovering has been President of the American Association, and still presides over the American Academy.

PROF. MCNAB, Swiney Lecturer on Geology in connection with the British Museum, wiil begin a course of twelve lectures on the fossil plants of the Palæozoic epoch on Monday next, at the Natural History Museum, Cromwell Road.

LAST night the *conversazione* of the Society of Arts took place at the South Kensington Museum.

A *conversazione* will be given by the Royal College of Surgeons, at the College, on Wednesday, June 27; and by the Royal Geographical Society, at Willis's Rooms, on Friday, June 29.

AN International Horticultural Exhibition is to be held at Cologne from August 4 to September 19.

WE have received from Messrs. West, Newman, and Co., samples of two kinds of botanical drying paper. One of the kinds differs but little from that which they have supplied for many years, which was originally manufactured, purposely for drying plants, by a paper-maker of the name of Bentall, who lived at Halstead in Essex, and contributed, a generation ago, to the distributions of the London Botanical Society. This paper has been largely used for the last thirty years, and combines in a very satisfactory manner the merits of a high degree of absorbence with a reasonable toughness. No doubt, for drying plants, it is the best paper that can be got, but yet, excepting grasses, Cyperaceæ, and mosses, one or more changes are required in the first few days to make satisfactory specimens in a climate like that of England. The new paper is quite without glaze, and seems a little more absorbent than the old "Bentall." The other kind is copied from an American model, a paper not made expressly for botanical use, sent to England by the late Dr. Asa Gray. It is twice as thick as the "Bentall," much more rigid, and very absorbent; a serviceable paper to mix with the lighter kind for home use, but too heavy to carry about in large quantities.

ACCORDING to *La Nature*, an immense terrestrial globe, constructed on the scale of one-millionth, will be shown at the Paris Exhibition of 1889. A place will be set apart for it at the centre of the Champ de Mars. The globe will measure nearly 13 metres in diameter, and will give some idea of real dimensions, since the conception of the meaning of a million is not beyond the powers of the human mind. Visitors to the Exhibition will see for the first time on this globe the place really occupied by certain known spaces, such as those of great towns. Paris, for instance, will barely cover a square centimetre. The globe will turn on its axis, and thus represent the movement of rotation of the earth. The scheme was originated by MM. T. Villard and C. Cotard, and *La Nature* says that it has been placed under the patronage of several eminent French men of science.

WE have received a sample of tobacco grown by Messrs. James Carter and Co., at a farm in Kent, and cured by Messrs. Cope Brothers and Co. It represents one of the first experimental crops brought to maturity, and passed through the various processes of manufacture, in this country, since the time of Charles II. The packet is accompanied by a card, on which we find the somewhat discouraging counsel: "Examine leisurely—use warily—smoke sparingly." Mr. Goschen was asked the other evening in the House of Commons whether he would cause an inquiry by experts into the results attending the experiment made by Messrs. Cope, with the view, if possible, of relaxing the fiscal restrictions upon the culture of tobacco in Great Britain. The Chancellor of the Exchequer cautiously replied that "only experience would show the value to smokers of this tobacco, and no inquiry by experts would be so valuable as that practical test. If any hon. member wished to try it, samples would be placed in the smoking-room. It was impossible to give any form of relaxation in the fiscal regulations which would injure the revenue."

ACCORDING to the *Kavkaz* newspaper, a shock of earthquake was felt at Julfa, in the Armenian province of Erivan, on May 15, about midday. The first shock was followed by a stronger one, which lasted for about three seconds, and seemed to have a direction from east to west.

THE Council of the Italian Meteorological Society held its first annual meeting at Turin on Sunday, April 15, under the presidency of Padre Denza. It was decided to hold the third general assembly of the Society at Venice, in September next, just before or after the Congress of the Alpine Club at Bologna. The establishment by the Society of a new Observatory in the Argentine Republic was notified, and also of four new meteorological stations in Italy. The arrangements being made with respect to the hygienic stations at five large cities were explained, as well as the proposed method of publication of the observations. The President submitted the Report of the Geodynamic Committee, nominated at the meeting at Aquila (NATURE, vol. xxxvi. p. 614), with reference to seismological observations and the protection of buildings. The Report, which is printed in the monthly Bulletin of the Italian Meteorological Society for May, consists of nine articles, and will be distributed to the Prefects and Mayors of districts liable to earthquake-shocks.

THE Hydrographer of the Admiralty has issued notices of the recent establishment of the following storm-signals:—(1) By the Japanese Government, at forty-seven stations on the coasts of Japan. A red ball, or one red light, to indicate that strong winds are probable from any direction. A red cone, or three red lights in the shape of a triangle, to indicate that strong winds are probable, at first from the northward or southward, according as the apex is upwards or downwards. (2) By the harbour authorities at Chittagong, relative to the signals at that port. A ball, or three lights placed vertically, to indicate that a severe cyclone is near Akyab, and will probably advance towards Chittagong. A drum, or two lights placed vertically, to indicate the early approach of a severe cyclone, with its attendant storm-wave. We take this opportunity of suggesting the desirability of introducing more uniformity in these signals in different countries, wherever practicable.

THE atomic weight of the element osmium has been re-determined by Prof. Seubert. The necessity for this re-determination has been felt ever since the principle of periodicity began to take firm root in the minds of chemists; and the more recent values arrived at for the atomic weights of iridium, platinum, and gold have tended to render this necessity even more imperative. The natural sequence, according to their chemical and physical properties, of the metals of the platinum group is generally accepted as—osmium, iridium, platinum, gold. Now the atomic weight of iridium as determined in 1878 by Seubert is 192·5, that of platinum as fixed by the same chemist in 1881 is 194·3, and that of gold as estimated last year by Thorpe and Laurie, and by Krüss, is 196·7, while the recognized atomic weight of osmium as given by Berzelius in 1828 is so high as 198·6. Obviously, if the grand conception of Newlands, Mendelejeff, and Lothar Meyer is correct, the atomic value of osmium required most careful revision. Such an undertaking, however, is endowed with peculiar interest owing to the dangerous nature of work with the osmium compounds, and many chemists who have been interested in this subject have been deterred by the knowledge that accidental contact with the fumes of the tetroxide, which are so frequently evolved by the spontaneous decomposition of many osmium compounds, might deprive them of the use of their eyes for ever. Prof. Seubert has happily succeeded without accident in establishing the validity of our "natural classification" by means of the analysis of the pure double chlorides of osmium with ammonium and potassium, $(NH_4)_2OsCl_6$ and K_2OsCl_6. Both these salts were obtained in well-formed octahedral crystals, of deep red colour while immersed in their solutions, but appearing deep black with a bluish reflection when dry, and yielding bright red powders on pulverization. The method of analysis consisted in reducing the double chlorides in a current of hydrogen: in case of the ammonium salt the spongy osmium which remained after reduction was weighed, and the expelled ammonium chloride and hydrochloric acid caught in absorption apparatus, and the total chlorine estimated by precipitation with silver nitrate. In case of the potassium salt the expelled hydrochloric acid was absorbed and determined, and the metallic osmium left after removal of the potassium chloride by washing was weighed. The mean value yielded by all these various estimations is 191·1, thus placing osmium in its proper place before iridium, and removing the last striking exception to the "law of periodicity."

AT a recent meeting of the Washington Society of Anthropology, Mr. H. M. Reynolds read a paper on Algonquin metalsmiths. He expressed the opinion that the working of the copper-mines of Lake Superior is not of such high antiquity as has been supposed, and that it may have been continued until comparatively modern Indian times.

SOME time ago the Smithsonian Institution issued inquiries as to the existence and geographical distribution of "rude and unfinished implements of the Palæolithic type." The *American Naturalist* says that responses have been received from thirty States and Territories. The implements already noted amount to between six and seven thousand, and their distribution extends nearly all over the United States. Several hundreds of implements—none of which seem to have been found in the mounds—have been sent to the Institution. The object of the Institution in undertaking this investigation was to determine whether there was in America a Palæolithic Age, and, if so, whether it had any extended existence.

THE Free Public Libraries and Museum of Sheffield seem to be in a most flourishing condition. According to the last Report, which has just been sent to us, there has been a steady increase in the number of books issued. The number issued during the year ending August 31, 1887, was 410,395. The number issued during the previous year was 399,653, so that there was an increase of 10,742.

MESSRS. LONGMANS, GREEN, AND CO. have sent us a series of their test cards in mechanics, packed in neat little cardboard cases. The questions on the many and various branches of the subject are arranged in three stages. Each stage consists of about thirty cards with six questions on each, and is supplemented by cards containing the answers to all the numerical questions. The questions are excellently chosen, and are arranged in an intelligible and progressive order.

A CAREFUL and very valuable bibliography of the works of Sir Isaac Newton, with a list of books illustrating his life and works, by G. J. Gray, has just been issued by Messrs. Macmillan and Bowes, Cambridge. The bibliography is divided into ten sections: (1) collected editions of works; (2) the "Principia"; (3) "Optics"; (4) "Fluxions"; (5) "Arithmetica Universalis"; (6) minor works; (7) theological and miscellaneous works; (8) works edited by Newton; (9) memoirs, &c.; (10) index.

A NEW edition of the late Prof. Humpidge's translation of Dr. Hermann Kolbe's "Short Text-book of Inorganic Chemistry" (Longmans) has been issued. The greater part of this edition was prepared by Dr. Humpidge last summer. Being unable, owing to failing health, to complete the task of revision, he asked Prof. D. E. Jones, of the University College, Aberystwith, to undertake it, and to see the book through the press.

A REPORT, with admirable illustrative maps, on the geology and natural resources of part of Northern Alberta, and the western parts of the districts of Assiniboia and Saskatchewan, by Mr. J. B. Tyrrell, Field Geologist of the Geological Survey of Canada, has just been published at Montreal. The Report is, to a certain degree, preliminary, but the author hopes that, for the present at all events, it may suffice as a guide to the extent, position, and character of the mineral wealth of the district.

AN interesting paper by Mr. Tyrrell, giving an account of the journeys of David Thompson in North-Western America, has been issued at Toronto. It was read lately before the Canadian Institute, and is published in advance of the Proceedings by permission of the Council. The materials for this narrative are contained in Mr. Thompson's field note-books and journals, which are preserved in the office of the Crown Land Department of Ontario. Mr. Thompson died in 1857 at the age of eighty-seven.

MR. LELAND will shortly send to the printer his work on "Americanisms," which will follow on the "Dictionary of Slang, Jargon, and Cant" now in the press. It will contain much folk-lore in the form of proverbs, songs, and popular phrases, and also the etymology and history of the words, as far as they could be traced. The work will include an account of American dialects, such as Pennsylvanian Dutch, Chinook, Creole, and Gumbo. A number of American scholars will deal with special subjects.

WE have received a copy of the *Tōyō Gakugei Zasshi* (the *Eastern Science Journal*), printed in Japanese characters. This magazine is published monthly, and is edited by a committee, most of whose members are Professors of the Imperial University at Tokio. Nearly 3000 copies of each number are sold.

THE first part of the second volume of the Journal of the College of Science, Imperial University, Japan, has been sent to us. The contents include, besides a mathematical paper, in German, by Dr. P. R. Fujisawa, the following articles in English: on the composition of bird-lime, by Dr. E. Drivers, F.R.S., and Michitada Kawakita; on anorthite from Miyakejima, by Yasushi Kikuchi; the source of *Bothriocephalus latus* in Japan, by Dr. Isao Ijima; and earthquake-measurements of recent years, especially relating to vertical motion, by S. Sekiya.

MESSRS. D. C. HEATH AND CO. (Boston) will publish at once Compayre's "Lectures on Pedagogy: Theoretical and Practical," a companion volume to their Compayre's "History of Pedagogy." It is translated and annotated by Prof. Payne, of the University of Michigan.

PROF. J. VIOLLE has just issued the first part of the second volume of his "Cours de Physique." The present part relates to acoustics.

WE reprint from *Science* of June 1, 1888, the following suggestive paragraph:—"The Committee of the House of Representatives on acoustics and ventilation has actually reported favourably a Bill appropriating seventy-five thousand dollars to subsidize a man who thinks he can construct a steel 'vacuum' balloon of great power. He is to be allowed to use the facilities of one of the navy-yards for the building of his machine, and is to have the money as soon as he has expended seventy-five thousand dollars of private capital upon his air-ship. One of the mathematical physicists of Washington was asked by a member of Congress whether such a balloon could be successfully floated. He set to work upon the problem, and here are some of his results, which are rather curious:—A common balloon is filled with hydrogen gas, which, being lighter than air, causes the balloon to rise and take up a load with it. But, as the pressure of the gas within is equal to the pressure of the atmosphere without, no provision other than a moderately strong silk bag is required to prevent collapse. The inventor of the proposed steel balloon hopes to gain greater lifting-power by using a vacuum instead of gas, the absence of substance of any kind being lighter than even hydrogen gas. But he has to contend with the tendency of the shell to collapse from the enormous pressure of the atmosphere on the outside, which would not be counterbalanced by anything inside of it. The first question which presented itself was, How thick could the metal of the shell be made, so that the buoyancy of the sphere, which would be the most economical and the strongest form in which it could be constructed, would just float it without lifting any load? The computations showed that the thickness of the metal might be ·000055 of the radius of the shell. For example: if the spherical shell was one hundred feet in diameter, the thickness of the metal composing it could not be more than than one-thirtieth of an inch, provided it had no braces. If it was thicker, it would be too heavy to float. Now, if it had no tendency to buckle, which of course it would, the strength of the steel would have to be equivalent to a resistance of more than 130,000 pounds to a square inch to resist absolute crushing from the pressure of the air on a cross-section of the metal. Steel of such high crushing-strength is not ductile, and cannot be made into such a shell. If the balloon is to be braced inside, as the inventor suggests, just as much metal as would be used in constructing the braces would have to be subtracted from the thickness of that composing the shell. Of course, such a shell would buckle long before the thickness of the metal of which it was composed was reduced to ·000055 of its radius. In other words, it is mathematically demonstrated that no steel vacuum balloon could be constructed which could raise even its own weight. This is an illustration of how intelligently Congress would be likely to legislate on scientific matters unguided by intelligent scientific advice."

THE additions to the Zoological Society's Gardens during the past week include two Pig-tailed Monkeys (*Macacus nemestrinus* ♂ ♀) from Java, presented by Mr. C. W. Ellacott; a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented by Mr. J. Wiltshire; a Pig-tailed Monkey (*Macacus nemestrinus*) from Java, presented by Mrs. Gleig; two Spotted Cavys (*Cœlogenys paca* ♂ ♀) from South America, presented by Mr. W. H. Stather; a Mauge's Dasyure (*Dasyurus maugæi*) from Australia, presented by Mr. H. R. Brame; three Abyssinian Sheep (*Ovis aries*, var.) from Abyssinia, presented by Mr. A. J. Baker; two Pallas's Sand Grouse (*Syrrhaptes paradoxus*) from the Island of Tiree, Argyllshire, presented by Lieut.-Colonel Irby and Captain Savile Reid, F.Z.S.; a Wapiti Deer (*Cervus canadensis* ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE CONSTANT OF ABERRATION.—In the year 1862, Prof. J. S. Hubbard commenced a series of observations of α Lyræ with the prime vertical instrument of the Washington Naval Observatory, which was continued by either Profs. Newcomb, Harkness, or Hall until 1867. The purpose of these observations had been to obtain corrections to the assumed values of the constants of nutation and aberration, and to afford an absolute determination of the annual parallax of the star. The series was not continued for a sufficient period for the first purpose; and Prof. Asaph Hall, when engaged on the determination of the parallax of α Lyræ by another method, found that these observations would give it a small negative value. From this and other circumstances he was at that time induced to think the observations would not repay the trouble of a careful discussion; but recently, reflecting that they had been skilfully designed, and carried out with care, he resolved to ascertain the result they would furnish for the constant of aberration. The observations commenced 1862 March 25, and extended to 1867 April 25, and were 436 in number. The mean resulting value of the parallax is—

$$\pi = -0''\cdot079 \pm 0''\cdot0134,$$

whilst

$$\text{Constant of aberration} = 20''\cdot4506 \pm 0''\cdot0142,$$

with an average probable error for a single observation of $\pm 0''\cdot174$.

Adopting a parallax of $+0''\cdot15$, the result would be—

$$\text{Constant of aberration} = 20''\cdot4542 \pm 0''\cdot0144.$$

Prof. Hall prefers this latter result, notwithstanding the uncertainty as to the true parallax of the star. The negative result obtained for the parallax may probably be due to the fact that the coefficient of parallax obtains its extreme values in January and July, when the mean temperature is likewise at its extreme points; the January observations also are made in daylight, but the July at night, which would tend to produce a systematic difference in the method of observing. The coefficient of aberration, on the other hand, has its greatest values in April and October, when the conditions of observation will be nearly the same.

The above value of the constant of aberration gives, for the solar parallax—

$$\pi = 8''\cdot810 \pm 0''\cdot0062,$$

Hansen's values of the mean anomaly of the earth, and eccentricity of its orbit being assumed, together with Clarke's value for the equatorial radius, and Michelson and Newcomb's determination of the velocity of light, viz. 186,325 miles per second.

THE MARKINGS ON MARS.—The observations of M. Perrotin at Nice, and M. Terby at Louvain, and, in England, of Mr. Denning at Bristol, have confirmed the presence on the planet of most of the "canals" or narrow dark lines which were discovered by M. Schiaparelli in 1877, and at subsequent oppositions. M. Perrotin has also been able to detect, in several cases, the gemination or doubling of the canals, and M. Terby has observed the same phenomenon in one or two cases, but with much greater difficulty than in the opposition of 1881–82. But some curious changes of appearance have been noted. An entire district (Schiaparelli's *Lybia*) has been merged in the adjoining "sea," *i.e.* its colour has changed from the reddish hue of the Martial "continents" to the sombre tint of the "seas." The district in question is larger than France. To the north of this district a new canal has become visible, and again another new canal has appeared to traverse the white North Polar cap, or, according to M. Terby, to divide the true Polar cap from a white spot of similar appearance a little to the south of it. With the exception of these changes, the principal markings, both light and dark, are those which former oppositions have rendered familiar.

COMET 1888 *a* (SAWERTHAL).—The following ephemeris for Berlin midnight is by Herr Berberich (*Astr. Nach.*, No. 2838), from elliptic elements which he has found for it, and which closely resemble those of Prof. Boss given in NATURE of May 24 (p. 88):—

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
June 23	0 55 11	46 11·5 N.	0·2760	0·3129	0·042
25	0 57 1	46 40·5			
27	0 58 42	47 8·9	0·2887	0·3173	0·039
29	1 0 16	47 36·6			
July 1	1 1 42	48 3·7	0·3009	0·3212	0·036
3	1 3 0	48 30·2			
5	1 4 9	48 56·0	0·3127	0·3247	0·033
7	1 5 9	49 21·2			
9	1 6 1	49 45·7	0·3241	0·3278	0·031
11	1 6 44	50 9·6			
13	1 7 18	50 32·8 N.	0·3352	0·3306	0·029

The brightness at discovery is taken as unity.

THE Kazan Observatory has celebrated its "Jubilee" by publishing an interesting report about its activity since it was founded by Littrow fifty years ago. The mapping of the stars between 75° and 80°, which was begun by Prof. Kovalsky, was continued and extended by his successor, Prof. Dubyago.

THE Tashkend Observatory has just issued the second volume of its "Works."

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JUNE* 24–30.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on June 24

Sun rises, 3h. 46m.; souths, 12h. 2m. 13·7s.; sets, 20h. 19m.: right asc. on meridian, 6h. 14·5m.; decl. 23° 25′ N. Sidereal Time at Sunset, 14h. 33m.

Moon (Full, June 23, 21h.) rises, 19h. 57m.*; souths, 0h. 9m.; sets, 4h. 20m.: right asc. on meridian, 18h. 19·6m.; decl. 21° 5′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian h. m.	Declination on meridian ° ′
Mercury	5 33	13 25	21 17	7 37·2	19 52 N.
Venus	3 23	11 41	19 59	5 53·7	23 36 N.
Mars	13 28	18 53	0 18*	13 6·5	7 39 S.
Jupiter	17 6	21 29	1 52*	15 42·7	18 47 S.
Saturn	6 29	14 19	22 9	8 31·3	19 34 N.
Uranus	12 56	18 36	0 16*	12 49·3	4 35 S.
Neptune	1 59	9 45	17 31	3 56·9	18 47 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Comet Sawerthal.

June.	h.	Right Ascension. h. m.	Declination. ° ′
24	0	0 55·2	46 12 N.
28	0	0 58·7	47 9

Occultations of Stars by the Moon (visible at Greenwich).

June.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
24	50 Sagittarii	6	22 6	23 16	65 250
28	50 Aquarii	6	2 28	2 59	163 215

June.	h.	
25	9	Mercury stationary.
27	23	Mercury at greatest distance from the Sun.

Meteor-Showers.

	R.A.	Decl.		
Near 52 Herculis	253°	47° N.	June 25–30.	Swift.
,, δ Cygni	295	40 N.	Slow.	
,, ε Delphini	305	9 N.	June 28.	

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	June 25,	22 54	*m*
			,, 30,	22 33	*m*
R Geminorum	7 0·6	22 53 N.	,, 27,		*M*
δ Libræ	14 55·0	8 4 S.	,, 29,	2 2	*m*
U Ophiuchi	17 10·9	1 20 N.	,, 28,	2 52	*m*
			,, 28,	23 0	*m*
W Sagittarii	17 57·9	29 35 S.	,, 24,	2 0	*m*
T Herculis	18 4·9	31 0 N.	,, 27,		*M*
U Sagittarii	18 25·3	19 12 S.	,, 30,	2 0	*m*
β Lyræ	18 46·0	33 14 N.	,, 28,	22 0	*m*
S Vulpeculæ	19 43·6	27 1 N.	,, 26,		*M*
η Aquilæ	19 46·8	0 43 N.	,, 24,	21 0	*m*
R Sagittæ	20 9·0	16 23 N.	,, 27,		*m*
X Cygni	20 39·0	35 11 N.	,, 26,	22 0	*M*
δ Cephei	22 25·0	57 51 N.	,, 27,	21 0	*M*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES.

LIEUTENANTS KUND AND TAPPENBECK have been conducting an expedition into the Cameroons interior during the latter part of 1887 and the beginning of the present year. Starting from Batanga they succeeded in penetrating as far as 12° 30′ W. long., when, being attacked by Soudan Negro traders they were forced to retreat, both of them seriously wounded. They succeeded in tracing the course of the Beundo or Njong River far into the interior, and brought back much information concerning the people and the products of the country. With regard to general results, they found that the water-parting between the rivers that discharge in the Cameroons region and those that flow into the Congo Basin lies not near the coast as has hitherto been supposed, and therefore it is hoped that a navigable route may be discovered that will lead well into the interior. The water-parting between the left tributaries of the Binué and the rivers in the German Cameroons also lies far in the interior. The division between the Soudan Negroes and the Bantus is not to be looked for in the direction of Adamawa, but southwards is formed by the Zannaga River and eastwards lies at a distance of 150 miles from the coast. Lieutenants Kund and Tappenbeck assert that the area of Mohammedan influence extends much farther south than has hitherto been thought. No signs of volcanic action have been met with as far as the Zannaga River or in the mountains to the north. The profile which accompanies the report shows a coast plain about 70 feet high, succeeded by a sharp slope rising to a height of from 3000 to 4000 feet, beyond which the country slopes gradually to the inner African plateau, about 2500 feet above the sea.

THE June number of Petermann's *Mitteilungen* is mostly occupied with a memoir by Dr. Supan on "A Century of African Exploration," written in commemoration of the centenary of the British African Association, founded in June 1788. Dr. Supan traces the gradual opening up of the continent and its various regions, the text being illustrated by a series of most instructive maps. In indicating what yet remains to be done, Dr. Supan maintains that it is a mistake to assert that the days of pioneer exploration are over. He shows that while a few patches have been surveyed with some care, while of others we have a general knowledge, and while in other regions lines of travel have been run through, there are great regions that still remain absolutely blank. In the north, in the region of the Sahara, which has been so long known to Europe, the blanks are almost greater than elsewhere, leaving ample room for pioneer work, which may very well be carried on alongside of more minute exploration.

TECHNICAL INSTRUCTION.[1]

IN celebrating as we are now doing the fifty-first annual meeting of the Yorkshire Union of Institutes, one's thoughts naturally revert to the foundation of that Union and to the educational progress which our country has made since the earlier years of the century; and round these thoughts will gravitate recollections of the life and labours of your revered President,

[1] Address delivered by Sir Henry Roscoe, M.P., F.R.S., at Castleford, on Wednesday, June 20, on the occasion of the fifty-first annual meeting of the Yorkshire Union of Mechanics' Institutes.

Sir Edward Baines, for in him we have a living picture of the history of the educational progress of the century. Truly, he has been a witness, and an active witness, of English educational reform from his earliest years, nor have his efforts in the great cause from that time forward ever ceased. Was he not even as a boy in Leeds so long ago as 1809 an earnest listener to the expositions of one who may be justly regarded as the founder of our present system of national education, I mean Joseph Lancaster? The name of Baines, again, is intimately connected with those of Birkbeck and Brougham in the great work of founding mechanics' institutes.

The English character is ever prone to consecutive action, sudden revolutions are contrary to its spirit, and this characteristic is evidenced by the present phase of interest in so-called technical education, for this is doing nothing more than carrying out in accordance with the necessities of the hour the old principle enunciated by Birkbeck, Brougham, and Baines in 1825 in the founding of mechanics' institutions, which have for their object the teaching to our workmen the principles of art and science which underlie the trades they practise. This, too, is our definition of technical instruction. We do not attempt to teach trades, but the principles, artistic or scientific, upon which these trades depend. The school can teach how to make the best article, how to apply the principles which lie at the foundation of the manufacture. The workshop, on the other hand, teaches what the workshop alone can teach—how to produce the article most economically. This I take to be the essential distinction between school teaching and workshop practice. The boy at school learns how to do the work well, the man at the factory or shop must learn to do it not only well but most cheaply. If we keep these two parts of the question separate, give to the school what belongs to the school, and to the workshop what belongs to the workshop, we shall avoid all conflict between the so called theorist and the practical man, we shall preserve what is greatly to be prized, our English workshop experience, but add thereto a knowledge of principles which have hitherto been greatly wanting. Each does necessary work; what we desire and need to develop and to foster is the proper union of theory and practice, without which the supremacy in manufacturing industry, the chief glory and mainstay of our country, will be endangered in the industrial warfare in which all civilized nations are now engaged.

This, then, is the problem which Baines sought to solve, and which your Union and all ardent educationists of the present day are striving to accomplish. For this end we now seek Government aid, and are asking for national recognition of a national necessity. What else is the meaning of the Bills for the promotion of technical education now before Parliament? We ask simply for powers to develop and to strengthen the work which mechanics' institutes were founded to accomplish. We desire to carry on that work on sound lines; that, whilst asking for Imperial aid and for the imprimatur of a national system, we shall be left to decide for ourselves the exact mode of carrying out that system which each locality and each special industry knows is best adapted to satisfy its peculiar requirements. These should be the main objects of any Technical Bill. Are these objects properly put forward, and are these conditions properly safe-guarded in the Government Technical Bill now before Parliament? This is the pressing question of the hour. It is for you, and for similar associations throughout the length and breadth of the land, to say whether this is so or not, to satisfy yourselves on this point, and to urge your representative in Parliament—than whom none is more willing or more able to assist you—to see that your claims and opinions on this subject are made known to the Government which is responsible for bringing this great subject forward For, gentlemen, it is a great question, one which lies at the foundation of the future welfare—I had almost said the future existence—of the nation.

May I, then, venture to call your attention to one or two of the salient points in this Bill, and to point out to you what I consider some of its valuable provisions as well as some of its defects? In the first place, then, the chief and leading principle of the Bill is the recognition that the time has arrived for giving national aid, whether from local rates or from Imperial sources, for the promotion of technical instruction. The establishment of this principle is one, I venture to think, of the highest possible importance, which if once admitted may well cover a multitude of minor defects. Still, every benefit may be purchased too dear, and it is well to look at the conditions with which this concession to public opinion is coupled. Here I am speaking to educationists, but I am also speaking in Yorkshire and to Yorkshiremen, who have always upheld, and especially at the present moment do uphold, the standard of Liberal opinion in political as well as in educational matters, and I therefore feel that in expressing my opinion against certain conditions attached to the Bill—conditions which are diametrically opposed to the ideas and principles upon which the Liberal party has always acted—I say in expressing these objections I may claim your support as well as your attention.

Clause 2 of the Bill makes it compulsory on every School Board adopting its provisions as to technical instruction—that is, upon every School Board undertaking to rate its district to the limited penny in the pound—to aid the supply of technical instruction in any other public elementary school not under its management in like manner as it aids the supply of such instruction in its own schools. This clause, which as you all will see may be most sweeping in its effects, must be entirely rejected; indeed, it could not stand one hour's scrutiny in the House of Commons, for it offends against the cardinal principle that those who pay the rates should have a voice, either directly or indirectly, in the spending of them, and this is not provided for. But whilst strongly objecting to this compulsory clause—the only compulsory one in the Bill—I, for one, am willing to consider, and to deal fairly with, the just claims of the voluntary schools; for although I am a believer in the Liberal creed, I am before all things an educationist, and I cannot forget that if we are to have our children made more fit for succeeding in the modern battle of life, we must endeavour to bring to bear upon them all, without distinction of creed or of party, the lever which will raise them in the social scale and enable them to use their heads and their hands to their own benefit, and therefore to that of the nation of which they form the units.

Hence, remembering that more than one-half of our population are educated in voluntary schools, and that in many localities these schools are the only ones in existence, and moreover that they are doing excellent educational work, I, speaking for myself, whilst strongly opposed to any compulsory powers, do not feel the same difficulty in admitting the provisions of the first clause in the Bill by which "any School Board in England may from time to time supply, or aid the supply of, such manual or technical instruction or both, as may be required, for supplementing the instruction in any public elementary school in its district, whether under its own management or not." This clause, you will perceive, enables School Boards if they think fit to assist voluntary schools in their districts by aid from the rates for the special purposes of technical instruction, and through the School Board the ratepayers have a voice as to whether their rates shall or shall not be thus spent. But here comes in the limiting clause that not more than 1*d.* in the pound shall be spent. I object to this limit. It will obviously be very difficult for any School Board to ascertain how far the expenses of giving technical instruction can be accurately defined, and I should prefer to leave the amount spent on this object to the good sense and judgment of the locality as represented by the School Board. But how about districts which possess no School Board? Are they to be left out in the cold? No. Provision is made in a further clause by which any local authority having adopted the Free Libraries Acts may hand over to the voluntary schools in its district a sum not exceeding 1*d.* in the pound for the purpose of supplying technical education to be given in its district public elementary schools. Here again the clause is a permissive one only, and the local authority as representing the ratepayers is the judge of whether and how far such aid is to be given. I do not like the plan of mixing up the vexed question of free libraries with that of technical education, and should much prefer the names of the authorities to be simply scheduled, as I see grave objections to the necessary *plébiscite* in districts which have not already adopted the Acts. Still I do not know that on this account I should wish to see the Bill rejected.

Another grave defect in the Bill is a limit is placed on the teaching of technical subjects in Board schools at the seventh standard. This deals a fatal blow at the higher elementary schools. Thus in the Central School in Manchester at the present moment no fewer than 500 scholars who have passed Standard VII. are now learning the sciences—subjects included within the term technical instruction. These scholars cannot continue thus to be taught under the Bill. We must have a similar provision introduced to that in the Scotch Bill, by which the Boards are empowered to use the rates for the maintenance of higher-grade schools; and these matters must be attended to if we are to have a Technical Bill worthy of the name. The higher technical education, as that given in the Colleges, may be assisted by rates levied by local authorities or by Imperial grants, in addition to those made now by the Department. All acknowledge the importance of this higher training. If the head is not educated, the hands are apt to get into mischief. Hence, as these University Colleges can never be self-supporting, it is greatly to be hoped that they will receive that national aid which their importance to the State demands.

But we have a second Bill before the House of Commons—one introduced by myself on behalf of the National Association for the Promotion of Technical Education. I naturally prefer the provisions of my own Bill to those of the Government. They are much simpler, less clogged and hampered by conditions, and confer the same benefits as the Government Bill proposes to confer, with one exception only, viz. aid from the rates to voluntary schools, for to this many of my friends are strongly opposed; but, so far as I am myself concerned, I am free to admit that I should not object to see the difficulty settled by permissive powers being given to the School Boards to aid voluntary schools in their district, just as it is proposed that local authorities shall have power to do the same where no School Boards exist; for, as I have pointed out, the ratepayers have it in their power to refuse such payments by electing members who will oppose such an application of the rates.

Now, to turn to the more immediate question relating to your Union, you may, I think, be gratified with the results of your fifty-one years' work. You can look back upon half a century of admirable endeavour. You have now 260 institutions in union, containing upwards of 500,000 members and 14,000 technical students. You have spent half a million of money in buildings contributed by voluntary subscriptions, with the exception of 1 per cent. derived from S.K. grants for building. All the members of your committees are unpaid, and many of them have been at work for you all their lives. Your claims for national aid are therefore high, and such aid is much needed, for, though the progress you have made is great, you have not nearly accomplished all that has to be done. We want continuation evening schools established on a new and generous basis. We want a new and more elastic evening school code. We want to emancipate from the rigid lines and requirements of payment on individual results. We want an attendance and merit grant for evening continuation schools—say 12*s*. per head for attendance of sixty nights to insure good and continuous teaching. Above all, we wish that existing institutions should be rendered effective. The 260 institutes are in existence, but need help.

When we look abroad we see that both Governments and municipalities vie with each other in aiding technical schools. They are proud to do so, for they know their value. "Do you suppose," said an intelligent German to me, "that we, weighted as we are with heavy taxation for our military and civil services, would willingly further tax ourselves for the purposes of technical schools unless we were convinced that the outlay will repay us over and over again?" This is German opinion, and it is the opinion which we need to inculcate in the minds of our own people, for then we shall get what we want.

Nor need we be ashamed of the beginnings which we have already made; many of our existing institutions will bear favourable comparison with Continental models. Take Huddersfield for example; the school there exactly meets the requirements of the district, and it has already exerted a very marked and beneficial influence on the trades of the district, especially as concerns dyeing and design. This school cost £20,000, all raised by voluntary effort, but though doing excellent work it is heavily in debt, and its friends have difficulty in raising funds to keep it going—not for lack of pupils, for the school is largely attended, but for the reason that such higher schools cannot be self-supporting, and the greater the number of pupils the greater the cost. Surely, if our people understood their true interests as well as our neighbours and competitors do, they would not rest until such an institution is placed in a position to do all it can to raise the condition of their industries by supplanting the too common and worn-out rule of thumb by scientific knowledge always new and always productive. Then again at Yeadon, a small place, you have a school which cost £7000 to build, and in which 350 students are being instructed. But here, too, funds are urgently needed to carry on the work. Surely there ought not to be many who grudge spending a penny in the pound on such objects. In Castleford itself, your Mechanics' Institute has done during its forty years of life, and is now doing, good work. The building is, however, too small for the requirements of the day; your numbers have increased from 80 to 210, and the necessary appliances for teaching science and technology are deficient. Let us hope that when the Technical Bill becomes an Act, Castleford will be one of the first to take advantage of its provisions.

But you may ask, What good will come to our leading industries here—coal and glass—by your technical education? How shall the employers and employed benefit therefrom? In the first place, then, there is no industry in which the value of even a little scientific training is so important for both masters and men as in that of coal-getting. Such a training may, for instance, be, and indeed has often been, the means of saving hundreds of valuable lives. One ignorant man may place in jeopardy or even sacrifice by a single careless act the lives of his comrades, an act which no one acquainted with the properties of explosive gases would dare to commit. In a thousand other ways scientific knowledge—which after all is only organized common-sense—will help all concerned in this great industry. So again in glass-making, how great is the aid given by scientific and artistic knowledge. What a step was the introduction of the Siemens regenerative tank furnace, and how much more remains to be achieved. Then your bottle trade might, by the application of artistic knowledge, be made the foundation of a higher and more tasteful industry which might successfully compete with the wares of Bohemia and Venice. Why not? Are not our workmen both mentally and physically superior to the foreigner? I believe them to be so. They only need teaching, and that we have hitherto withheld from them.

It has been well said that whilst we have confined our attention to improving our machines, the Germans have devoted themselves to educating their men. Let us lose no time in following their lead. "What we fear," said one of the masters to me, "is not either free trade or protection. What we fear is that some day you English will wake up to the necessity of educating your manufacturing population as we do, and then with your racial and physical advantages it will become difficult, if not impossible, for us to compete with you." Let us, then, take to heart the old adage that victory comes to the strong, but remember that it is not to the bodily strong, but only to the strong mentally and morally that the victory comes. Let us see that in this struggle for existence our people are healthy and vigorous in all these three essentials, and act upon the true and eloquent words of Huxley, "You may develop the intellectual side of a people as far as you like, and you may confer upon them all the skill that training and instruction can give, but if there is not underneath all that outside form and superficial polish the firm fibre of healthy manhood and earnest desire to do well, your labour is absolutely in vain."

THE INTERNATIONAL GEOLOGICAL CONGRESS.

ADMIRABLE arrangements have been made for the London meeting of the International Geological Congress, from September 17 to 22 next. The following details are taken from a printed letter signed by the General Secretaries, Mr. J. W. Hulke and Mr. W. Topley. The meetings will be held in the rooms of the University of London, Burlington Gardens, where accommodation for the Council, Committees, Exhibition, &c., has been granted by the Senate of the University. There is a refreshment-room in the building, and there are several restaurants and hotels in the immediate neighbourhood. Arrangements will be made at one of these restaurants for a room to be set apart for the social meetings of members of the Congress. The opening meeting of the Congress will take place on Monday evening, September 17, at 8 p.m., when the Council will be appointed, and the general order of business for the session will be determined. The ordinary meetings of the Congress will be held on the mornings of Tuesday, the 18th, and succeeding days, beginning at 10 a.m. In the afternoons there will be visits to Museums, or to places of interest in the neighbourhood of London. Arrangements for the evenings will be made at a later date. The ordinary business of the Congress will include the discussion of questions not considered at Berlin, or adjourned thence for fuller discussion at the London meeting. Amongst these are: the geological map of Europe; the classification of the Cambrian and Silurian rocks, and of the Tertiary strata; and some points of nomenclature, &c., referred to the Congress by the International Commission. Miscellaneous business will also be considered. In addition to these questions, the Organizing Committee proposes to devote a special sitting to a discussion on the Crystalline Schists. An Exhibition will be held during the week of the Congress, to which geologists are invited to send maps, recent memoirs, rocks, fossils, &c. Foreign members of the Congress are invited by the Council of the British Association to attend the meeting of that Association at Bath. During the week when the Association meets, there will be short excursions in the neighbourhood of Bath, and longer excursions will be made after the meeting. At these excursions excellent sections of the Lower Secondary and Upper Palæozoic rocks will be visited. Excursions will take place in the week after the meeting of the Congress (September 24 to 30). The number of these will depend upon the number of members desirous of attending, and upon the districts which they most wish to visit. The excursions at present suggested are :—(1) The Isle of Wight (visiting the Ordnance Survey Office at Southampton on the way)—Cretaceous, Eocene, Oligocene. (2) North Wales—Pre-Cambrian and the older Palæozoic rocks ; West Yorkshire (Ingleborough, &c.)—Silurian and Carboniferous Limestone. (3) East Yorkshire (Scarborough, Whitby, &c.)—Jurassic and Cretaceous. Should the number of members be so large as to make additional excursions necessary, they will probably be :—(4) Norfolk and Suffolk—Pliocene (Crag) and Glacial beds. (5) To the Jurassic rocks of Central England. The short excursions during the week of the Congress will probably be to Windsor and Eton, to St. Albans, to Watford, to Brighton, to the Royal Gardens at Kew, and to other places of interest. Brief descriptions of the districts to be visited in these excursions will be prepared (with illustrative sections, &c.), and will, if possible, be sent to members before the meeting. The full Report of the London meeting will be issued soon after the close of the session. It will contain, in addition to reports of the ordinary business of the Congress, the Report of the American Committee on Nomenclature (about 230 pp.) ; the Memoirs on the Crystalline Schists (about 150 pp.), and reports of discussion on the same ; and probably a reprint, with additions, of the Report of the English Committee on Nomenclature (about 150 pp.).

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The Burdett-Coutts Scholarship in Geology has been awarded to Mr. M. Hunter, B.A., Queen's College.

The degree of M.A. *honoris causâ* has been conferred on Dr. S. J. Hickson, the Deputy Linacre Professor, and on Mr. Wyndham R. Dunstan.

Scholarships in Natural Science are announced for competition, at Merton and Corpus jointly on June 26, at Magdalen on October 9, and at Balliol, Christ Church, and Trinity jointly on November 20. Information may be had from the science tutors of the various Colleges.

A statute is being discussed by Congregation, which will place the biological sciences on the same footing as the physical sciences so far as the examinations for pass degrees are concerned, and it is hoped that the changes to be introduced will increase the numbers of the biological and medical schools.

Mr. F. J. Smith, of the Millard Laboratory at Trinity, has been appointed University Lecturer in Mechanics and Experimental Physics.

CAMBRIDGE.—An amended report on the Natural Science Examinerships has just appeared, but the scheme proposed is very complex. It having been found difficult to get examiners to undertake the honours, and ordinary degree, and M.B. examinations combined, it is proposed to separate the elementary examination work, and appoint two examiners each in elementary chemistry, in elementary physics, and in elementary biology, while two examiners in each subject of the Natural Sciences Tripos are to be appointed as before, and two in pharmaceutical chemistry, for the second M.B. Thus there will be twenty-four examiners in all. The examiners are to be paid a minimum of fifteen, twenty, or thirty pounds each, with a payment of five shillings for each Tripos candidate in their subject, or one, two, and four shillings per candidate in other examinations. Moreover, it is required that all papers and all practical work in honours shall be examined by both examiners in a subject. Both examiners are to be present at all oral work in their subject; and all examiners must be present at the meeting for arranging the class-list for any examination. We prognosticate that the list of examiners, if at all worthy of the University, will not largely consist of non-residents, under the new scheme. The worst mistake perhaps that the University makes is in continuing the one-sided ordinary degree examinations in single subjects, such as geology, botany, and zoology ; for all combined there were only four candidates in the last academical year ; and for these there were six separate examinations provided, though two were not held. The chemistry "special" attracts a number of candidates, who might be much better employed in preparing for the First Part of the Natural Sciences Tripos. It would be far easier to work the Natural Science Examinations if these were abolished. It is absurd to keep up a machinery of examination which is tabooed even by candidates. The Tripos is a success, which the specials are not, and still more liberal payments and regulations ought to be made. It ought to be remembered that the graduates pay heavy degree fees in addition to examination fees.

The examiners for 1888 in the Second Part of the Mathematical Tripos were Edward John Routh, Sc.D., Peterhouse ; James Whitbread Lee Glaisher, Sc.D., Trinity College ; Joseph John Thomson, M.A., Trinity College ; Andrew Russell Forsyth, M.A., Trinity College. The names, in each class and in each division, are arranged in alphabetical order, and not in order of merit. All the candidates passed the Mathematical Tripos, Part I., in June 1887.

Class I.—Division 1.—Baker, B.A., Joh. ; Berry, B.A., Trin. ; Flux, B.A., Joh. ; Mitchell, B.A., Trin. Division 2.—Brown, B.A., Christ's ; Clay, B.A., Trin. ; Iles, B.A., Trin.

Class II.—Little, B.A., Trin. ; Norris, B.A., Joh. ; Peace, B.A., Emman. ; Soper, B.A., Trin.

Class III.—None.

The faint hope that there was till lately that a Geological Museum might soon be begun has been dissipated by the Financial Board having reported that the University has no funds available at present, although the Sedgwick Fund has £19,000 in hand to supplement the University contribution.

The late Sir Charles Bunbury's valuable herbaria have been presented to the University by Lady Bunbury.

At the Annual Scholarship Election at St. John's College, on June 18, the following awards in Natural Science were made :—Foundation Scholarships continued or augmented—Seward, Rolleston, Rendle. Turpin, Groom, d'Albuquerque ; Foundation Scholarships awarded—Hankin, Horton-Smith, Locke, Baily, Simpson ; Exhibitions awarded—d'Albuquerque, Hankin, Horton-Smith, Blackman, Schmitz. In Mathematics, the following awards were made :—Foundation Scholarships continued or augmented—Baker, Flux, Norris, Orr, Sampson, Harris, Rudd, Bennett ; Foundation Scholarships awarded—Palmer, Carlisle, Burstall, Monro, Cooke, Lawrenson ; Exhibitions awarded—Sampson, Harris, Monro, Dobbs, Reeves, Bennett, Burstall, Cooke, Lawrenson, Brown, Finn, Kahn, Salisbury, Schmitz, Shawcross ; Proper Sizarship awarded—Finn. Wright's Prizes to Simpson, Hankin, Blackman, for Science ; and Orr, Burstall, Reeves, for Mathematics. The Herschel Prize to Salisbury, for Astronomy ; the Hockin Prize for Electricity not awarded. The Hutchinson Studentship of £60 a year for two years is awarded to Mr. G. S. Turpin for research in Organic Chemistry ; and the Hughes Prize to Orr (Senior Wrangler) and Brooks (Senior Classic).

SCIENTIFIC SERIALS.

American Journal of Science, June.—Note on earthquake-intensity in San Francisco, by Edward S. Holden. The object of this paper is to obtain an estimate of the absolute value of the earthquake-intensity developed at San Francisco during the American historic period, based on the very complete records collected by Thomas Tennant. The intensity of each separate shock (417 altogether) is assigned on the arbitrary scale of Rossi and Forel. The total average intensity during the 80 years from 1808 to 1888 is found to be nearly equal to the intensity of 28 separate shocks as severe as that of 1868, and the 417 shocks of known intensities correspond to 33,360 units of acceleration.—On the relations of the Laramie Group to earlier and later formations, by Charles A. White. The author's further studies of this group, by some geologists referred to the Tertiary, by others to the Cretaceous ages, lead to the conclusion that the upper strata form a gradual transition from the latter to the former, while there is strong presumptive evidence of the Cretaceous age of the greater part of it.—The gabbros and diorites of the "Cortlandt Series" on the Hudson River near Peekskill

New York, by George H. Williams. With this paper the author concludes for the present his elaborate petrographic studies of the extremely varied massive rocks of the "Cortlandt Series," as it has been designated by Prof. J. D. Dana. He treats in detail the gabbro, diorite, and mica-diorite varieties of norite occurring chiefly in the south-western portion of the area.—Three formations of the Middle Atlantic slope (continued), by W. J. McGee. In this concluding paper the whole subject of the Columbia formation is recapitulated, the general conclusion being that it is much older than the moraine-fringed drift-sheet of the North-Eastern States, and that while th evertebrates of its correlatives suggest a Pliocene origin, both stratigraphy and the invertebrate fossils prove that it is Quaternary. Thus the Columbia formation not only enlarges current conceptions of Quaternary time, and opens a hitherto sealed chapter in geology, but at the same time bridges over an important break in geological history, between the Tertiary and Quaternary epochs.—A comparison of the elastic and the electrical theories of light with respect to the law of double refraction and the dispersion of colours, by J. Willard Gibbs. The main object of this paper is to show the great superiority of the electric over the elastic theories of light as applied to the case of plane waves propagated in transparent and sensibly homogeneous media. The phenomena of dispersion here studied corroborate the conclusion which seemed to follow inevitably from the law of double refraction alone.—Mr. Henry J. Biddle contributes some valuable notes on the surface geology of Southern Oregon, visited by him during the summer of 1887.

SOCIETIES AND ACADEMIES.

London.

Royal Society, June 7.—"An Additional Contribution to the Placentation of the Lemurs." By Prof. Sir Wm. Turner, Knt., M.B., LL.D., F.R.S.

In 1876 the author contributed to the Royal Society a memoir "On the Placentation of the Lemurs," which was published in the Philosophical Transactions of that year (vol. clxvi. Part 2). The gravid uteri which he examined and described were from specimens of *Propithecus diadema*, *Lemur rufipes*, and *Indris brevicaudatus.*

In April of the present year he received from Mr. F. E. Beddard, Prosector to the Zoological Society of London, the gravid uterus of a Lemur, which was *Lemur xanthomystax.*

The examination of this gravid uterus confirmed the conclusions to which both Alphonse Milne Edwards[1] and the author had arrived independently from previous investigations, that the placenta in this important group of animals is diffused and non-deciduate, and that the sac of the allantois is large and persistent up to the time of parturition. In these important respects, therefore, the Lemurs, are, in their placental characters, as far removed from man and apes as it is possible for them to be.

Although the author is not disposed to attach too much weight to the placenta as furnishing a dominant character for purposes of classification, yet he cannot but think that animals which are megallantoid, non-deciduate, and with the villi diffused generally over the surface of the chorion, ought no longer to be associated in the same order with animals in which, as in the apes, the sac of the allantois early disappears, and the villi are concentrated into a special placental area, in which the fœtal and maternal structures are so intermingled that the placenta is highly deciduate. Hence he is of opinion that the Lemurs ought to be grouped apart from the Apes in a special order, which may be named either with Alphonse Milne Edwards *Lemuria*, or with Victor Carus and others *Prosimii.*

The fœtus possessed an imperfect covering, external to the hairy coat, and quite independent of the amnion, composed of a cuticular membrane. It corresponded with the envelope named by Welcker *epitrichium*, and described both by him and by the author as present in *Bradypus* and *Cholopus*. But it occurred in the fœtus both of *Lemur xanthomystax* and *Propithecus diadema* in flakes and patches, and not as a continuous envelope as in the Sloths.

Physical Society, May 26.—Mr. Shelford Bidwell, F.R.S., Vice-President, in the chair.—The following communications were read:—Note on the governing of electromotors, by Profs. W. E. Ayrton and J. Perry. In a paper read before the Society of Telegraph-Engineers in 1882 the authors deduced the conditions of self-regulation of electromotors for varying load when supplied either at constant potential or with constant current. The conditions involved "differential winding," *i.e.* the use of a shunt motor with series demagnetizing coils. With this arrangement fairly good regulation has been obtained, but owing to want of economy the methods have not been developed further. Since then another arrangement, in which a simple shunt motor is used, and a few accumulators placed in series with the armature, has been devised for working in a constant current system. By means of a suitable switch, the accumulators can be charged when the motor is at rest. On the assumption that the E.M.F. of motors is given by $E = n(p + tZ)$, where n = speed, Z = number of turns on magnets, and p and t are constants, it is shown that the speed at which a motor will govern is given by

$$n = \frac{z + a + a'}{t},$$

and the constant current

$$C = \frac{e - np}{a + a'},$$

where z and a are the resistances of the shunt and armature respectively, and e and a' the E.M.F. and resistance of the accumulators. Since a and a' may be small and np not large, the value of e need not be great to give a considerable value for C, and thus only a small number of accumulators will be required.—On the formulæ of Bernoulli and Haecker for the lifting-power of magnets, by Prof. S. P. Thompson, read by Prof. Perry. The formulæ referred to are $P \propto \sqrt[3]{W^2}$ and $P = a\sqrt[3]{W^2}$ respectively, where P = lifting-power, W = mass of magnet, and a a constant depending on the material and shape of the magnet. These formulæ, the author shows, are equivalent to saying that the lifting-power of magnets in which the magnetic induction, B, has been carried to an equal degree, is proportional to the polar surface, and that Haecker's coefficient a is proportional to B^2 through the surface. Assuming the induction uniform over the surface, it is shown that

$$P = \frac{1}{8\pi} B^2 A,$$

where A = area of surface, and this gives a very convenient method of determining B from measurements made upon the pull exerted at a given polar surface. If P be measured in kilogrammes and A in square centimetres, the formula for B becomes

$$B = 5000 \sqrt{\frac{P}{A}};$$

and if the measurements be made in pounds and inches, the constant becomes 1317. It will be readily seen that the greater power of small magnets in proportion to weight does not require for its explanation the sometimes alleged fact that small pieces of steel can be more highly magnetized than large ones, for if B be the same, the lifting-power will be proportional to the polar surface, and not to weight, and hence must necessarily be greater relatively to weight in small magnets. In the case of electro-magnets for inductions between 6000 and 16,000, between which the permeability, μ, is approximately given by

$$\mu = \frac{16{,}000 - B}{3 \cdot 2},$$

the lifting-power is shown to be

$$P = A\left(\frac{3 \cdot 2\, Si}{Si + 2 \cdot 56 l}\right)^2,$$

where P is in kilogrammes, A in square centimetres, Si = ampere turns, and l = mean length of the magnetic circuit.—Experiments on Electrolysis; Part ii., Irreciprocal Conduction,[1] by Mr. W. W. Haldane Gee and Mr. H. Holden. An abstract was read by the Secretary. The authors have observed, when strong sulphuric acid is used as an electrolyte, the electrodes being of platinum, that the decomposition nearly ceases, if, by decreasing the resistance in circuit, it is attempted to increase the current beyond a certain maximum. When this condition (called the insulating condition) is arrived at, reversing the current immediately restores the conductivity. Experiment shows that the current density is an important factor, and that the composition,

[1] "Histoire Naturelle des Mammifères de Madagascar," forming vol. vi. chap. ix. of Grandidier's "Histoire de Madagascar."

[1] Irreciprocal conduction is said to occur if a reversal of the direction of a current causes any change in its magnitude.

viscosity, and temperature of the electrolyte, as well as the previous history of the electrode, have considerable influence on the current density at which the insulating condition occurs. The seat of the insulating layer is found to be at the anode; and the authors believe it due to very concentrated acid formed around the electrode, whose specific resistance is very high. Experiments were also made with carbon and gold electrodes, and phosphoric acid, caustic potash, soap, and sodium benzoate were used as electrolytes, the results of which seem compatible with the concentration hypothesis above stated. The paper contains an historical and critical account of allied phenomena, and tables expressing the numerical results obtained by the authors are given.

Linnean Society, June 7.—Mr. Carruthers, President, in the chair.—The following were nominated Vice-Presidents: Mr. F. Crisp, Dr. Maxwell Masters, Dr. John Anderson, Mr. C. B. Clarke.—An exhibition under the microscope of decalcified and stained portions of the test of *Laganum depressum* was then given by Prof. Martin Duncan, who made some very instructive remarks on the structural characters to be relied on for discriminating the species.—Mr. D. Morris, of Kew, exhibited some drawings of a Fungus (*Exobasidium*) causing a singular distortion of the leaves of *Lyonia*, from Jamaica.—A paper was then read, by Mr. H. N. Ridley, on the natural history of Fernando Noronha, in which he gave the general results of his investigations into the geology, botany, and zoology of this hitherto little explored island.

Royal Meteorological Society, May 16.—Dr. W. Marcet, F.R.S., President, in the chair.—The following communications were read:—Report of the Wind Force Committee on experiments with anemometers conducted at Hersham, by Mr. G. M. Whipple and Mr. W. H. Dines. A whirling apparatus, with arms 29 feet radius, was rotated by means of a small steam-engine. On the arms of the whirler four different anemometers were placed. Each experiment lasted fifteen minutes, the steam-pressure remaining constant during the run. For the Kew standard anemometer, with arms 2 feet long, the experiments give a mean value for Robinson's factor of 2·15; and for two smaller instruments the factor is 2·51 and 2·96. Mr. Dine's helicoid anemometer gave very satisfactory results, the mean factor being 0·996.—On the measurement of the increase of humidity in rooms by the emission of steam from the so called bronchitis kettle, by Dr. W. Marcet, F.R.S. The author described a number of experiments which he had made by steaming a room with a bronchitis kettle, and ascertaining the rise and fall of the relative humidity from readings of the dry- and wet-bulb thermometers. He found that the air in the room could not be saturated, the relative humidity not exceeding 85 per cent.

Entomological Society, June 6.—Dr. D. Sharp, President, in the chair.—Mr. Pascoe brought for exhibition a book of fine plates of *Mantidæ*, drawn by Prof. Westwood, which it had been hoped would have been published by the Ray Society.—Mr. E. Saunders exhibited a species of Hemiptera, *Monanthia angustata*, H-S., new to Britain, which he had captured by sweeping, near Cisbury, Worthing. The insect is rather closely allied to the common *Monanthia cardui*, L.—Mr. McLachlan exhibited a species of *Halticidæ*, which had been sent him by Mr. D. Morris, Assistant Director of the Royal Gardens, Kew, who had received them from Mr. J. H. Hart, of the Botanic Gardens, Trinidad, with a note to the effect that they had attacked young tobacco and egg-plants badly in that island. Mr. Jacoby had, with some reserve, given as his opinion that it might possibly turn out to be *Epitrix fuscata*, Duv., a species which had been described from Cuba.—The Rev. H. S. Gorham exhibited a collection of beetles lately captured in Brittany including *Diachromus germanus*, L., *Onthophagus taurus*, L., *Hister sinuatus*, Ill., and other species which are exceedingly rare, or altogether wanting in Britain, and yet occur very commonly in the north of France.—Mr. White exhibited living larvæ of *Endromis versicolora*, from near Bristol, and remarked that when quite young they are nearly black, owing to being very thickly spotted with that colour; the body-colour is green, and after two or three changes of skin the spots disappear. Mr. White also exhibited two preserved larvæ of *Phorodesma smaragdaria*, which he had recently taken, and made some remarks concerning the so-called "case," which this insect is said to construct from the leaves of its food-plant, *Artemisia maritima*. This he did not consider to be really a case, but he had discovered that the larva possessed on its segments certain secretory glands, at the apex of each of which there is a bristly hair; this appears to retain pieces of the plant, which are probably fixed firmly afterwards by means of the secreted fluid. These pieces are very irregularly distributed, and their purpose does not seem quite evident.—Mr. Lewis exhibited about three hundred specimens of the genera *Hetærius*, Er., and *Eretmotus*, Mars. The most remarkable of these was *Hetærius acutangulus*, Lewis, discovered last year by Mr. J. J. Walker near Tangier, and recently taken by him at San Roche, in Spain.

PARIS.

Academy of Sciences, June 11.—M. Janssen, President, in the chair.—A study of the refrigerant mixtures obtained with solid carbonic acid, by MM. Cailletet and E. Colardeau. These researches seem to show that the ether generally used in combination with snow and carbonic acid for the purpose of obtaining intense cold, plays a much greater part than has been supposed in lowering the temperature of the mixture.—Representation of the attitudes of human locomotion by means of figures in relief, by M. Marey. The figure of a runner at a given moment is here reproduced from a relief obtained by M. Engrand by means of the photochronograph. It is pointed out that a continuous series of such figures, obtained by this process, would be of great service in determining for artists and physiologists the successive changes of attitude in running and walking.—Determination of the mean level of the sea, by means of a new instrument, by M. Ch. Lallemand. In a previous note (*Comptes rendus*, May 28, 1888) the principle was described of this apparatus, which is here figured and named the *medimaremeter*. It gives the mean sea-level without any mechanical adjustments, and almost without the need of calculations—On the artificial reproduction of hydrocerusite, on the chemical composition of this mineral species, and on the constitution of white lead, by M. L. Bourgeois. These synthetic researches throw much light on the hitherto problematical nature of hydrocerusite, as well as on the constitution of white lead (ceruse), in which the author distinguishes only two definite substances, both existing in nature—hydrocerusite and cerusite. Analysis shows that the formula of the artificially prepared hydrocerusite is $3PbO,2CO_2,HO$, or $2(PbO,CO_2) + PbO,HO$, which is no doubt that of the natural substance also.—On the variations of the personal equation in the measurement of double stars, by M. G. Bigourdan. Thiele supposes that the personal equation of each observer remains somewhat constant during a "season of observations," and then takes a different value for another period, the duration of the "seasons" varying from a few days to several months. But according to Struve these variations are rapid, occurring in a few hours, and lasting only a single night. The observations of the author tend to show that these apparently contradictory views are capable of being reconciled, both being to a certain extent true.—On the determination of some new rings of Saturn lying beyond those already known, by Dom Lamey. These were first vaguely perceived by the author in 1868, and have been repeatedly observed since February 12, 1884, with the 16 cm. refractor in the clearer atmosphere of the Grignon Observatory. They are four in number, and are visible as well-defined elliptical rings in the regions intermediate between Mimas and Titan, first and sixth satellites of Saturn. The semi-diameter of the planet being taken as 1, the semi-diameter of the rings, measured from the middle of the most intense region, would be $2·45 \pm 0·05$; $3·36 \pm 0·02$; $4·90 \pm 0·50$; $8·17 \pm 0·23$. They were also independently observed by two of the author's fellow-workers, and cannot therefore be explained away as optical illusions due to the terrestrial atmosphere or any other sources of error.—On a point in the history of the pendulum, by M. Defforges, with remarks by M. C. Wolf. In connection with Kater's memoir of 1818, presented to the Royal Society, on the "convertible" pendulum, and his repudiation of de Prony's claim to priority of invention, M. Defforges announces the discovery of some documents in the Ecole des Ponts et Chaussées fully confirming de Prony's claim. M. Wolf, however, points out that these documents (undated, but no doubt written in 1800) were never published, and certainly unknown both to Bohnenberger when he announced the project of a pendulum with reciprocal axes (1811), and to Kater when he rejected de Prony's claim to priority of invention (1818). Hence, although de Prony now appears to have been the precursor, the rights of Bohnenberger and Kater remain intact as discoverers of the principles to which is due the revolution effected in the observations of the pendulum during the present century.—On a correction to be made in Regnault's determinations of the weight of a litre of the elementary gases, by M. J. M. Crafts. The error already pointed out by Lord Rayleigh is here corrected for air, N, H, O, and CO_2.—Experiments with a non-oscillating pendulum, by M. A. Boillot. It is shown that the oscillating pendulum, which in Foucault's experiment demonstrates the movement of the globe, may be used for the same demonstration by suppressing the oscillatory action and operating in a room.—Measurement of the velocity of etherification by means of electric conductors, by M. Negreano. A process is explained for measuring the rapidity of the chemical reactions which take place between certain resisting bodies at the moment their electric resistances become varied. These resistances have been measured according to the method indicated by Lippmann.—On a diamantiferous meteorite, which fell on September 10/22, 1886, at Novo-Urei, in the Government of Penza, Russia, by MM. Ierofeieff and Latchinoff. Analysis of this specimen, weighing 1762 gr., shows that it contains 1 per cent. of very fine carbonado, or diamond dust, besides 1·26 of amorphous carbon. The other chief substances were—peridot, 67·48; pyroxene, 23·82; and nickled iron, 5·45.

BERLIN.

Physical Society, June 1.—Prof. von Helmholtz, President, in the chair.—Dr. Lummer gave an account of experiments which he had made on the determination of the focal length of lenses by the method of Abbe in Jena. The method is based upon the equation $f = \frac{a}{\beta_1 - \beta_2}$; where f is the focal length, a the distance of two objects from the lens, and β_1 β_2 the respective magnifications of their images. The speaker discussed first the way by which Abbe had arrived at the above equation, and then went thoroughly into an explanation of the methods for measuring the amount of magnification of the images. It must suffice here to say briefly that the magnification was measured by a microscope directed along the principal axis of the lens, and at right angles to its surface, the microscope then being moved backwards and forwards, until the upper and lower ends of the image were visible. Prof. von Helmholtz explained that during his physiological-optical researches he had already determined the focal lengths of lenses by the measurements of the magnification, in accordance with the formula given above, admitting at the same time that his methods were perhaps less exact.—Dr. Lummer then gave an abstract of a paper on the movement of air in the atmosphere, which he had recently read before the Academy of Sciences. In solving the problem, he had made use of the principle of mechanical similarities. When the hydrodynamic equation for a given motion is known, it is only necessary to multiply all the factors by n in order to represent the motion in much larger dimensions. Accordingly if the conditions of the occurrence of air currents, such as take place in the atmosphere, have been experimentally determined in the laboratory for 1 cubic metre of air, and if the atmosphere is assumed to be 8000 metres high, then the space, time, and moment must be multiplied by 8000, while on the other hand the internal friction must be taken as being only 1/8000 of that which has been determined by experiment. It follows from this that the internal friction is of very small account; but as against this, the friction of the earth's surface has a considerable influence and cannot be neglected. Supposing a mass of air moving horizontally is considered, then a series of particles of air, which were at the outset vertically each above the other, will finally place themselves along a curve of sines as the result of friction at the earth's surface. Calculation shows that it would require a period of 42,000 years before the motion was reduced to one-half as the result of internal friction. The speaker then considered the atmosphere as made up of rings of air which surround the earth in coincidence with the parallels of latitude: each of these rings of air has its own moment of rotation, which depends on its radius, and is therefore greatest at the equator and least at the poles. If the air which is streaming upwards at the equator were to stream down again to the earth in higher latitudes, it would be moving with a velocity far exceeding that of any known storm, even at the latitude of 30°. Since the internal friction of the air is so small that it may be neglected, the speaker proceeded to point out the other factors which have an influence in slowing down the air as it falls. He regards them as being the vortex motions which take place in the atmosphere at the iscontinuous surfaces of two masses of air moving with different velocities. These vortex motions cause the adjoining layers of the two masses of air to mix, and thus diminish their velocity. This is the explanation of the calms, trade-winds, sub-tropical rains, and other phenomena which occur in the atmosphere. It would occupy too much space to give even a brief statement of how these conclusions are arrived at.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Course of Practical Instruction in Botany, Part 1, 2nd edition: Prof. F. O. Bower (Macmillan).—Lessons in Elementary Mechanics, Stage 2: W. H. Grieve (Longmans).—Observations on the Embryology of Insects and Arachnids: A. T. Bruce (Baltimore).—Smithsonian Report, 1885, Part 2 (Washington).—Birdsnesting and Bird-skinning, 2nd edition: M. Christy (Unwin).—An Elementary Treatise on Mensuration: E. J. Henchie (School Books Publishing Co.)—First Elements of Experimental Geometry: P. Bert; translated (Cassell).—Introductory Inorganic Analysis: E. H. Cook (Churchill).—Origin and Growth of Religion as illustrated by Celtic Heathendom: Prof. J. Rhys (Williams and Norgate).—Sierra Leone; or the White Man's Grave: G. A. Lethbridge Banbury (Sonnenschein).—Explorations and Adventures in New Guinea: Capt. J. Strachan (Low).—Longmans' School Geography for Australasia: G. G. Chisholm (Longmans).—On the Dicotylinæ of the John Day Miocene of North America: E. D. Cope.—On the Mechanical Origin of the Dentition of the Amblypoda: E. D. Cope.—The Theory of the Tides: J. Nolan (Dulau).—The Perissodactyla: E. D. Cope (Philadelphia).—The Mechanical Origin of the Sectorial Teeth of the Carnivora: E. D. Cope (Salem).—Recent Advances in our Knowledge of the Law of Storms: F. Chambers (Bombay).—Causation of Pneumonia: H. B. Baker (Lansing).—Quarterly Journal of the Royal Meteorological Society, April (Stanford).—Quarterly Weather Report, Part 3 (Eyre and Spottiswoode).—Hourly Readings, 1885 (Eyre and Spottiswoode).—Travaux de la Société des Naturalistes de St. Pétersbourg, vol. xix. 1888, Section de Géologie et de Minéralogie (St. Pétersbourg).—Notes from the Leyden Museum, vol. x. Nos. 1 and 2 (Brill, Leyden).—Madras Journal of Literature and Science, Session 1887–88 (Madras).—Proceedings of the Academy of Natural Sciences of Philadelphia, Part 1, 1888 (Philadelphia).—Internationales Archiv für Ethnographie, Band i. Heft 3 (Trübner).

CONTENTS.

THURSDAY, JUNE 28, 1888.

THE EARLY CORRESPONDENCE OF CHRISTIAN HUYGENS.

Œuvres Complètes de Christian Huygens publiées par la Société Hollandaise des Sciences. Tome Premier : Correspondance 1638–1656. (La Haye : Martinus Nijhoff, 1888.)

NEVER before, we venture to assert, even in this age of "complete editions," has so colossal a literary monument been raised to the memory of a great man as the edition of the works of Christian Huygens, of which the first instalment now lies before us. In a huge and splendid volume of 621 quarto pages, is contained the correspondence, from his ninth to his twenty-eighth year, of the "young Archimedes," as his friends delighted to call him. Yet out of 2600 documents in the hands of the Commission charged by the Amsterdam Academy of Sciences with the superintendence of the publication, no more than 365 have as yet been printed. Seven additional tomes, at least as massive as that just now issued from the press at the Hague, will be needed to bring to completion the initial section of the comprehensive record. The works of Huygens, edited and inedited, will follow, with an elaborate biography, so that we may safely assume that the present century will not see the end of an enterprise the pecuniary responsibility of which has been generously undertaken by the Scientific Society of Holland.

We have nothing but praise to accord to the manner in which it has so far been conducted. All selective difficulties were indeed spared to the Commission ; for the collection at Leyden was of such exceptional value that their resolution to print everything it contained admitted of no cavil, and was arrived at without hesitation. Room was, however, left for discretion as to the manner of presenting to the public the materials at their disposal ; and it has been wisely exercised. The notes are elucidatory without being obtrusive ; the prefatory remarks are few and to the point ; the indexes (of which there are no less than five) afford a satisfactory clue to a labyrinth of close upon four hundred letters in Latin, French, and Dutch, miscellaneous in their contents, and necessarily chronological in their arrangement. They are of great and varied interest. Scientific history, the dispositions and modes of thought of "men of light and leading" in the seventeenth century, the manners and customs of the time, are all in turn illustrated by them ; above all, their perusal offers singular advantages for studying the development of the powerful and active mind of the protagonist in the life-drama they partially unfold.

Christian Huygens was born at the Hague, April 14, 1629. Every educational advantage which the age could afford was showered upon him. His father, Constantine Huygens, was distinguished as a statesman, a poet, a man of letters, and a musician. Himself a product of the most varied culture, he desired that none of the brilliant faculties early apparent in his two elder sons should rust in disuse. They were accordingly taught to sing and play the lute as well as to compose Latin verses ; they attended the juridical lectures of Vinnius, and studied mathematics under Van Schooten ; they were accomplished in dancing and drawing no less than in Greek, rhetoric, and logic ; they travelled to see the world and improve their manners ; they could, as occasion required, play the courtier, or work as skilled mechanics. The native turn of each was, however, different. Constantine excelled in the lighter branches of literature ; Christian promptly shot ahead of him in geometry. Study and invention went, with him, in this direction, hand in hand. Before he was seventeen, he had begun to strike out original lines of investigation, and the promise of these juvenile essays was discerned, among the first, by Descartes. Mersenne about the same time opened a correspondence with him, and predicted for him greatness beyond that of the towering figure of Archimedes.

He made his *début* in print in 1651 with a treatise on quadratures, to which he appended a refutation of the theorems on the same subject of Gregory of St. Vincent, with the unusual result of gaining (besides many admirers) a friend in the person chiefly interested in the controversy. The little book was received with acclamations of praise. At once and everywhere, the genius of its author was acknowledged. The mathematicians of France, England, and Germany vied with those of Holland in doing him honour. He was lauded as "Vieta redivivus," placed on a level with Pappus and Apollonius, hailed as the great coming light of science. Yet it was not in pure mathematics that his brightest laurels were to be gathered. Many lesser men did more to help on the great revolution in method which signalized his age. He remained, throughout its progress, constant to the ancient models, and looked on, indifferent or averse to changes the full import of which he failed to realize. His extraordinary ability was, however, never more conspicuous than in his successful grappling with problems—such as that of the isochronous curve—unapproachable by geometers of a more common-place type without the aid of the calculus ; and there is reason to think that, had he lived longer, he would have reinforced his powers by its adoption. It appears from a letter of Leibnitz to him, of October 1, 1693, that he was just then, eighteen months before his death, "beginning to find the convenience" of the infinitesimal mode of calculation, and had gone so far as to express publicly his approbation.

The most interesting part of the correspondence now before us refers to Huygens's observations on Saturn. As early as November 1652, we find him making inquiries as to the best manner of preparing and polishing lenses. Assisted by his brother Constantine, he prosecuted the subject with a diligence for which he half apologized to his learned friends, and which produced unwelcome gaps in his communications with them. By the commencement, accordingly, of 1655, he was in possession of a telescope of 12 feet focal length, undoubtedly the best produced up to that date. It showed him, not only the phases of Venus and the satellites of Jupiter, but—March 25, 1655—"aliud quid memorabile," unseen by Fontana or Hevelius, namely a Saturnian moon, afterwards named Titan, the sixth counting outward from the planet, the first in order of terrestrial detection. He concealed and endeavoured to secure his discovery, after the fashion set by Galileo, in an anagram which was widely circulated, and expounded in the following year. The precaution was nevertheless insufficient to prevent a claim to priority being put forward. Dr. Wallis, the Savilian Professor of Geometry, prepared on behalf of his friends Wren and Neile, a storage-battery of fame in the shape of a counter-anagram, which—if Huygens's private notes are to be relied upon—he fraudulently interpreted as an announcement similar in purport to that imparted to him from the Hague. Some unexplained circumstance possibly underlies a transaction on the face of it highly discreditable to our countrymen. The pretensions of the English observers were at any rate quickly and quietly withdrawn, and Huygens was left in undisturbed enjoyment of the credit most justly due to him.

Shortly after his return from Paris, late in 1655, he constructed a telescope of 23 feet, magnifying one hundred times; and the comparison of the observations it afforded him with those of the previous year enabled him at once to penetrate the mystery of Saturn's enigmatical appendages. His hypothesis as to their nature, wrapt up in the customary logogryph, was appended to his little tract on the Saturnian satellite, with an accompanying prediction of the future changes of figure to be expected in the planet. Its verification, however, falls outside the limits of the publication we are at present concerned with. Nor does it include any mention of the novel sight disclosed to Huygens by his improved instrument in the constellation of Orion, where a certain "hiatus" in the firmament permitted (as he supposed) the pure, faint splendour of the empyrean to shine through on his amazed vision.

Huygens had an eminently sane and sagacious mind. His fortunate intuitions were numerous, and the investigations they suggested were singularly solid and complete. A great part of his work was thus fitted to be, and has actually become, the substructure of the modern scientific edifice. He was, however, less happy in the few cases in which, relaxing his habitual prudence, he gave the rein to speculation. His prevision that the measure of discovery in the solar system was filled by the disclosure of Titan, was belied with scarcely civil haste by Cassini's further detections hopelessly overthrowing the numerical balance between six primary and six secondary bodies. And the surmises which constituted the bulk of his "Cosmotheoros" were, for the most part, infelicitous. Yet he reprehended, as woven out of figments, the Cartesian theory of the origin of the universe, and concluded with the wise and memorable words:—"To me it would be much if we could understand how things actually are, which we are far enough from doing. How they were brought about, what they are, and how begun, I believe to be beyond the range of human ingenuity to discover, or even by conjectures to approach."

A. M. CLERKE.

NORWEGIAN GEOLOGY.

Bömmelöen og Karmöen med Omgivelser. Geologisk beskrevne af Dr. Hans Reusch. (Kristiania: Published by the Geological Survey of Norway, 1888.)

THE attention of geologists in all parts of the world has for some years been concentrated upon the crystalline schists, which have so long presented insuperable difficulties to those who would explore their origin. Little by little the darkness has been rising from these ancient foundation-stones of the earth's crust; and though a long time must probably still elapse before their history can be even approximately sketched, there can be no doubt that we are now at last on the right road of investigation. Fresh evidence is continually being obtained from the most widely-separated regions, and each additional body of facts goes to support the view that the schistose rocks are the records of gigantic terrestrial displacements, whereby portions of the crust have been pushed over each other, and so crushed and deformed as to acquire new internal rock-structures. Out of these mechanical movements, with their accompanying chemical transformations, a true theory of metamorphism will no doubt eventually be evolved. In the meantime it is too soon to generalize; what we need is a far larger mass of observations. The subject is a wide one, for it involves the labours of the field-geologist, the petrographer, the mineralogist, the chemist, and the physicist. And only by the united exertions of these fellow-workers can we hope for good progress and solid results.

The most recent contribution to the question of the origin of the crystalline schists has just appeared in the form of a handsome volume, by Dr. Hans Reusch, on the Bömmel and Karm Islands off the mouth of the Hardanger Fjord. It consists of a mass of detailed observations on the structure of the crystalline rocks of that part of the Scandinavian coast, and furnishes an admirable array of fresh data for the study of the problems of regional metamorphism. Dr. Reusch's previous researches on the compressed conglomerates and metamorphosed fossiliferous rocks of the same district were of the utmost value in the discussion of the question, and he now augments these by new details from the surrounding region.

Especially important are the numerous illustrations of the effects of pressure and stretching in the production of the well-known structures of the crystalline schists. The strangely deceptive resemblance to stratification resulting from these processes is exhibited in many examples. Excellent instances are likewise given of the production of foliation in dykes. Eruptive diabases and gabbros are shown to pass into dioritic rocks, and hornblendic schists and granite into various foliated compounds. More novel features of the essay are the careful studies of the deformation and foliation of what were unquestionably at one time ordinary sedimentary deposits—sandstones, conglomerates, and limestones. It is shown, for instance, that in a mass of still recognizable conglomerate the planes of stratification are cut across, almost at right angles, by those of foliation, while the lines that mark the direction of stretching or deformation slant upwards across the latter.

Dr. Reusch brings forward some remarkable observations regarding the connection between conglomerates and granitic rocks. He thinks that in some places what is now granite has resulted from the metamorphism of what was originally a breccia or conglomerate composed of fragments of granite, gneiss, quartzite, and quartz. The quartzite and quartz, being less liable to change, remain still visible, while the granite and gneiss have passed into common granite. In another locality he finds what he believes to be evidence of the passage of a conglomerate into augen-gneiss. Without in any way calling in question the accuracy of his observations, a geologist who has had much experience among the crystalline schists in districts where great thrust-planes and other proofs of powerful displacements prevail, will recall examples of breccias that might at first be taken to be sedimentary masses, but which have eventually proved to be portions of rocks crushed during the disturbances that produced the schistose structure. Coarse pegmatites, for example, may be traced through various stages of comminution, until they pass at length, along the plane of movement, into finely fissile rocks, that in some cases might be mistaken for shales, in others for eruptive rocks with the most exquisitely developed flow-structure. The "eyes" in some augen-gneisses are almost certainly fragments resulting from the crushing of largely crystalline rocks, such as coarse pegmatites.

Dr. Reusch shows that in Scandinavia, as in the north-west and north of the British Isles, the axes of the great terrestrial plications run, on the whole, from north-east to south-west, and that as they have involved Upper Silurian strata in their folds, the movements must be of later date than some part, if not the whole, of the Upper Silurian period. His essay is most welcome as a valuable contribution to one of the most perplexing problems in geology. It once more shows him to be a careful and intrepid field-geologist, and, at the same time, a skilful worker with the microscope. This combination of qualifications fits him in a special manner for the researches to which he has devoted himself with so much ardour and success. His volume is copiously illustrated with figures in the text, and a selection of coloured geological maps. English geologists will also welcome in it a copious English summary of the contents. We may confidently predict that, before long, some of his drawings will be reproduced in the text-books as standard representations of the facts of regional metamorphism.

A. G.

TRAVELS IN ARABIA DESERTA.

Travels in Arabia Deserta. By C. M. Doughty. 2 Vols. (Cambridge: University Press, 1888.)

MR. DOUGHTY'S book takes us back to the age of the old travellers. His wanderings were in countries where not only no European had preceded him, but where he had to travel with his life continually in his hand. He travelled alone, and without any of the equipment which the modern explorer considers a necessity of existence, living with the Beduin of the desert, and sharing with them their wretched subsistence. Even the style in which he writes is a style in which it is safe to say no Englishman has written for the last two hundred years, and while it attracts us by its quaintness it makes us not unfrequently wonder what is exactly the author's meaning. Indeed, were it not for the very excellent index, it would often be almost impossible to find one's way through the labyrinth of Mr. Doughty's sentences or to ascertain the exact chronology of his route.

Mr. Doughty seems to have been born under an evil star. While he possesses most of the requisites of a successful traveller—a love of adventure, an insatiable curiosity, indomitable patience, and extraordinary powers of endurance—he lacks, on the other hand, just those qualities which would have smoothed his journey and made his life more comfortable. He is a man, by his own confession, of blunt and plain speech, improvident and forgetful, with an old-world belief in the falsity of Mohammedanism and the Koran, and the iniquity of countenancing them even by a politic word. His explorations took place at the time of the war between Turkey and Russia, when the fanaticism of the Mohammedans of Arabia was excited to the utmost, and he had to leave Damascus at the outset of his journey without any letters or help from the British Consul. The latter, indeed, declared that "he had as much regard of" him, would he "take such dangerous ways, as of his old hat." It is no wonder that Mr. Doughty complains of conduct which caused him "many times come nigh to be foully murdered."

His explorations were conducted in Central Arabia, a country which is less known than Central Africa. He accompanied the Mecca pilgrims as far as "the kella" or fort of Medain, where he lived with the Turkish garrison, visiting from time to time the ruins of Medain Salihh, and taking squeezes of the Nabathean inscriptions there. After some months he joined the nomad Beduin, and wandered with them in various directions, visiting the lava crags on the west and Teyma on the north-east. Eventually he made his way to Háyil in the Nejd—a centre of Wahabi fanaticism—where a sort of settled government was established under Ibn Rashîd. From Nejd he was forwarded, along with some Beduin, to Kheybar, not far to the north of Medineh, where he found himself once more within what was nominally Turkish territory, and was arrested as a spy. Released after a while, he was sent back again, for reasons which are never explained, to Háyil, and here his troubles began. The people of the place would not receive the Christian stranger a second time; his Beduin escort were afraid of bringing him back to Kheybar, and after a series of misadventures he was finally deserted near Aneyza, a town considerably to the south of Háyil. The governor and leading merchants of Aneyza fortunately befriended him, and he at last found his way to Taif and Jedda, though not without being first stripped of the little that still belonged to him, and narrowly escaping with his life.

Mr. Doughty was a careful observer, and he has not only made important additions to our geographical knowledge of Arabia, but also to our geological knowledge of it. The inscriptions he obtained at Medain Salihh and elsewhere have been published by the French Government, and important inferences have been drawn from them. They prove not only that a powerful and civilized State existed in this part of Arabia far on into the Christian era—a fact which was already known—but that this State was Nabathean in its language and character. M. Berger has come to the conclusion that before the rise of Mohammedanism the Arabic of the Koran was the language of Mecca only and the surrounding district, the Nabathean with its Aramaic affinities prevailing in the northern part of Arabia, and the Himyaritic in the south. It seems clear, at all events, that the Nabathean and Himyaritic civilizations once adjoined one another, and that their overthrow marked the triumph of the Beduin children of Ishmael. Since Mr. Doughty's travels, Prof. Euting and M. Huber (who was afterwards murdered by the Hharb Arabs) have visited Medain Salihh and Teyma, and carried away with them a large number of valuable inscriptions. One of these, on a stêlê discovered at Teyma, is now in Paris.

It is interesting to find Mr. Doughty confirming the statement that the final *n* of classical Arabic is still pronounced in the Nejd. His remarks on the diseases prevalent among the natives are also curious, though it is difficult to believe that the ophthalmia from which he had himself suffered is due to drinking cold water before going to bed. Everyone, however, who has had much experience of the Beduin will agree with the character he gives of them. The Egyptians have a proverb: "He who shows a Beduin the way to his door will have long sorrow"; and the traveller is unfortunate who is compelled to intrust himself to their tender mercies.

A. H. S.

OUR BOOK SHELF.

Charts showing the Mean Barometrical Pressure over the Atlantic, Indian, and Pacific Oceans. (London: Published by the Authority of the Meteorological Council, 1888.)

THESE charts are issued in the form of an atlas, and deal in a very complete manner with the barometer means and range of all oceans. The months for which separate charts are given are February, May, August, and November, which have been selected to represent the mean values for winter, spring, summer, and autumn respectively in either hemisphere. In addition to the large charts, which give the material in considerable detail, there are four index charts, on a smaller scale, which exhibit for the same months the isobars, or lines of equal pressure, over the entire globe. These are followed by four charts, on the same scale, showing the range of barometrical pressure. The observations have been derived from logs and documents deposited in the Meteorological Office; logs and remark-books of Her Majesty's ships, furnished by the Admiralty; published narratives of various voyages, and various published results of other nations; also observations at coast stations and islands obtained from all available sources. The number of observations obtained from the Meteorological Office logs for the several oceans are: the Atlantic Ocean, 339,300; the Indian Ocean, 162,000; the Pacific, 88,300.

The barometrical means are given in large figures for areas of 5° of latitude by 5° of longitude, and for the benefit of those who require the material in greater detail smaller figures are given to show the means for areas of 2° of latitude by 2° of longitude, the several means being obtained from the daily averages. The range to the nearest tenth of an inch for each 5° area is placed over the mean for that area, and the number of observations under it; so that the charts not only supply the navigator with all the detail he is likely to require, but afford opportunity of the values being combined by other compilers with material of a similar nature. The isobars are given for each tenth of an inch, and the free use which has been made of the barometrical values for the coast stations greatly enhances the degree of dependence of the several lines. To facilitate the use of the charts for the navigator, the observations are corrected for a constant altitude of 11 feet above the sea, and are reduced to 32° F., but are not corrected for gravity; a table is, however, given on the face of each chart to facilitate this correction.

The general charts which give the isobars of the globe show very conspicuously the prevalence of high-pressure areas in each ocean in each of the four seasons. Change is of course shown in the distribution of pressure, but there is the same tendency to the persistency of high reading. It is seen that these areas oscillate and alter somewhat in intensity with the season, but there are many characteristics in common. The northern Indian Ocean, which is much more surrounded by land, is, however, an exception, the high pressure being situated over the northern part of the ocean, in November and February, and decreasing southwards; whilst in May and August the pressure is lowest in the north and increases southwards, this change being intimately related to the monsoon winds. The charts of range show well the influence of season, the largest differences occurring in the winter months in each hemisphere. In February the range to the west of the British Islands is 2·0 inches, whereas in August it is only one-half as great. The effect of latitude on the amount of range is very evident, the values near the equator being very small. These charts, which have been compiled by Nav.-Lieut. Baillie, R.N., are considerably in advance of any previous work of a similar nature, and will materially aid in explaining the general circulation of the wind over the globe, barometric pressure and wind being so intimately co-related.

Commercial Mathematics. (London: Longmans, Green, and Co., 1888.)

THIS volume is the continuation of a series of books on commercial education, and specially adapted for candidates preparing for the Oxford and Cambridge Schools Examination Board. Arithmetic is first dealt with, the first chapter consisting of an account of the decimal system in France. Moneys, weights, and measures, of Germany, Italy, Spain, Portugal, and Russia, are next discussed, followed by numerous examples; and the first part concludes with a chapter on "Exchange." Algebra is the subject of Part II., which extends as far as quadratic equations, including involution and evolution, and a chapter on the methods of testing algebraical results. The examples are very numerous throughout, and the book ought to be much in demand by the above-mentioned students and others. The volume concludes with a list of results of the various examples.

A Wanderer's Notes. By W. Beatty-Kingston. In Two Vols. (London: Chapman and Hall, 1888.)

FOR about thirteen years Mr. Beatty-Kingston acted as a newspaper correspondent, and in this capacity he had to visit many centres of life on the Continent. In the present volumes he offers a selection from the innumerable pen-and-ink sketches taken during his "multifarious peregrinations." The work, we need scarcely say, has no strictly scientific interest; but it is fresh and amusing, and will no doubt give pleasure to many a reader who has never had an opportunity of seeing the places described in its lively pages. The author is particularly successful in the chapters devoted to Germany, where he seems to have had exceptional means of making himself acquainted with the characteristics of the various classes of the community.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The "Sky-coloured Clouds" again.

THESE clouds have reappeared. Last night was the first occasion I have noticed any very distinct display of them this year; but I first saw them on June 12, and again on the 14th; and I think I saw them on June 13 and 17, but was not sure. Previous to that, on May 15 and 16, the green sky, when the sun had set, was of unusual brightness, showing, as I thought, a tendency to the formation of these clouds. Each summer they appear to be growing fainter since they were first generally noticed in 1885.

This year's observations were made in Cornwall, with the exception of last night's, which was at Sunderland.

Sunderland, June 26. T. W. BACKHOUSE.

Earth Pillars in Miniature.

I HAVE taken two photographs of an interesting specimen I obtained from the cliffs here. The stone is composed of very fragile sand-rock containing fragments of flint. A large mass of this became detached from the higher part of the cliff, and some of the pieces chanced to fall on a ledge upon which dry sand was constantly pouring in windy weather. The action of this falling sand wore away all parts of the surface of the stone save those protected by the small embedded fragments of flint, and hence the formation of these miniature pillars.

Owing to the extreme incoherency of the substance, I unfortunately lost one of the most perfect pillars before the photograph was taken.

I conclude that the formation of these pillars was the work of a very few days—perhaps hours. On visiting the spot a few days later, all traces of sand-action had been obliterated by rain. An analogous case was that described by Mr. Blake ("Geol. Miscell. Tracts," 10) as occurring in the Pass of San Bernardino, California; the surface of the granite had been worn by blown sand, but the garnets therein stood out in relief upon long pedicles of feldspar, as a proof of their superior hardness. CECIL CARUS-WILSON.

Bournemouth, June 23.

Egg-masses on *Hydrobia ulvæ*.

CAN any of your readers give me information in regard to the eggs of the Gastropod *Hydrobia ulvæ*?

At a recent excursion of the Biological Society to Hilbre Island, while crossing the great stretch of wet sand which lies in the estuary of the Dee, it was noticed that the surface was covered in some places with vast numbers of *Hydrobia*. Some of these were brought back to the laboratory in their wet sand; and, on being put in a dish of sea-water, the mollusks were found next day to have crawled out of the sand, and I then noticed that nearly every specimen had several little rounded excrescences scattered over the surface of its shell. On examining these, it was found that each was a little mass of small sand grains, in the centre of which was a clear jelly containing several segmenting ova or young embryos. They were undoubtedly molluscan eggs, as I kept them alive until one or two had reached a veliger stage; but did they belong to the *Hydrobia* or to some other mollusk? No other mollusk was, however, noticed in any abundance in the neighbourhood. Has, then, the *Hydrobia* acquired the habit of laying its eggs upon its neighbours' shells, as being the only comparatively stable objects to be found in the fine shifting sands around it? Possibly the method of oviposition of *Hydrobia* is already known, but I have not come across any reference to it. W. A. HERDMAN.

Zoological Laboratory, University College, Liverpool, June 23.

Interpretation of the Differential Equation to a Conic.

MAY I ask, with reference to Mr. Asutosh Mukhopadhyay's geometrical interpretation of the above in NATURE of the 21st inst., how to draw a *curve* at *every* point of which the radius of curvature vanishes, or the curvature is infinite?

Is it not evident that the osculating conic of a conic is the conic itself, and the "aberrancy curve" therefore a *point*, the centre of the conic?

The "sought found," then, is the fact that a conic is a conic!

June 24. R. B. H.

The Nephridia of Earthworms.

THE last number of the *Quarterly Journal of Microscopical Science* has just come into my hands, containing a paper, by Mr. Beddard, on the nephridia of certain earthworms. In November of last year I read a paper, before the Royal Society of Victoria, on the anatomy of the large Gippsland earthworm, *Megascolides australis*. This, which reaches the length of 6 to 8 feet, is, I believe, the largest recorded earthworm, and its nephridial system is of great interest, corresponding closely in many points to that described by Mr. Beddard, in the above paper, as present in *Acanthodrilus multiporus* and *Perichæta aspergillum*. My drawings have been for some time in the lithographers' hands, but as it will still be one or two months before the full paper is published, I should be glad to draw attention to the, in some ways, still more interesting features of the nephridial system in *Megascolides australis*. The nephridia are very evident, and can be divided clearly into two sets.

(1) A great number of small vascular-looking little tufts lining the body-wall, save in the mid-dorsal and ventral lines, especially abundant in the segments containing the reproductive organs (segments 11–19). They have no internal opening.

(2) A series of much larger nephridia, one pair of which only is present in each of the segments in the middle and posterior regions of the body—that is, from about segment 120 to segment 500, or whatever may be the number of the last segment, which varies according to the worm's size. They are placed in the anterior part of each segment, whilst the smaller nephridia form a ring round the body-wall posteriorly. Each one has the usual ciliated funnel opening through the septum into the segment in front.

Throughout the body, where the smaller nephridia occur, there is a network of intra-cellular ducts lying immediately beneath the peritoneal epithelium in connection with the nephridia, and giving off an irregularly arranged series of branched ducts opening externally. Ventrally, also, there appears to be on either side, in the middle and posterior portions of the body, a longitudinal duct running from segment to segment within the most ventral pair of setæ: into this duct open, first, the larger nephridia, and, secondly, the most ventrally placed small nephridia of the same segment; the latter, again, are united with the network of ducts connected with the ring of smaller nephridia.

In the case of the latter there appear to be two somewhat differently formed sets of external openings. All over the body, except in the clitellar region, where there is a great glandular development in the body-wall, the duct leading to the exterior is intercellular, small, and composed of minute cubical cells; in the clitellar region, on the other hand, the duct, though similarly intercellular, is much swollen out, slightly coiled, and always provided with a distinct coiled blood-vessel running by its side: its lining cells form a flattened epithelium.

The external opening itself is formed of cells of the epidermis, so modified as to present very much the external appearance of a taste-bulb—that is, they form a sphere with the cells thicker in their middle parts, and the two ends attached to the poles of the sphere, the duct passing right up through the centre. This structure of the external opening is common to all the ducts in the body, but is more clearly made out in the case of those referred to.

The large size and ciliated funnels of the paired nephridia distinguish these clearly from the more numerous smaller ones, which are devoid of internal openings, and are without a doubt homologous with those of *Acanthodrilus* and *Perichæta*. At the same time it is important to note that histologically the network of ducts and the longitudinal duct, which are intimately connected with each other, are precisely similar in structure, and, *a priori*, might be expected to have a similar origin, *i.e.* to be derived from the same germinal layer.

Leaving out of consideration at present the question dealt with by Mr. Beddard and others as to the homology of the larval nephridia of Chætopods, and assuming the existence of a genetic relationship between the adult nephridial system of Platyhelminths and Chætopods, the following questions suggest themselves with regard to the various nephridial structures present in different forms:—

(1) Are the longitudinal ducts in *Lanice*, the embryo of *Lumbricus* and *Megascolides*, homologous with each other? Before this can be determined the development of each must be known.

(2) Granted, of which there can be little doubt, that the smaller nephridia of *Megascolides* are homologous with the nephridia of *Perichæta* and *Acanthodrilus*, are not the large nephridia of the former, which are completely wanting in both

of these, homologous with the nephridia of other worms, such as *Lumbricus*, to which they are at all events suspiciously similar in arrangement and structure?

(3) What is the relationship of the large to the smaller nephridia? Are they modifications of the latter, or independent later developments?

(4) In either case the Platyhelminth system must be more closely represented by the small nephridial bodies devoid of internal openings and provided with a network of ducts such as is found in *Perichæta*, *Acanthodrilus*, and *Megascolides*, than by the more specialized paired nephridia of such a form as *Lumbricus*.

Possibly the course of development as represented in living forms may be somewhat as follows:—

(1) A series of numerous nephridia present in each segment devoid of internal openings, and connected by a continuous network of ducts, as in *Perichæta*.

(2) The aggregation of these smaller nephridia into tufts in various parts, as in the posterior region of *Acanthodrilus*; the subsequent enlargement of certain of these nephridia and the acquirement by them of secondary internal openings. It is interesting to note in *Megascolides* that in the anterior part of the body, where the small nephridia are scattered over the whole body-wall of the segment, large nephridia are absent, whilst they are present in the posterior region, where the small nephridia are confined to a ring in the posterior part of the segment. In this case, as the nephridia become aggregated into tufts in the anterior part, the ducts connecting them with those in the posterior region of the segment next in front will become fewer, until when, as in *Megascolides*, only a single, modified, large nephridium remains on either side anteriorly, there will be simply one duct from segment to segment uniting with a network of ducts in the region where the small nephridia still persist.

It is interesting to note that the aggregation of the smaller nephridia, and on this supposition the modification of certain of them to form the larger ones, commences in the posterior region of the body.

In certain worms, such as *Acanthodrilus*, the connection of the network of ducts from segment to segment seems to have been lost, at any rate in the adult: aggregation of these in the neighbourhood of the setæ, and subsequent modification, would give rise to a certain number of nephridia in each segment without any longitudinal duct.

(3) The next stage is reached in such a form as *Lanice*, where the longitudinal duct persists, but all trace of the smaller nephridia is lost.

(4) The final stage is present in most earthworms where, in the adult, all traces of both small nephridia and longitudinal duct are lost, though the latter is present, as in *Lumbricus*, during development.

These lead to three conclusions, two of which are practically identical with those of Mr. Beddard:—

(1) That the smaller nephridia without internal openings, irregularly scattered, and with a network of ducts such as are seen in *Acanthodrilus*, *Perichæta*, and *Megascolides*, are homologous with the nephridial system of Platyhelminths.

(2) That the larger nephridia typical of most earthworms are secondary modifications of certain of the smaller ones subsequent to their aggregation into groups; the modified ones acquiring each an internal opening.

(3) That there is no homology between the longitudinal duct of *Lumbricus*, *Lanice*, *Megascolides*, &c., with that of the Platyhelminths, since it has only been developed in the above forms in connection with the larger nephridia and as a modification of the original network, and has thus had its origin within the Chætopod group. W. BALDWIN SPENCER.

Melbourne University, May 3.

Strange Rise of Wells in Rainless Season.

MY attention has been directed to a letter published by you a few weeks ago (May 31, p. 103) under the above heading. It would appear that there is something mysterious in the eyes of the author of the communication in question in the fact that the water in two wells at Fareham rose several feet in the month of March, as he states, "after a continuance of north-east wind, *without rain*, but with half a gale blowing"; so that it would appear that there was some connection between the north-easterly gale and the rise of the water.

In this, however, the author is entirely mistaken; the rise of water in the wells in question is nothing more than the ordinary seasonable rise due to percolation. For twelve years past I have been carrying on constant observations of the underground water-supplies in various parts of this country, and it is quite true, as mentioned by the writer of the letter, that ordinarily the water in wells rises in the winter and falls in the summer; but this is by no means an exceptional rule, for in the present season there have been two low waters, the last of which occurred in the southern counties on the 8th of March in the present year. After that date commenced a very wet period, and before the end of the month over $2\frac{1}{2}$ inches of rain had absolutely passed through the ground as measured by my percolation gauges. The water in a well on the Surrey hills, which had been falling up to March 8, rose before the end of the month over 30 feet, which rise was entirely due to the replenishment from rainfall. I may point out that there are many wells at the present time in which the water is still rising, while in others in the same districts the water is falling, for the simple reason that as a rule underground water follows the same law as water flowing in a river, and that the floods or high waters descend from the highest to the lowest districts, so that at present in wells situated in high positions the water is falling, while the crest of the wave of high water in the same watershed has not yet been reached in the lower levels of the district.

That the water in wells *does* fluctuate under certain conditions of the wind there is no doubt, as I have already drawn attention both to the fluctuations which take place in the water-levels of wells under barometric pressure and also in the volume of water discharged from the ground with a fall of the barometer. It should be noted that the rise of water in wells when due to barometric changes coincides with the fall of the barometer. Now a north-easterly wind as a rule is accompanied by a high barometer, and therefore is not likely to influence the rise of water in a well. During the month of March the rainfall was above the average, while there were comparatively few days with easterly winds, the only time when it could be termed a half-gale from the north-east occurring on the 19th of March, by which time the water in all the wells had made a considerable rise, due simply to ordinary percolation. Thus there is no mystery attaching to the rising of the water in these wells at Fareham. The rise simply took place from the replenishment of the springs, which this year occurred at a period somewhat different from ordinary years. BALDWIN LATHAM.

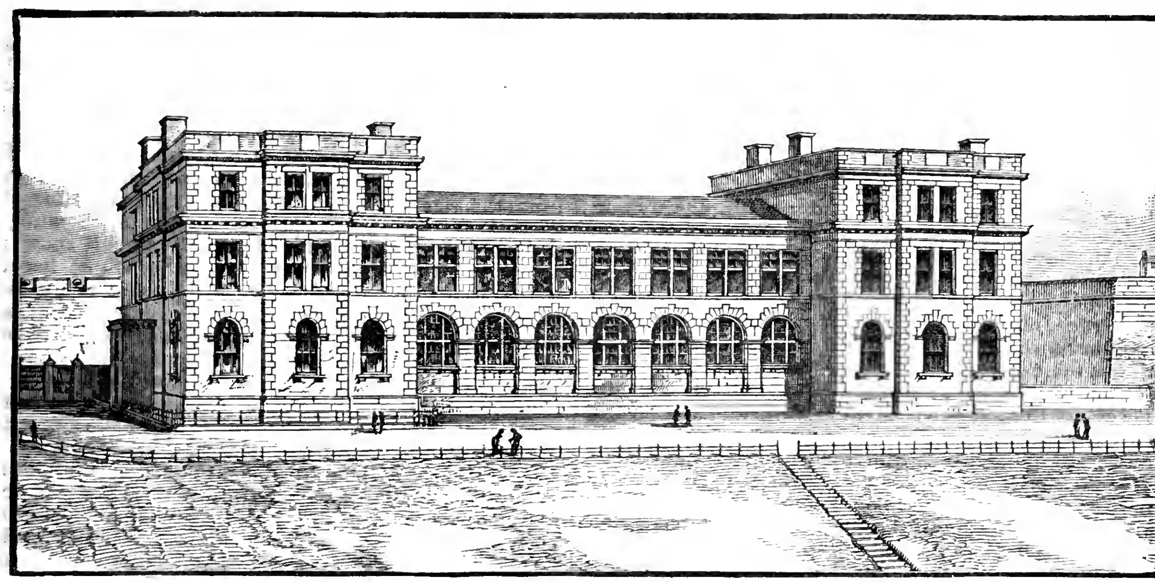
7 Westminster Chambers, Westminster, June 21.

THE OPENING OF THE MARINE BIOLOGICAL LABORATORY AT PLYMOUTH.

THE Laboratory at Plymouth, which is now ready for work, is remarkable as being the first institution in this country designed purely for scientific research which has been originated and firmly established by the efforts of scientific men appealing to the generosity and confidence of wealthy individuals and corporations who desire the progress of knowledge for practical ends and the general good of the community.

It may be said that the Marine Biological Association will begin its active career on and after Saturday next. On that day Prof. Flower will, on behalf of the Association, declare that the Laboratory at Plymouth, which is now complete, is open for the purposes of biological research. The opening of the Laboratory may be said to mark an epoch in English zoological science, just as the opening of the Stazione Zoologica at Naples, which is essentially a German undertaking, marked an epoch in German science. It is true that small sea-side laboratories have already been established in the United Kingdom—at Granton, St. Andrews, and Liverpool Bay; but none of them can compare with the present undertaking in size and importance, and none can offer such advantages to the investigator.

The present institution, it may be remembered, is historically the outcome of the International Fisheries Exhibition held in London in 1883. That Exhibition served partly as an amusement to Londoners, but it also performed a far more important service—it directed people's minds towards the importance of our fisheries, and made them in some slight degree acquainted with the conditions under which those fisheries are worked. At the close of the Exhibition a large balance was left in the hands of its promoters, and it was hoped by many leading men of science that the money thus obtained would be utilized, in part at least, for the purpose of encouraging investigations upon the habits and economy of food-fishes. But the money was appropriated to other purposes, excellent in themselves, though useless as a means of promoting the welfare of the fishing industry. Prof. Lankester, however, nothing daunted by this want of success in obtaining funds from the surplus of the Fisheries Exhibition, and feeling that it was time to strike whilst people's minds were awakened to the importance of our fisheries and to the lack of scientific knowledge concerning them, determined to found an Association for the purpose of encouraging the study of the marine fauna of the British coasts, and with the consent and co-operation of the officers of the Royal Society called a meeting for this purpose in the rooms of the Society on March 31, 1884. The meeting was eminently successful. The Duke of Argyll proposed a resolution to found the Marine Biological Association of the United Kingdom, and was supported by the most eminent biologists in the country. An appeal was made for subscriptions in aid of the Association's projects, and was soon liberally responded to. His Royal Highness the Prince of Wales graciously consented to be patron of the Association, and gave liberally to its funds; the scientific Societies, the City Companies, the Universities, and finally Her Majesty's Government, joined the list of subscribers; and in a short time the Association was in a position to undertake the building of a laboratory. After some debate as to the most suitable locality for a laboratory, Plymouth was selected, partly because it is a large and important fishing port, partly because the richness of the marine fauna of the Sound and neighbouring shores was extolled by such eminent authorities as the late Dr. Gwyn Jeffreys, Mr. C. Spence Bate, and Prof. Charles Stewart. The Association was fortunate in securing a magnificent site for the Laboratory from the War Office. For this site, than which a better could not be found, the Association is greatly indebted to the Earl of Morley,



South Front of the Laboratory of the Marine Biological Association, on the Citadel Hill, Plymouth.

then Under-Secretary of State for War, and to Sir Andrew Clarke, Inspector-General of Fortifications. The site granted is that part of the fosse of the Citadel lying to the south of the portion of the Citadel wall known as King Charles's Curtain; it has a frontage towards the sea of 265 feet, and extends some 240 feet southwards of the Citadel.

The Laboratory which has been erected upon this site is admirably adapted to the purposes of the Association. It is, indeed, more than a laboratory, it is also an aquarium, whose tanks are extensive and fitted with every improvement that modern science can suggest. The total cost of building, machinery, and fittings, including all fees, has been about £12,500. The structure comprises a central portion with a wing at either end. The east wing is almost wholly taken up by the residence of the Director, and needs no further comment. The west wing has on the ground floor the caretaker's rooms, and a receiving-room into which the results of the day's fishing will be brought for examination. On the first floor are chemical and physiological laboratories, and on the second floor a library, a work-room, and lavatory. The main part of the building contains on the ground floor the aquarium or tank-room, and on the first floor the large laboratory. The tank-room is fitted with slate and glass tanks, of which one on the northern side is a noble window tank, 30 feet in length, 9 feet in breadth, and 5 feet deep. There are three large window tanks on the north side, nine smaller window tanks on the south side, and a series of five table tanks in the middle of the room. The tanks are supplied with salt water from two reservoirs, capable of holding 50,000 gallons each. From these the salt water is led by means of pumps through vulcanite pipes into the tanks; the openings of the pipes are placed rather more than a foot above the level of the water in the tanks, and are provided with nozzles through which the water is forced at high pressure, so as to form jets descending deep into the tank and carrying with them a quantity of atmospheric air. Circulation has been established in the tanks for the last fortnight, and there is every reason to be satisfied with the arrangements for aërating the water. The jets carrying down the air deep into the water of the tank cause it to be filled with minute bubbles so as to resemble champagne, and all the animals that have hitherto been placed in the tanks are thriving in a remarkable manner, which is the more surprising as new tanks are generally supposed to be highly injurious to organisms introduced into them at an early a date. It would be too much to expect that tanks which have been so lately put up should be fully stocked within a fortnight, nevertheless they will present to the visitors on Saturday next a sufficiently interesting collection of local marine forms. For the rest the tank-room is a plain room, without any attempt at ornamentation. It is felt that the scientific nature of the institution must be kept in the foreground, and therefore nothing has been done to make the aquarium a place of popular amusement.

The main laboratory is at present fitted with seven compartments, each to contain a single naturalist, along its north side. When the necessity arises, similar compartments will be placed along the south side. In the centre of the room is a series of slate and glass tanks supplied with salt water from the circulating pumps. Beneath these a convenient shelf has been arranged, so that naturalists will be able to arrange for themselves any temporary apparatus that they may devise on as small a scale as is desired. All the arrangements for laboratory work will be completed at the end of the week, and the only thing now required is a company of ardent naturalists ready to undertake the work that lies to hand.

The material for work and for stocking the tanks is obtained from the Sound and the sea outside the breakwater by means of the trawl, dredge, and tow-net. In general a small shrimp-trawl is used in preference to a dredge, as it is much wider and equally effective in collecting the animals that live at the bottom. Hitherto the Association has been content to hire fishing-boats for dredging and trawling. Most of the work has been done in a small hook-and-line boat, the *Quickstep*, of about 6 tons burden, and on special occasions the trawler *Lola*, of 50 tons burden, has been hired. But this method of hiring is too expensive to be continued; the Association will soon have to purchase boats, and probably will find it necessary to acquire a steam-boat. Without a steam-boat the station is at the mercy of the weather. If it is a dead calm—and calms are frequent in summer along the south coast—no dredging or surface netting can be done, a cruel fate when one knows that the pelagic surface fauna swarms thickest on bright calm days. Or if it is wished to explore a certain region on a certain day, if the winds prove contrary more than half the day is lost in beating up to the station; in any case one may generally expect to have a contrary wind on either the outward or the homeward journey. Such losses of time and material are most prejudicial to an institution like the Marine Biological Association. A steam-launch has been found necessary at all other marine stations. Dr. Dohrn has two, the *Johannes Müller* and the *Francis Balfour*, at Naples; and the Granton Station is well provided for by the steam-yacht *Medusa*. But the funds of the Association have been well nigh exhausted in the building of the Laboratory. If a steam-launch is found requisite, it will be necessary to make another appeal to its friends, which, let it be hoped, will be as heartily responded to as the first appeal for funds for building the Laboratory.

It was stated in the early part of this article that the Association would begin its active existence on the 30th. It would have been more proper to say its active public existence, for its staff has been active for some time past. Under the guidance of Mr. W. Heape, the late Superintendent, a careful though necessarily incomplete exploration of the Sound has been made, and numbers of animals have been identified, preserved, and put aside for future reference. Mr. Heape has also drawn up a complete list of the fauna and flora of the Sound, as recorded up to the present date, and a very formidable list it is.[1] Botanists will note that there are more than 250 species of marine Algæ recorded from the neighbourhood, and some of them are extremely rare. Zoologists will see that there is an unlimited field in certain groups, particularly in the Crustacea and the Mollusca, but that some of the most interesting forms, the "pets of the laboratory," such as Amphioxus and Balanoglossus, are absent. But to say that they are absent means only that other less familiar forms are present, and that these old favourites have not been recorded. A good authority states that Amphioxus can be found in the immediate neighbourhood, whilst it is confidently expected that both Balanoglossus and Amphioxus can be introduced from the Channel Isles, and kept alive in the tanks. The zoologist need not fear that he will be hindered by the poverty of the fauna; there is material enough and to spare. The remarkable Hydroid, *Myriothela*, occurs at low-tide mark in considerable quantities. The interesting Actiniæ, *Edwardsia* and *Peachia*, are to be found. Appendiculariæ and Sagittæ are taken in hundreds in the tow-net. *Antedon rosaceus* is abundant a quarter of a mile from the Laboratory, and magnificent specimens of Pinna will attract the interest of the malacologist.

Such an institution as that at Plymouth challenges comparison with Dr. Dohrn's famous zoological station at Naples. But there is this remarkable difference between them. The Naples Station was founded for purely scientific objects: it does not profess to undertake investigations for the benefit of economic interests. The Marine Biological Association receives an annual grant from the Treasury, on the express understanding that it shall conduct researches upon questions relating to the life-history and habits of food-fishes. It must not be supposed that this work is not scientific because it has a practical object in view. Science is not only the art of thinking correctly, but of observing and recording correctly, and correct observations and records of the life-history of our food-fishes are just what are wanted at the present time. The work of Mr. J. T. Cunningham, Naturalist of the Association, is an admirable example of scientific method as applied to a practical investigation. Mr. Cunningham has been working for several months at the development of fishes, with the view of obtaining and artificially fertilizing their ova and rearing their young in captivity. His results are necessarily incomplete, as he has been working in a half-finished laboratory, without gas or water, and under unfavourable conditions as regards boats and men. But he has succeeded in tracing out the life-history of the "merry sole" (*Pleuronectes microcephalus*), and has acquainted himself with such important facts concerning the development of the common sole, that he confidently expects to be able to hatch out the young next season, his experiments this year having failed only for want of the proper apparatus. He has also recorded the interesting fact that the herring spawns continuously from January to June in the Channel, and appears to have no definite breeding-season as it has in northern waters; and has discovered important facts relative to the breeding of the mackerel, conger, and pilchard, which will be made public as soon as his researches are complete. He has now stocked one of the large tanks in the aquarium with conger, and hopes in a short time to give a final opinion on the obscure question of the breeding of this fish. Not less interesting than Mr. Cunningham's researches are those of Mr. Weldon on the breeding of the common lobster, and the rock-lobster or craw-fish (*Palinurus*). Another of the tanks in the aquarium is occupied by the "berried" females of these forms, whose bright colours and active movements are as attractive to the casual spectator as their study is interesting to the zoologist and fisherman. So much has been

[1] Mr. Heape's list will be published in the forthcoming number (No. II.) of the Journal of the Marine Biological Association.

done already by Messrs. Cunningham and Weldon under the most unfavourable conditions that it cannot but be anticipated that when a number of investigators are working under favourable conditions on different groups, but with a common object in view, results of the greatest scientific and practical importance will accrue.

The ceremony on Saturday will be interesting and important. Many of the leading biologists in England will be present, but unfortunately the eminent President of the Association, Prof. Huxley, will be absent on account of ill-health, and so, unfortunately, will Prof. Moseley, one of its most ardent and generous supporters. The Fishmongers' Company have added to their munificent patronage of the institution by undertaking the entertainment of the numerous guests who have been invited to the ceremony; and the Association will be launched on its career of usefulness in a manner worthy of its aspirations, and satisfactory in the highest degree to its energetic promoters. G. C. B.

PERSONAL IDENTIFICATION AND DESCRIPTION.[1]

II.

PERSONAL characteristics exist in much more minute particulars than those described in the last article. Leaving aside microscopic peculiarities which are of unknown multitudes, such as might be studied in the 800,000,000 specimens cut by a microtome, say of one two-thousandth part of an inch in thickness, and one tenth of an inch each way in area, out of the 4000 cubic inches or so of the flesh, fat, and bone of a single average human body, there are many that are visible with or without the aid of a lens.

The markings in the iris of the eye are of the above kind; they have been never adequately studied except by the makers of artificial eyes, who recognize thousands of varieties of them. These markings well deserve being photographed from life on an enlarged scale. I shall not dwell now upon these, nor on such peculiarities as those of hand-writing, nor on the bifurcations and interlacements of the superficial veins, nor on the shape and convolutions of the ear. These all admit of brief approximate description by the method explained in the last article—namely, by reference to the number in a standard collection of the specimen that shall not differ from it by more than a specified number of units of unlikeness. I fully explained what a unit of unlikeness was, and certain mechanical means by which a given set of measures could be compared with great ease and by a single movement with every set simultaneously, in a large standard collection of sets of measures.

Perhaps the most beautiful and characteristic of all superficial marks are the small furrows with the intervening ridges and their pores that are disposed in a singularly complex yet even order on the under surfaces of the hands and the feet. I do not now speak of the large wrinkles in which chiromantists delight, and which may be compared to the creases in an old coat or to the deep folds in the hide of a rhinoceros, but of the fine lines of which the buttered fingers of children are apt to stamp impressions on the margins of the books they handle, that leave little to be desired on the score of distinctness. These lines are found to take their origin from various centres, one of which lies in the under surface of each finger-tip. They proceed from their several centres in spirals and whorls, and distribute themselves in beautiful patterns over the whole palmar surface. A corresponding system covers the soles of the feet. The same lines appear with little modification in the hands and feet of monkeys. They appear to have been carefully studied for the first time by Purkinje in 1822; since then they have attracted the notice of many writers and physiologists, the fullest and latest of whom is Kollman, who has published a pamphlet upon them, "Tastapparat der Hand" (Leipzig, 1883), in which their physiological significance is fully discussed. Into that part of the subject I am not going to enter here. It has occurred independently to many persons to propose finger-marks as a means of identification. In the last century, Bewick in one of the vignettes in the "History of Birds" gave a woodcut of his own thumb-mark, which is the first clear impression that I know of. Some of the latest specimens that I have seen are by Mr. Gilbert Thomson, an officer of the American Geological Survey, who, being in Arizona, and having to make his orders for payment on a camp suttler, hit upon the expedient of using his own thumb-mark to serve the same purpose as the elaborate scroll engraved on blank cheques—namely, to make the alteration of figures written on it, impossible without detection. I possess copies of two of his cheques. A San Francisco photographer, Mr. Tabor, made enlarged photographs of the finger-marks of Chinese, and his proposal seems to have been seriously considered as a means of identifying Chinese immigrants. I may say that I can obtain no verification of a common statement that the method is in actual use in the prisons of China. The thumb-mark has been used there as elsewhere in attestation of deeds, much as a man might make an impression with a common seal, not his own, and say, "This is my act and deed"; but I cannot hear of any elaborate system of finger-marks having ever been employed in China for the identification of prisoners. It was, however, largely used in India, by Sir William Herschel, twenty-eight years ago, when he was an officer of the Bengal Civil Service. He found it to be most successful in preventing personation, and in putting an end to disputes about the authenticity of deeds. He described his method fully in NATURE, in 1880 (vol. xxiii. p. 76), which should be referred to by the reader; also a paper by Mr. Faulds in the next volume. I may also refer to articles in the American journal *Science*, 1886 (vol. viii. pp. 166 and 212).

The question arises whether these finger-marks remain unaltered throughout the life of the same person. In reply to this, I am enabled to submit a most interesting piece of evidence, which thus far is unique, through the kindness of Sir Wm. Herschel. It consists of the imprints of the two first fingers of his own hand, made in 1860 and in 1888 respectively; that is, at periods separated by an interval of twenty-eight years. I have also two intermediate imprints, made by him in 1874 and in 1883 respectively. The imprints of 1860 and 1888 have now been photographed on an enlarged scale, direct upon the engraver's block, whence Figs. 9 and 11 are cut; these woodcuts may therefore be relied on as very correct representations. Fig. 10 contains the portion of Fig. 9 to which I am about to draw attention. On first examining these and other finger-marks, the eye wanders and becomes confused, not knowing where to fix itself; the points shown in Fig. 10 are those it should select. They are those at which each new furrow makes its first appearance. The furrows may originate in two principal ways, which are not always clearly distinguishable: (1) the new furrow may arise in the middle of a ridge; (2) a single furrow may bifurcate and form a letter **Y**. The distinction between (1) and (2) is not greatly to be trusted, because one of the sides of the ridge in case (1) may become worn, or be narrow and low, and not always leave an imprint, thus converting it into case (2); conversely case (2) may be changed into (1). The position of the origin of the new furrow is, however, none the less defined. I have noted the furrow-heads and bifurcations of furrows in Fig. 9, and shown them separately in Fig. 10. The reader will be able

[1] The substance of a Lecture given by Francis Galton, F.R.S., at the Royal Institution on Friday evening, May 25, 1888. Continued from p. 177.

to identify these positions with the aid of a pair of compasses, and he will find that they persist unchanged in Fig. 11, though there is occasional uncertainty between cases (1) and (2). Also there is a little confusion in the middle of the small triangular space that separates two distinct systems of furrows, much as eddies separate the stream

Fig. 9.—Enlarged impressions of the fore and middle finger tips of the right hand of Sir William Herschel, made in the year 1860.

Fig. 10.—Positions of furrow-heads and bifurcations of furrows, in Fig. 9.

Fig. 11.—Enlarged impressions of the fore and middle finger tips of the right hand of Sir William Herschel, made in the year 1888.

lines of adjacent currents converging from opposite directions. A careful comparison of Figs. 9 and 11 is a most instructive study of the effects of age. There is an obvious amount of wearing and of coarseness in the latter, but the main features in both are the same. I happen to possess a very convenient little apparatus for recording the positions of furrow-heads. It is a slight and small, but well-made wooden pentagraph, multiplying five-fold, in which a very low-power microscope, *with coarse cross-wires*, forms the axis of the short limb, and a pencil-holder forms the axis of the long limb. I contrived it for quite another use—namely, the measurement of the length of wings of moths in some rather extensive experiments that are now being made for me in pedigree moth-breeding. It has proved very serviceable in this inquiry also, and was much used in measuring the profiles spoken of in the last article. Without some moderate magnifying power, the finger-marks cannot be properly studied. It is a convenient plan, in default of better methods, to prick holes with a needle through the furrow-heads into a separate piece of paper, where they can be studied without risk of confusing the eye. There are peculiarities often found in furrows that do not appear in these particular specimens, to which I will not further refer. In Fig. 10 the form of the origin of the spirals is just indicated. These forms are various; they may be in single or in multiple lines, and the earlier turns may form long loops or be nearly circular. My own ten fingers show at least four distinct varieties.

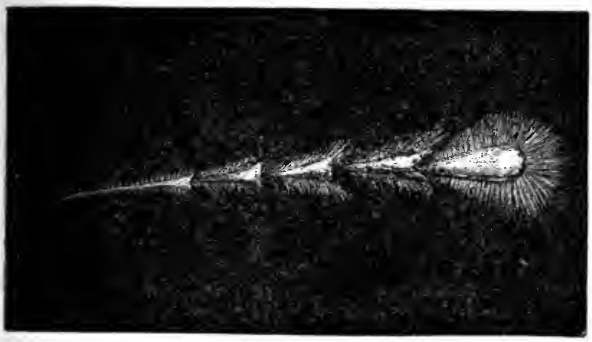
Notwithstanding the experience of others to the contrary, I find it not easy to make clear and perfect impressions of the fingers. The proper plan seems to be to cover a flat surface, like that of a piece of glass or zinc, with a thin and even coat of paint, whether it be printers' ink or Indian ink rubbed into a thick paste, and to press the finger lightly upon it so that the ridges only shall become inked, then the inked fingers are pressed on smooth and slightly damped paper. If a plate of glass be smoked over a paraffin lamp, a beautiful negative impression may be made on it by the finger, which will show well as a lantern transparency. The blackened finger may afterwards be made to leave a positive impression on a piece of paper, that requires to be varnished if it is to be rendered permanent. All this is rather dirty work, but people do not seem to object to it; rivalry and the hope of making continually better impressions carries them on. It is troublesome to make plaster casts; modelling-clay has been proposed; hard wax, such as dentists use, acts fairly well; sealing-wax is excellent if the heat can be tolerated; I have some good impressions in it. For the mere study of the marks, no plan is better than that of rubbing a little thick paste of chalk ("prepared chalk") and water or sized water upon the finger. The chalk lies in the furrows and defines them. They could then be excellently photographed on an enlarged scale. My own photographic apparatus is not at hand, or I should have experimented in this. When notes of the furrow-heads and of the initial shape of the spiral have been made, the measurements would admit of comparison with those in catalogued sets, by means of a numerical arrangement, or even by the mechanical selector described in the last article. If a cleanly and simple way could be discovered of taking durable impressions of the finger tips, there would be little doubt of its being serviceable in more than one way.

In concluding my remarks, I should say that one of the inducements to making these inquiries into personal identification has been to discover independent features suitable for hereditary investigation. It has long been my hope, though utterly without direct experimental corroboration thus far, that if a considerable number of variable and independent features could be catalogued, it might be possible to trace kinship with considerable certainty. It does not at all follow because a man inherits his main features from some one ancestor, that he may not also inherit a large number of minor and commonly overlooked features from many ancestors. Therefore it is not improbable, and worth taking pains to inquire whether each person may not carry visibly about his body undeniable evidence of his parentage and near kinships.

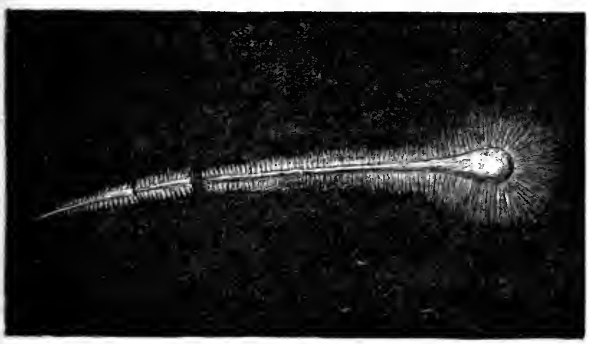
A MAGNIFICENT METEOR.

WE have received from Mr. C. Weatherall Baker (who writes from Penang) the following notes on a magnificent meteor seen from the s.s. *Prometheus* in longitude 62° E., latitude 10° 20′ N., at 10 40 p.m. on Friday, April 6, 1888:—

"It rose from the north by west horizon, and, passing behind a small cloud, travelled in a south by east direction, being at one period of its transit immediately above the ship. Sketch A represents the meteor when in that position. It traversed the whole arc of 180°, and was visible from first to last with the exception of the time when it was behind the small cloud before mentioned, the transit occupying about 30 seconds. When directly above the ship, the head appeared as near as possible the size of the moon when at its height, and the tail streamed out as in the sketch, to a length of about 15 diameters of the head. It was a brilliant white, and threw shadows on the deck as dense as those caused by the moon at the full. Sketch B represents the meteor as it appeared a few degrees above the horizon on its upward course, and on reaching the same distance above the south by east horizon it was simply a dull red ball with no tail whatever. Captain J. K. Webster, of the s.s. *Prometheus*, who has had many years' experience in most parts of the world, tells me that he has never seen a meteor in any way equalling this one for size or brilliancy."



A.—View as seen directly over ship at 10.40 p.m.



B.—View as seen shortly after appearing.

NOTES.

THE Council of the Royal Meteorological Society have issued a circular requesting that photographs of lightning may be sent to them. In response to a similar appeal last year, about sixty photographs of lightning-flashes were received from various parts of Europe and America. The Council remind photographers, amateur and professional, that the photography of lightning does not present any particular difficulties. "If a rapid plate, and an ordinary rapid lens with full aperture, be left uncovered for a short time at night during a thunderstorm, flashes of lightning will, after development, be found in some cases to have impressed themselves upon the plate. The only difficulty is the uncertainty whether any particular flash will happen to have been in the field of view. A rapid single lens is much more suitable than a rapid doublet; and it is believed that films on paper would effectually prevent reflection from the back. The focus should be that for a distant object; and, if possible, some point of landscape should be included to give the position of the horizon. If the latter is impossible, then the top of the picture should be distinctly marked. Any additional information as to the time, direction in which the camera was pointed, and the state of the weather, would be very desirable."

THE Kew Bulletin for June contains, besides an account of the manufacture of quinine in India, papers on "Job's Tears" (the round, shining fruits of a grass widely distributed in tropical countries, and used by the Karens for the decoration of clothing); on China grass or Ramie, the fibre of which, if it could be extracted and cleaned at a cheap rate, would have great economic value; and on a new botanical station at Lagos, which promises to exercise a very favourable influence on the industrial development of the West African colonies.

SOME time ago the Agassiz Association appointed a Committee to arrange for a seaside meeting during the present summer. This Committee, according to *Science*, proposes that the meeting shall be known as the "Agassiz Seaside Assembly." Its membership is to consist of such persons as shall send their names to the secretary before the opening of the assembly, or such as shall be elected members according to by-laws adopted afterwards. It is intended that the organization shall be made permanent. A six-days' session will be held this year, at Asbury Park, N.J., provided suitable accommodations can be secured at that place in the month of August. The subjects to be discussed this year will be principally botany and entomology, under the direction of such practical specialists as can be secured. The work is to include several field-day excursions with experienced guides.

THE heat in India lately has been unprecedented, in consequence of the delay of the monsoon. On June 24, when the Calcutta Correspondent of the *Times* despatched a telegram on the subject, the temperature was the highest that had ever been registered. Professional business was almost entirely suspended, and trading operations were greatly hampered. Many persons had suffered from heat-apoplexy and sunstroke, some cases having terminated fatally.

A *conversazione* was given yesterday evening by the President and Fellows of the Royal College of Physicians, at the College. The President of the Society of Telegraph-Engineers and Electricians, and Mrs. Graves, have issued invitations for a *conversazione* in the galleries of the Royal Institute of Painters in Water Colours on Tuesday, July 10.

A REMARKABLE new series of compounds of silicon tetrafluoride with organic derivatives of ammonia have been prepared by Messrs. Comey and Loring Jackson, of Harvard. Many years ago, Gay Lussac and Thénard discovered that silicon tetrafluoride formed with gaseous ammonia a singular compound, $2NH_3.SiF_4$; this substance, which is comparatively stable in air and distinctly crystalline, is decomposed by water with formation of ammonium fluoride and silicofluoride and deposition of silicic acid. The American chemists now show that a very large number of substituted ammonias form similar compounds, and give an interesting description of the methods by which they have isolated the most important members of the series. Aniline forms two such compounds, the most stable being represented by the formula $3C_6H_5NH_2 . 2SiF_4$, and the other $2C_6H_5NH_2 . SiF_4$, corresponding to the well-known compound with ammonia itself. The first was obtained by passing gaseous tetrafluoride of silicon over aniline, the gas delivery tube not quite touching the surface of the aniline so as to avoid the stoppage of the passage by the solid product. The combination is so rapid that practically all the fluoride is absorbed, considerable heat being evolved during the process; and, which is very satisfactory, the reaction is one of the few quantitative ones, the whole of the aniline being eventually converted into a loose white crystalline solid, which sublimes about 200° C. without fusion. This new substance is further remarkable by being insoluble in the usual organic solvents, alcohol alone slowly acting upon it with decomposition. Brought in contact with water it is at once decomposed with deposition of silicic acid; the solution, on evaporation, yielding beautiful pearly tabular crystals of aniline fluosilicate, aniline fluoride remaining dissolved. When aniline vapour was conducted into a receiver filled with silicon tetrafluoride the second compound was formed as a white powder, decomposing when warmed or when treated with water and even spontaneously on keeping. From the fact that the products of spontaneous decomposition are the first compound and free aniline, it is very probable that the true formula is $4C_6H_5NH_2 . 2SiF_4$, double the empirical formula; and it is evidently more than a mere coincidence that the values obtained by Mixter for the vapour density of the ammonia compound also point to the fact that its real composition is $4NH_3 . 2SiF_4$.

A SEVERE shock of earthquake was felt in the Hernö, an island in the Baltic, on June 7, at 7.24 a.m. Houses shook, and furniture moved. The shock went in a direction north-north-west. At the Lungö Lighthouse the shock was felt at 9.50, and was accompanied by a detonation like that of heavy artillery. Here the shock went in a direction north-east to south-west. The shock was also felt in the town of Hernösand.

A SPECIAL Committee, under Prof. Mushketoff, appointed to inquire into the causes of the earthquake which nearly destroyed Vyernyi, in Russian Turkistan, on June 9, 1887, has delivered its report to the Russian Geographical Society. The Committee, which consisted of four mining engineers and several topographers, began its work in August with a systematic exploration of the crevices in the buildings and the soil, both at Vyernyi and in the surrounding region as far as Lake Balkhash, Kulja, Lake Issyk-kul, and Tashkent. Detailed maps were made, and numerous photographs taken of the destroyed buildings. The chief shock of earthquake took place at 4h. 35m. a.m. on June 9; it destroyed nearly all the stone buildings of Vyernyi. It was followed at 4h. 45m. by another severe shock. Severe shocks continued for nearly half an hour, at intervals of one minute, and they were succeeded by feebler shocks which were felt throughout the day. Nearly 1500 stone houses were destroyed, while scarcely any harm was done to houses made of wood. Of a population of 30,000, no fewer than 332 persons were killed. The shocks continued almost every day throughout the months of June, July, and August; since September they have not been so frequent, but they go on still, and on March 4, 1888, there was a rather severe shock. The total number of shocks noticed (without instruments) reaches more than 200. It appears that the wave of earthquake had its origin in the south of Vyernyi, in the Alatau Mountains; and in the spur of mountains which separates the Kaskelen and the Berezovaya Rivers, the Expedition discovered at a height of from 5000 to 6000 feet a region where a dislocation of the rocks had taken place on an immense scale. The granitic and porphyritic rocks were dislocated and covered the slopes with masses of fresh *débris*. As to the softer deposits—clays and so on—which were still more softened by the very severe showers which preceded the earthquake, they were flowing and gliding like glaciers on the slopes of the mountains. One of these masses, on the Aksai River, has a volume of no less than 10,000,000 cubic metres. The centre of the earthquake was at a depth of from 5000 to 8000 metres, and its projection on the surface of the earth of the most severely affected regions covers a surface about twenty-three miles long and three miles wide on the northern slope of the Alatau. The earthquake spread with greater force towards the north than to the south; thus the region of the greatest destruction extends for about twenty-five miles northwards, and for only ten or thirteen miles southwards; but the whole region where the earthquake was felt has a length of nearly 1000 miles from south-west to north-east, and about 600 miles from south-east to north-west. As to its cause, it obviously must be searched for in the interior movements of the rocks—not in volcanic agencies. Regular seismological stations in Turkistan and the Caucasus will probably be the immediate outcome of the work of the Committee.

IN the *American Meteorological Journal* for May, Mr. Bôcher contributes an article on the labours of Dove, Redfield, and Espy, the greater part of whose work was included between the years 1830 and 1860. Redfield's first paper on the theory of storms was published in 1831, and was due to the fact of his having previously noticed, during a journey after a storm, that the trees were lying in opposite directions to those near his home. Espy supposed that the wind always blows inwards from the edge of the storm to a central point or line. He was a persistent opponent of Redfield. Dove's work on the theory of storms was essentially the same as Redfield's, but he also deals with the subject of winds in general. In a second article Mr. Rotch gives the description and history of the Sonnblick Mountain Observatory in Austria, and some of the preliminary results obtained. Mr. W. Upton contributes, on the part of the New England Meteorological Society, a very able paper on the remarkable storm which visited the eastern portion of the United States from the 11th to 14th of March last, and which is known as the New York "blizzard." Its peculiar characteristics were (1) the rapidity with which its energy was developed; (2) the excessive precipitation which accompanied it, principally as snow. West of the 72nd meridian it was almost wholly snow, piled up in immense drifts, making it absolutely impossible to measure it. East of this meridian it was snow and rain mixed. In a table giving the ratio of unmelted and melted snow it is shown that the density varied greatly, and furnishes proof that the method of assuming that 1·0 inch of snow equals 0·1 inch of rain is exceedingly erroneous. (3) The relatively small area of its maximum intensity. This storm was one of the most notable in this century over the Atlantic, and its behaviour over the ocean will be the subject of a special investigation by the United States Hydrographic Office.

THE Pilot Chart of the North Atlantic Ocean for the month of June shows that seven pronounced cyclonic storms passed over portions of the North Atlantic during May, but none appear to have traversed the entire ocean. Ice has been reported in increased quantity west of the 46th meridian, and, although confined for the most part to the coast of Newfoundland, it has been met with as far south as latitude 41°, in longitude 46° W. There has been a marked increase of fog over the Grand Banks and off the American coast north of Hatteras, while the amount encountered east of the 40th meridian has been unusually large. It is attributed almost entirely to the prevalence of southerly winds in that part of the ocean. During the past six months 51 vessels are known to have met with disaster in the North Atlantic ocean; the general drift of the logs of the great raft has been about east by south, and most of them are now about west-south-west from the Azores. Very few, if any, have drifted north of the 40th parallel. On April 10, latitude 41° 59'

N., longitude 47° 30′ W., Capt. McKay, of the s.s. *Pavonia*, saw a large waterspout travelling north-east at about 30 miles an hour. The great column of water reached up to a dense black, low-lying cloud, and was in shape like a huge hour-glass. It was accompanied by a terrific roaring. The spout broke, with a thunder and hail storm. Many pieces of ice, 4 to 6 inches in diameter, fell on board the ship. On the next day three distinct spouts were seen by another ship, about 250 miles north-east of the above position. These spouts gradually merged into one, and travelled out of sight.

FISHERMEN report that early on the morning of June 13 a waterspout was seen in the Grosses Haff, off Stettin. About 11.45 another one appeared near Dammausch. A steamer was, at the time, only 100 yards distant, and had to reverse her engines in order to escape it. Each lasted about a quarter of an hour.

SIR TERENCE O'BRIEN, Governor of Heligoland, in his report on the condition of that colony during the past year, states that at his instigation the Council of the Meteorological Department agreed to start a station there, and the Secretary, Mr. Scott, having gone over to superintend the putting up of the instruments, the observatory was established in August last, and will, he hopes, not only be of benefit to this branch of science, but will enable more accurate data than were formerly obtainable from the old and imperfect instruments at their disposal to be forthcoming in future Blue-book statistics.

IN a recording rain-gauge, recently devised by M. Brassard, the water passes from the bottom of the receiver into a centrally-pivoted trough, having each arm slightly depressed in the middle. It fills the two divisions alternately: the filled arm goes down, and empties itself into a lower trough, and the rocking thus caused is registered by an ordinary counter. Each rocking of the trough indicates one-tenth of a millimetre of water having fallen into the receiver. The instrument is designed to eliminate the error usually arising from evaporation.

ADVICES from the fishing village of Kerschkaranza, in the Kola Peninsula, on the White Sea, state that on January 5 a curious and destructive phenomenon occurred there. At 4 a.m. the inhabitants were awakened by a peculiar, dull, heavy detonation like that of distant artillery. Piled up to a height of several hundred feet, the ice—in consequence, no doubt, of the enormous pressure of the ocean ice without—was seen to begin moving from the north-west towards the shore. The gigantic ice wall moved irresistibly forward, and soon reached the shore and the village, which it completely buried, the ice extending a mile inland. The forward movement of the ice lasted four hours. No lives were lost.

ON April 29, when off the Westman Islands, Iceland, the captain of the Danish mail-steamer *Laura* threw overboard a letter written in Danish. On May 6 the letter was found in the stomach of a cod caught by a French fisherman off Reykjanæs, about 120 miles distant. The man showed it to the French Consul at Reykjavik, who submitted it to the captain of the *Laura*. It was much decomposed, but still readable.

A LANCE, an axe, a sword—all of bronze—an urn, a couple of whetstones, and some human remains have been found in a mound at Ogue, on the south-west coast of Norway.

AT the last meeting of the Asiatic Society of Japan, Dr. Knott read a biographical note on Ino Chukei, the great Japanese surveyor and cartographer. The following summary is taken from the report of the *Japan Weekly Mail*:—Ino was born in 1744, but did not begin his scientific career till he was fifty years of age. Up to that time he was a successful brewer. Towards the close of the century he went to Yedo, and there studied astronomy under the elder and younger Takahashi. The latter is the man who was put on his trial in 1830 for having exchanged maps of Yesso and Japan with Von Siebold for some books; the case, however, was never concluded, for he died in the meantime. In the year 1800, Ino began his work of surveying the coasts and islands of Japan, and for eighteen years he continued to labour at it, making in that time innumerable measurements of distance, and between 1100 and and 1200 direct measurements of latitude. The wonder is that he did so much with such rude instruments as he had, which resembled those in use in the West in the sixteenth and seventeenth centuries. The records of his survey were compiled in 1821, and were published, under the authority of the Tokio University, in book form in 1870. In fact, the charts he constructed have been the basis of all maps that have since been made. About six or seven years ago, Ino was raised by Imperial decree to the rank of "Posthumous," or Senior Fourth Class, an honour seldom held in his time by any but nobles, and, moreover, posthumous honours are very rarely given. Ino might be named the Japanese Picard, the French astronomer who made the first good calculation of the size of the earth. The instruments—an azimuth circle and a quadrant—used by Ino in his survey were destroyed by fire, but exact copies of them, constructed in 1828, were exhibited at the meeting.

ACCORDING to the report of the Inspector of Schools in Hong Kong for the past year, the total number of schools subject to Government supervision was 94, as against 45 in 1877 and 13 in 1867; the numbers of scholars for the corresponding years being respectively 5974, 3144, and 700. Of the 5974 pupils who attended schools under Government supervision in 1887, 4160 attended missionary schools, and 1814 the Government undenominational establishments. In the colony there are five classes of schools: (1) Chinese, where a purely Chinese education is given; (2) Romanized Chinese, in which a European education is given in the Chinese language; (3) Portuguese, where a European education is given in the Portuguese language only; (4) Anglo-Chinese schools, numbering eight, with 1160 scholars; (5) English schools, numbering six, with 688 scholars, in which the children are taught in the English language only. The Government Central School presented 384 boys for the annual examination, and of these 375 passed—that is, the very high percentage of 97·65. At this latter school the subjects taught are: reading, dictation, arithmetic, Chinese into English, English into Chinese, grammar, geography, map-drawing, composition, Euclid, algebra, mensuration, history, and Latin.

MESSRS. EYRE AND SPOTTISWOODE, as the Government publishers, have issued two new volumes of the "Report on the Scientific Results of the Voyage of the *Challenger*": vol. xxiv. Zoology (2 parts, text and plates), Report on the Crustacea Macrura; vol. xxv. Zoology, Report on the Tetractinellida.

A PAPER on "Wasted Sunbeams," by Dr. G. M. Smith, of New York, has just been reprinted from the *Medical Record*. The author's aim is to show that great advantages to health might be secured by a rearrangement of the upper stories of private dwellings. "Cannot architectural ingenuity," he asks, "coached by sanitary science, contrive some method of using the thousands of acres of housetops, so that roofs, now so useful in affording indoor protection from cold, sleet, and rain, can be made additionally useful, at certain seasons, by affording outdoor recreation and protection from invalidism? Cannot the same skill contrive new designs for the upper and most salutary stories of our dwellings; playing-rooms and sunning-rooms, especially adapted for the winter season, but so cleverly fashioned that too intense torrid beams can be excluded in summer?"

MR. J. ELLARD GORE has in the press a volume entitled "Planetary and Stellar Studies: papers on the Planets, Stars, and Nebulæ." It will be published shortly by Messrs. Roper and Drowley.

THE Fifteenth Annual Report of the progress of the Geological and Natural History Survey of Minnesota, by Mr. N. H. Winchell, State Geologist, has been issued. This Report relates chiefly to the geology of the iron-bearing rocks. It seems that during the last two years great interest has been manifested with regard to the iron industry in Northern Minnesota.

WE have received Part 3 of the twenty-first volume of the Journal and Proceedings of the Royal Society of New South Wales. Among the contents are papers on Port Jackson silt beds, by F. B. Gipps; some New South Wales tan-substances, parts 3 and 4, by J. H. Maiden, Curator of the Technological Museum, Sydney; soils and subsoils of Sydney and suburbs, by J. B. Henson; quarantine and small-pox, by J. Ashburton Thompson; on the presence of fusel-oil in beer, by W. M. Hamlet; autographic instruments used in the development of flying-machines, by Lawrence Hargrave.

PART I of the seventh volume of the "Encyclopædic Dictionary" (Cassell and Co.) has just been issued. This carefully-compiled work, as we have repeatedly had occasion to note, contains all the words in the English language, with a full account of their origin, meaning, pronunciation, and use. Great pains are taken to secure that scientific terms shall be properly explained.

MESSRS. OLIVER AND BOYD are about to publish "India in 1887, as seen by Robert Wallace, Professor of Agriculture and Rural Economy in the University of Edinburgh." The author was four months in India and Ceylon, and made inquiry as to the breeds of cattle and horses, and as to the condition of native agriculture, soils, irrigation, &c. The work contains 290 illustrations. Prof. Wallace especially wished to "learn in an unmistakable manner what fruits the Cirencester College training had borne."

WE have received Parts I and 2 of "The Speaking Parrots," by Dr. Karl Russ (Upcott Gill). Much useful information is given as to the purchase and reception of parrots, the cages in which they ought to be kept, their food, the best way of taming and training them, the preservation of their health, and as to their diseases.

AN Australian edition of Longmans' "School Geography," by Mr. George G. Chisholm, has just been issued. For this edition the sections on Australasia and the British Isles have been entirely re-written, and modifications have been made in other parts of the text with the view of calling attention to matters of special interest in Australia and New Zealand.

A NEW catalogue of mathematical works has been issued by Messrs. Dulau and Co.

THE current number of the *Technology Quarterly* opens with an interesting paper, by Mr. James P. Munroe, on the beginning of the Massachusetts Institute of Technology. The Institute was legally established on April 10, 1861, after more than two years of almost constant effort in the face of opposition and discouragement.

IT has been decided that the Miss Williams Scholarship for Women, of the annual value of £20, tenable for three years, shall be offered at the entrance scholarship examination at University College, Cardiff, on September 18, and that it may be held with a College exhibition. As it is specially intended to encourage the higher education of women in Wales, preference will be given to the children of Welsh parents.

A COLLECTION of American pottery for the American National Museum is about to be made by Dr. David T. Day of the United States Geological Survey. *Science* says that the collection of Sèvres pottery presented by the French Government is an exceedingly fine one, as is also that of Japanese ceramics; and the department of Indian pottery is not approached elsewhere in the world. But the Museum possesses very little modern American pottery.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. A. B. Parker; a Larger Hill-Mynah (*Gracula intermedia*) from North India, presented by Mrs. M. von Kornatzki; two Naked-footed Owlets (*Athene noctua*) from France, presented by Miss Pierce; a Swainson's Lorikeet (*Trichoglossus novæ-hollandiæ*) from Australia, presented by Mr. H. A. Hankey; two Loggerhead Ducks (*Tachyeres cinereus*) from the Falkland Islands, presented by Mr. Archibald McCall; a Duyker-bok (*Cephalophus mergens* ♀) from South Africa, a Red-legged Partridge (*Caccabis rufa*), a Barbary Partridge (*Caccabis petrosa*), five —— Pigeons (*Columba ballii*) from Teneriffe, deposited; a Bennett's Wallaby (*Halmaturus bennetti* ♀), two Long-fronted Gerbilles (*Gerbillus longifrons*) born in the Gardens; a Yellow-legged Herring Gull (*Larus cachinnans*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

ROTATION PERIOD OF THE SUN FROM FACULÆ.—The fifth part of vol. iv. of the Publications of the Astrophysical Observatory at Potsdam has recently appeared, and contains a determination by Dr. J. Wilsing of the rotation period of the sun from observations of faculæ. The previous determinations of the solar rotation have been based upon observations of the spots, or upon the relative displacement of lines in the spectra of the east and west limbs, for, as faculæ can usually only be seen well when near the limb, and therefore can seldom be watched for more than three consecutive days, and as they often undergo rapid changes, they did not seem well suited for such a discussion. Their irregular and often straggling shapes, too, render measures of their positions much less precise than those of spots. Notwithstanding these difficulties Dr. Wilsing's inquiry seems to have met with a measure of success. Of the faculæ shown on the solar photographs taken at Potsdam from 1884 March 14 to August 31, 144 groups were seen at three or more different epochs, at intervals of one or more semi-rotations. Arranging these according to their distribution in solar latitude, in zones of 3° wide, Dr. Wilsing finds practically the same rotation period for each zone from + 24° to − 33°, the difference from the mean of the daily angular motion only exceeding 2′ in a single instance, and in many cases amounting only to 20″ or 30″. As these differences are so small and follow no law, it would appear that, whilst, as Carrington and Spoerer have shown, the different spot zones have different rates of rotation, the layer of the faculæ rotates as a whole. Since the faculæ are certainly at a higher level than the spots, this conclusion is one which will fail to be accepted until we have much further and more convincing evidence than we have at present. In the present discussion it sometimes happens that a group of faculæ is considered as identical with an earlier group seen two or three semi-rotations earlier, when the same part of the sun has been seen in the interval, but without showing the group, although the district has been favourably presented for displaying faculæ. In such a case, and particularly if several semi-rotations have elapsed, the two groups will be identified or not according to the rotation period assumed; so that if a single rotation period for the whole sun be assumed in the preliminary reductions of position for the sake of identification of the groups, there will be an inevitable tendency towards a single rotation period in the final result.

The mean daily angular velocity given by the faculæ is 14° 16′ 11″·3, corresponding to a sidereal period of 25d. 5h. 28m. 12s., the values for the northern and southern hemisphere, taken separately, differing only by 11″·5. It is worthy of note that this corresponds to the rotation period of spots about latitude 10°, as given alike by Carrington and Spoerer's formulæ, and that the two zones 5° to 15° yield the greater number both of spots and faculæ which are available for these investigations. The present discussion, with whatever reserve its conclusions are to be accepted, is, however, both interesting and important and should lead to further inquiries in the same direction, when a more extended series of observations should be laid under contribution.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JULY* 1-7.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on July 1

Sun rises, 3h. 50m.; souths, 12h. 3m. 38·5s.; sets, 20h. 17m.: right asc. on meridian, 6h. 43·5m.; decl. 23° 4′ N. Sidereal Time at Sunset, 14h. 58m.

Moon (at Last Quarter July 1, 4h.) rises, 0h. 8m.; souths, 6h. 9m.; sets, 12h. 22m.: right asc. on meridian, 0h. 47·5m.; decl. 0° 16′ S.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	5 9	12 51	20 33	7 31·5	18 16 N.
Venus ...	3 33	11 51	20 9	6 31·2	23 41 N.
Mars ...	13 16	18 35	23 54	13 15·9	8 46 S.
Jupiter ...	16 35	20 59	1 23*	15 40·5	18 42 S.
Saturn ...	6 6	13 55	21 44	8 34·7	19 22 N.
Uranus ...	12 29	18 9	23 49	12 49·5	4 36 S.
Neptune..	1 32	9 18	17 4	3 57·8	18 49 N.

* Indicates that the setting is that of the following morning.

Comet Sawerthal.

July.	h.	Right Ascension. h. m.	Declination. ° ′
1	0	1 1·0	47 51 N.
5	0	1 3·6	48 44

July.	h.	
3	17	Sun at greatest distance from the Earth.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	July 5,	22 12	*m*
R Sculptoris	1 21·8	33 7 S.	,, 4,		*M*
V Tauri	4 45·6	17 21 N.	,, 2,		*M*
T Cancri	8 50·3	20 17 N.	,, 6,		*m*
R Leonis Minoris	9 38·9	35 2 N.	,, 3,		*M*
W Virginis	13 20·3	2 48 S.	,, 7,	21 0	*M*
δ Libræ	14 55·0	8 4 S.	,, 6,	1 36	*m*
U Coronæ	15 13·6	32 3 N.	,, 2,	2 18	*m*
R Ursæ Minoris	16 31·5	72 30 N.	,, 7,		*m*
U Ophiuchi	17 10·9	1 20 N.	,, 3,	23 46	*m*
U Sagittarii	18 25·3	19 12 S.	,, 3,	1 0	*M*
β Lyræ	18 46·0	33 14 N.	,, 2,	3 0	*M*
R Lyræ	18 51·9	43 48 N.	,, 3,		*M*
η Aquilæ	19 46·8	0 43 N.	,, 2,	1 0	*m*
S Sagittæ	19 50·9	16 20 N.	,, 6,	1 0	*m*
S Cygni	20 3·2	57 40 N.	,, 1,		*M*
X Cygni	20 39·0	35 11 N.	,, 6,	23 0	*m*
T Vulpeculæ	20 46·7	27 50 N.	,, 2,	1 0	*M*
			,, 3,	2 0	*m*
δ Cephei	22 25·0	57 51 N.	,, 7,	0 0	*m*
R Cassiopeiæ	23 52·7	50 46 N.	,, 7,		*M*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES.

AT Monday's meeting of the Royal Geographical Society, Lieutenant Wissmann was present, and was formally presented by the President with the gold medal which has been awarded to him by the Society for his exploring work in Africa. Lieutenant Wissmann afterwards gave some account of his explorations in the region to the south of the great Congo bend. He began his African work eight years ago in company with the late Dr. Pogge, with whom he traversed the region lying between Loanda and Nyangwe on the Upper Congo. The Kassai and several others of the great rivers that flow north to the Congo were crossed, and a large area of new country, thickly covered with an interesting population, opened up. Dr. Pogge returned to the west coast, whilst Lieutenant Wissmann proceeded from Nyangwe to Zanzibar. He returned to Africa a second time in the service of the King of the Belgians, and in company with Dr. Wolf, Lieutenant von François, and others, made his way again from Loanda into the interior. During the period between 1884 and 1887, Lieutenant Wissmann explored the Great Kassai, and did much to unravel the complicated system of rivers, of which it is the centre. Moreover, his observations on the people, as well as the fauna and flora, render his work of great scientific value. He again crossed to Nyangwe, and, by Lakes Tanganyika and Nyassa, reached the east coast at the mouth of the Zambesi. He returned to Europe in the autumn of last year, with his health shattered, and was compelled to go to Madeira to recruit. Now Lieutenant Wissmann returns to Germany, and will no doubt there work out the results of his eight years' work in Africa. Already one volume has been published, dealing with the exploration of the Kassai-Sankuni.

CAPTAIN W. J. L. WHARTON, the Hydrographer, also read a paper at Monday's meeting of the Royal Geographical Society. He described the results of a very complete examination which has recently been made of Christmas Island, in the Indian Ocean, some 200 miles south of the western end of Java. The island is a peculiar one, and extremely difficult to explore. It consists apparently of high cliffs of coral, covered with the densest vegetation. After describing the results of examination by Captain Aldrich and others, Captain Wharton concluded by giving a summary of the conclusions to be drawn. We have, he said, a high island, on the surface of which, wherever examined, we find limestone, bearing in most places the appearance of coral origin, though in some specimens the shells of the Foraminifera abound, and in none of them have direct evidences of coral structure been detected. It must be remembered, however, that coral limestone becomes so altered by the deposition of lime by infiltration, that a large surface of it may be searched before a piece retaining its coralline structure is found, and that the specimens sent home are very small. From the description of Captain Aldrich, who is well acquainted with coral formations, it may be taken for granted that the majority of this rock is of coral origin. The rock forming the summit is of this structureless character. In two spots, and at the bottom of a hole in the summit of the ridge, we have volcanic rock. The island is very steep on all sides, great depths being found close to the cliffs, while on all sides, at a short distance, soundings over three miles in depth were obtained. It appears, then, most probable that Christmas Island is founded on a volcanic mound which rose from the bottom to a certain distance from the surface of the sea; that Foraminifera shells dying on the surface were rained upon it in sufficient number to form a stratum, since solidified into limestone rock; that as the mound neared the surface, corals built upon it, and it is possible from the sketch of the island, and from Captain Aldrich's description of the slope of the ridge inwards, that it first assumed an atoll form. This, however, is a mere inference from probabilities. The island was next gradually upheaved, the coral growing outwards on the gentle slope until a period of immobility ensued long enough to permit the waves to erode the upper cliff. Another short period of upheaval, and one of stationary character ensued, when the second cliff was worn away. A third interval of upheaval, probably longer than the others, and then a second stand, when the lowest and highest inland cliff was formed. Finally, another lift was given, and the stationary period now in existence completed the process. The volcanic stones found in various places on the higher parts of the island point to a thinning of the limestone covering in those places. Denudation has worn away the limestone, and the volcanic core is consequently exposed. Man has never lived on Christmas Island, nor would it be a pleasant residence, as, apart from the fact that there is no water—the rain sinking into the limestone rock—the extreme discomfort of locomotion, and the absence of any harbour whence the produce that might possibly be raised could be conveniently shipped, will deter any settlers from seeking a home there until other more favourable spots are occupied. There is no other instance with which Captain Wharton is acquainted of an island of this height retaining its coral covering so intact. Coral reefs have been found at heights of 1000 feet in Cuba, in the Fiji Islands, and other places; but in all cases they are mere fragments, and the intervening spaces show no signs of coral. Further and closer investigation may record more direct evidence of its structure, and of the successive steps which have resulted in its present condition; but the Hydrographer thought our present knowledge of Christmas Island was sufficient to make this short notice interesting to the Society.

DIFFRACTION OF SOUND.[1]

THE interest of the subject which I propose to bring before you this evening turns principally upon the connection or analogy between light and sound. It has been known for a very long time that sound is a vibration; and everyone here knows that light is a vibration also. The last piece of knowledge, however, was not arrived at so easily as the first; and one of the difficulties which retarded the acceptance of the view that light is a vibration was that in some respects the analogy between light and sound seemed to be less perfect than it should be. At the present time many of the students at our schools and universities can tell glibly all about it; yet this difficulty is one not to be despised, for it exercised a determining influence over the great mind of Newton. Newton, it would seem, definitely rejected the wave-theory of light on the ground that according to such a theory light would turn round the corners of obstacles, and so abolish shadows, in the way that sound is generally supposed to do. The fact that this difficulty seemed to Newton to be insuperable is, from the point of view of the advancement of science, very encouraging. The difficulty which stopped Newton two centuries ago is no difficulty now. It is well known that the question depends upon the relative wave-lengths in the two cases. Light-shadows are sharp under ordinary circumstances, because the wave-length of light is so small; sound-shadows are usually of a diffused character, because the wave-length of sound is so great. The gap between the two is enormous. I need hardly remind you that the wave-length of C in the middle of the musical scale is about 4 feet. The wave-length of the light with which we are usually concerned, the light towards the middle of the spectrum, is about the forty-thousandth of an inch. The result is that an obstacle which is immensely large for light may be very small for sound, and will therefore behave in a different manner.

That light-shadows are sharp is a familiar fact, but as I can prove it in a moment I will do so. We have here light from the electric arc thrown on the screen; and if I hold up my hand thus we have a sharp shadow at any moderate distance, which shadow can be made sharper still by diminishing the source of light. Sound-shadows, as I have said, are not often sharp; but I believe that they are sharper than is usually supposed, the reason being that when we pass into a sound-shadow—when, for example, we pass into the shade of a large obstacle, such as a building—it requires some little time to effect the transition, and the consequence is that we cannot make a very ready comparison between the intensity of the sound before we enter and its diminution afterwards. When the comparison is made under more favourable conditions, the result is often better than would have been expected. It is, of course, impossible to perform experiments with such obstacles before an audience, and the shadows which I propose to show you to-night are on a much smaller scale. I shall take advantage of the sensitiveness of a flame such as Professor Tyndall has often used here—a flame sensitive to the waves produced by notes so exceedingly high as to be inaudible to the human ear. In fact, all the sounds with which I shall deal to-night will be inaudible to the audience. I hope that no quibbler will object that they are therefore not sounds: they are in every respect analogous to the vibrations which produce the ordinary sensations of hearing.

I will now start the sensitive flame. We must adjust it to a reasonable degree of sensitiveness. I need scarcely explain the mechanism of these flames, which you know are fed from a special gas-holder supplying gas at a high pressure. When the pressure is too high, the flame flares on its own account (as this one is doing now), independently of external sound. When the pressure is somewhat diminished, but not too much so—when the flame "stands on the brink of the precipice" were, I think, Tyndall's words—the sound pushes it over, and causes it to flare; whereas, in the absence of such sound, it would remain erect and unaffected. Now, I believe, the flame is flaring under the action of a very high note that I am producing here. That can be tested in a moment by stopping the sound, and seeing whether the flame recovers or not. It recovers now. What I want to show you, however, is that the sound-shadows may be very sharp. I will put my hand between the flame and the source of sound, and you will see the difference. The flame is at present flaring; if I put my hand here, the flame recovers. When the adjustment is correct, my hand is a sufficient obstacle to throw a most conspicuous shadow. The flame is now in the shadow of my hand, and it recovers its steadiness: I move my hand up, the sound comes to the flame again, and it flares. When the conditions are at their best, a very small obstacle is sufficient to make the entire difference, and a sound-shadow may be thrown across several feet from an obstacle as small as the hand. The reason of the divergence from ordinary experience here met with is, that while the hand is a fairly large obstacle in comparison with the wave-length of the sound I am here using, it would not be a sufficiently large obstacle in comparison with the wave-lengths with which we have to do in ordinary life and in music.

Everything then turns upon the question of the wave-length. The wave-length of the sound that I am using now is about half an inch. That is its complete length, and it corresponds to a note that would be very high indeed on the musical scale. The wave-length of middle C being four feet, the C one octave above that is two feet; two octaves above, one foot; three octaves above, six inches; four octaves, three inches; five octaves, one and a half inch; six octaves, three-quarters of an inch; between that and the next octave, that is to say, between six and seven octaves above middle C, is the pitch of the note that I was just now using. There is no difficulty in determining what the wave-length is. The method depends upon the properties of what are known as stationary sonorous waves as opposed to progressive waves. If a train of progressive waves are caused to impinge upon a reflecting wall, there will be sent back or reflected in the reverse direction a second set of waves, and the co-operation of these two sets of waves produces one set or system of stationary waves; the distinction being that, whereas in the one set the places of greatest condensation are continually changing and passing through every point, in the stationary waves there are definite points for the places of greatest condensation (nodes), and others distinct and definite (loops) for the places of greatest motion. The places of greatest variation of density are the places of no motion: the places of greatest motion are places of no variation of density. By the operation of a reflector, such as this board, we obtain a system of stationary waves, in which the nodes and loops occupy given positions relatively to the board.

You will observe that as I hold the board at different distances behind, the flame rises and falls—I can hardly hold it still enough. In one position the flame rises, further off it falls again; and as I move the board back the flame passes continually from the position of the node—the place of no motion—to the loop or place of greatest motion and no variation of pressure. As I move back, the aspect of the flame changes; and all these changes are due to the reflection of the sound-waves by the reflector which I am holding. The flame alternately ducks and rises, its behaviour depending npon the different action of the nodes and loops. The nodes occur at distances from the reflecting wall, which are even multiples of the quarter of a wave-length; the loops are, on the other hand, at distances from the reflector which are odd multiples, bisecting therefore the positions between the loops. I will now show you that a very slight body is capable of acting as a reflector. This is a screen of tissue-paper, and the effect will be apparent when it is held behind the flame and the distances are caused to vary. The flame goes up and down, showing that a considerable proportion of the sonorous intensity incident upon the paper screen is reflected back upon the flame; otherwise the exact position of the reflector would be of no moment. I have here, however, a different sort of reflector. This is a glass plate —I use glass so that those behind may see through it—and it will slide upon a stand here arranged for it. When put in this position the flame is very little affected: the place is what I call a node—a place where there is great pressure variation, but no vibratory velocity. If I move the glass back, the flame becomes vigorously excited: that position is a loop. Move it back still more, and the flame becomes fairly quiet; but you see that as the plate travels gradually along, the flame goes through these evolutions as it occupies in succession the position of a node or the position of a loop. The interest of this experiment for our present purpose depends upon this—that the distances through which the glass plate, acting as a reflector, must be successively moved in order to pass the flame from a loop to the next loop, or from a node to the consecutive node, is in each case half the wave-length; so that by measuring the space through which the plate is thus withdrawn one has at once a measurement of the wave-length, and consequently of the pitch of the sound, though one cannot hear it.

[1] Lecture delivered by Lord Rayleigh, F.R.S., at the Royal Institution, on January 20, 1888.

The question of whether the flame is excited at the nodes or at the loops—whether at the places where the pressure varies most, or at those where there is no variation of pressure, but considerable motion of air—is one of considerable interest from the point of view of the theory of these flames. The experiment could be made well enough with such a source of sound as I am now using; but it is made rather better by using sounds of a lower pitch, and therefore of greater wave-length, the discrimination being then more easy. Here is a table of the distances which the screen must be from the flame in order to give the maximum and the minimum effect, the minimum being practically nothing at all.

Table of Maxima and Minima.

Max.	Min.
1·1	
	3·0
4·5	
	5·9
7·5	
	8·9
10·3	
	11·7
13·0	
	14·7
15·9	

The distance between successive maxima or successive minima is very nearly 3 cm., and this is accordingly half the length of the wave.

But there is a further question behind. Is it at the loops or is it at the nodes that the flame is most excited? The table shows what the answer must be, because the nodes occur at distances from the screen which are even multiples, and the loops at distances which are odd multiples; and the numbers in the table can be explained in only one way—that the flame is excited at the loops corresponding to the odd multiples, and remains quiescent at the nodes corresponding to the even multiples. This result is especially remarkable, because the ear, when substituted for the flame, behaves in the exactly opposite manner, being excited at the nodes and not at the loops. The experiment may be tried with the aid of a tube, one end of which is placed in the ear, while the other is held close to the burner. It is then found that the ear is excited the most when the flame is excited least, and *vice versâ*. The result of the experiment shows, moreover, that the manner in which the flame is disintegrated under the action of sound is not, as might be expected, symmetrical in regard to the axis of the flame. If it were symmetrical, it would be most affected by the symmetrical cause—namely, the variation of pressure. The fact being that it is most excited at the loop, where there is the greatest vibratory velocity, shows that the method of disintegration is unsymmetrical, the velocity being a directed quantity. In that respect the theory of these flames is different from the theory of the water-jets investigated by Savart, which resolve themselves into detached drops under the influence of sonorous vibration. The analogy fails at this point, and it has been pressed too far by some experimenters on the subject. Another simple proof of the correctness of the result of our experiment is that it makes all the difference which way the burner is turned in respect of the direction in which the sound-waves are impinging upon it. If the phenomenon were symmetrical, it would make no difference if the flame were turned round upon its vertical axis. But we find that it does make a difference. This is the way in which I was using the flame, and you see that it is flaring strongly. If I now turn the burner round through a right angle, the flame stops flaring. I have done nothing more than turn the burner round and the flame with it, showing that the sound-waves may impinge in one direction with great effect, and in another direction with no effect. The sensitiveness occurs again when the burner is turned through another right angle; after three right angles there is another place of no effect; and after a complete revolution of the flame the original sensitiveness recurs. So that if the flame were stationary, and the sound-waves, came, say, from the north or south, the phenomena would be exhibited; but if they came from the east or west, the flame would make no response.

This is of convenience in experimenting, because, by turning the burner round, I make the flame almost insensitive to a sound, and I am now free to show the effect of any sound that may be brought to it in the perpendicular direction. I am going to use a very small reflector—a small piece of looking-glass. Wood would do as well; but looking-glass facilitates the adjustment, because my assistant, by seeing the reflection, will be able to tell me when I am holding it in the best position. Now, the sound is being reflected from the bit of glass, and is causing the flame to flare, though the same sound, travelling a shorter distance and impinging in another direction, is incompetent to produce the result (Fig. 1).

I am now going to move the reflector to and fro along the line perpendicular to that joining the source and the burner, all the while maintaining the adjustment, so that from the position of the source of sound the image of the flame is seen in the centre of the mirror. Seen from the source, it is still as central as before, but it has lost its effect, and as I move it to and fro I produce cycles of effect and no effect. What is the cause of this? The question depends upon something different from what I have been speaking of hitherto; and the explanation is, that we are here dealing with a diffraction phenomenon. The mirror is a small one, and the sound-waves which it reflects are not big enough to act in the normal manner. We are really dealing with the same sort of phenomena as arise in optics when we use small pin-holes for the entrance of our light. It is not very easy to make the experiment in the present form quite simple, because the mirror would have to be withdrawn, all the while maintaining a somewhat complicated adjustment. In order to raise the question of diffraction in its simplest shape, we must have a direct course for the sound between its origin and the place of observation, and interpose in the path a screen perforated with such holes as we desire to try.

The screen I propose to use is of glass. It is a practically perfect obstacle for such sounds as we are dealing with; but it is perforated here with a hole (20 cm. diameter), rendered more evident to those at a distance by means of a circle of paper pasted round it. The edge of the hole corresponds to the inner circumference of the paper. We shall thus be able to try the effect of different-sized apertures, all the other circumstances remaining unchanged. The experiment is rather a difficult one before an audience, because everything turns on getting the exact adjustment of distances relatively to the wave-length. At present the sound is passing through this comparatively large hole in the glass screen, and is producing, as you see, scarcely any effect upon the flame situated opposite to its centre. But if (Fig. 2) I diminish the size of the hole by holding this circle of zinc (perforated with a hole 14 cm. in diameter) in front of it, it is seen that, although the hole is smaller, we get a far greater effect. That is a fundamental phenomenon in diffraction. Now I reopen the larger hole, and the flame becomes quiet. So that it is evident that in this case the sound produces a greater effect in passing through a small hole than in passing through a larger one. The experiment may be made in another way, by obstructing the central in place of the marginal part of the aperture in the glass. When I hold this unperforated disk of zinc (14 cm. in diameter) centrically in front, we get a greater effect than when the sound is allowed to pass through both parts of the aperture. The flame is now flaring vigorously under the action of the sonorous waves passing the marginal part of the aperture, whereas it will scarcely flare at all under the action of waves passing through both the marginal and the central hole.

This is a point which I should like to dwell upon a little, for it lies at the root of the whole matter. The principle upon which it depends is one that was first formulated by Huygens, one of the leading names in the development of the undulatory theory of light. In this diagram (Fig. 3) is represented in section the different parts of the obstacle. C represents the source of sound, B represents the flame, and A P Q is the screen. If we choose a point, P, on this screen, so that the whole distance from B to C, reckoned through P, viz. B P C, exceeds the shortest distance B A C by exactly half the wave-length of the sound, then the circular area, whose radius is A P, is the first zone. We take next another point, Q, so that the whole distance B Q C exceeds the previous one by half a wave-length. Thus we get the second zone represented by P Q. In like manner, by taking different points in succession such that the last distance taken exceeds the previous one every time by half a wave-length, we may map out the whole of the obstructing screen into a series of zones called Huygens' zones. I have here a material embodiment of that notion, in which the zones are actually cut out of a piece of zinc. It is easy to prove that the effects of the parts of the wave traversing the alternate zones are opposed; that whatever may be the effect of the first zone, A P, the exact opposite will be the effect of P Q, and so on. Thus, if A P and P Q are both allowed to operate, while all beyond Q is cut off, the waves will neutralize one another, and the effect will be immensely less than if A P or P Q operated alone. And that is what we saw just now. When I used the inner aperture only, a comparatively loud sound acted upon the flame. When I added to that inner aperture the additional aperture P Q, the sound disappeared, showing that the effect of the latter was equal and opposite to that of A P, and that the two neutralized each other.

[If A C = a, A B = b, A R = x, wave-length = λ, the value of x for the external radius of the nth zone is

$$x^2 = n\lambda \frac{a + b}{ab};$$

or, if $a = b$,

$$x^2 = \tfrac{1}{2}n\lambda a.$$

With the apertures used above, $x^2 = 49$ for $n = 1$; $x^2 = 100$ for $n = 2$; so that

$$\lambda a = 100,$$

the measurements being in centimetres. This gives the suitable distances, when λ is known. In the present case $\lambda = 1·2$, $a = 83$.]

Closely connected with this there is another very interesting experiment, which can easily be tried, and which has also an important optical analogy. I mean the experiment of the shadow thrown by a circular disk. If a very small source of light be taken—such a source as would be produced by perforating a thin plate in the shutter of the window of a dark room with a pin, and causing the rays of the sun to enter horizontally—and if we interpose in the path of the light a small circular obstacle, and then observe the shadow thrown in the rear of that obstacle, a very remarkable peculiarity manifests itself. It is found that in the centre of the shadow of the obstacle, where the darkness might

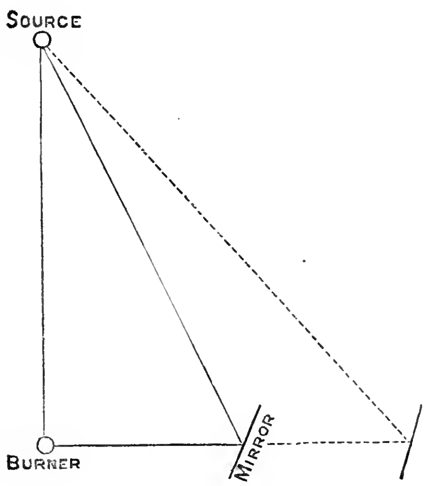


FIG. 1.

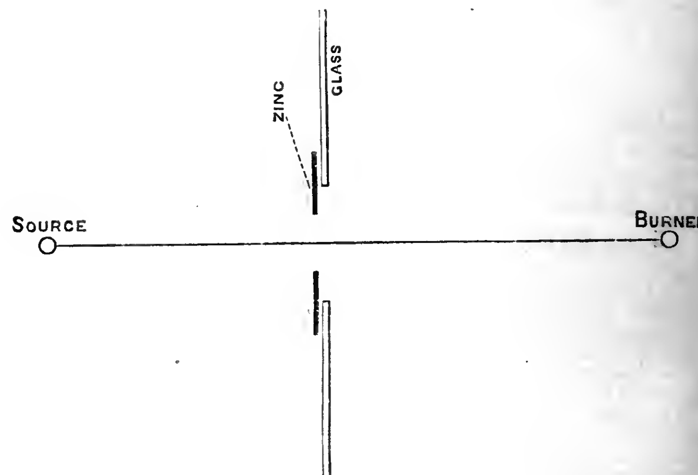


FIG. 2.

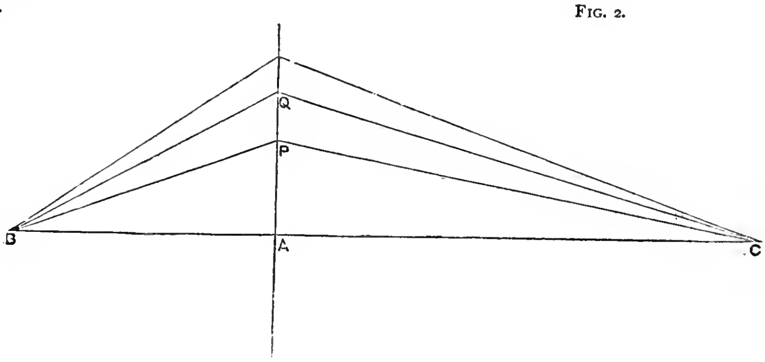


FIG. 3.

be expected to be greatest, there is, on the contrary, no darkness at all, but a bright spot, a spot as bright as if no obstacle intervened in the course of the light. The history of this subject is curious. The fact was first observed by Delisle in the early part of the eighteenth century, but the observation fell into oblivion. When Fresnel began his important investigations, his memoir on diffraction was communicated to the French Academy, and was reported on by the great mathematician Poisson. Poisson was not favourably impressed by Fresnel's theoretical views. Like most mathematicians of the day, he did not take kindly to the wave theory; and in his report on Fresnel's memoir, he made the objection that if the method were applied, as Fresnel had not then done, to investigate what should happen in the shadow of a circular obstacle, it brought out this paradoxical result, that in the centre there would be a bright point. This was regarded as a *reductio ad absurdum* of the theory. All the time, as I have mentioned, the record of Delisle's observations was in existence. The remarks of Poisson were brought to the notice of Fresnel, the experiment was tried, and the bright point was rediscovered, to the gratification of Fresnel and the confirmation of his theoretical views. I don't propose to attempt the optical experiment now, but it can easily be tried in one's own laboratory. A long room or passage must be darkened: a fourpenny bit may be used as the obstacle, strung up by three hairs attached by sealing-wax. When the shadow of the obstacle is received on a piece of ground glass, and examined from behind with a magnifying lens, the bright spot will be seen without much difficulty. But what I propose to show you is the corresponding phenomenon in the case of sound. Fresnel's reasoning is applicable, word for word, to the phenomena we are considering just as much as to that which he, or rather Poisson, had in view. The disk (Fig. 4), which I shall hang up now between the source of sound and the flame, is of glass. It is about 15 inches in diameter. I believe the flame is flaring now from being in the bright spot. If I make a small motion of the disk, I shall move the bright spot and the effect will disappear. I am pushing the disk away now, and the flaring has stopped. The flame is still in the shadow of the disk, but not at the centre. I bring the disk back again, and when the flame comes into the centre it flares again vigorously. That is the phenomenon which was discovered by Delisle and confirmed by Arago and Fresnel, but mathematically it was suggested by Poisson.

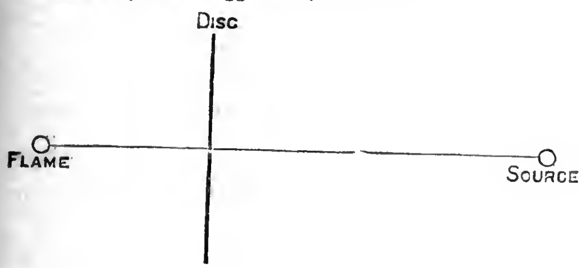


FIG. 4.

Poisson's calculation related only to the very central point in the axis of the disk. More recently the theory of this experiment has been very thoroughly examined by a German mathematician, Lommel; and I have exhibited here one of the curves given by him embodying the results of his calculations on the subject (Fig. 5).

The abscissæ, measured horizontally, represent distances drawn outwards from the centre of the shadow O; the ordinates measure the intensity of the light at the various points. The maximum intensity OA is at the centre. A little way outwards, at B, the intensity falls almost, but not quite, to zero. At C there is a revival of intensity, indicating a bright ring; and further out there is a succession of subordinate fluctuations. The curve on

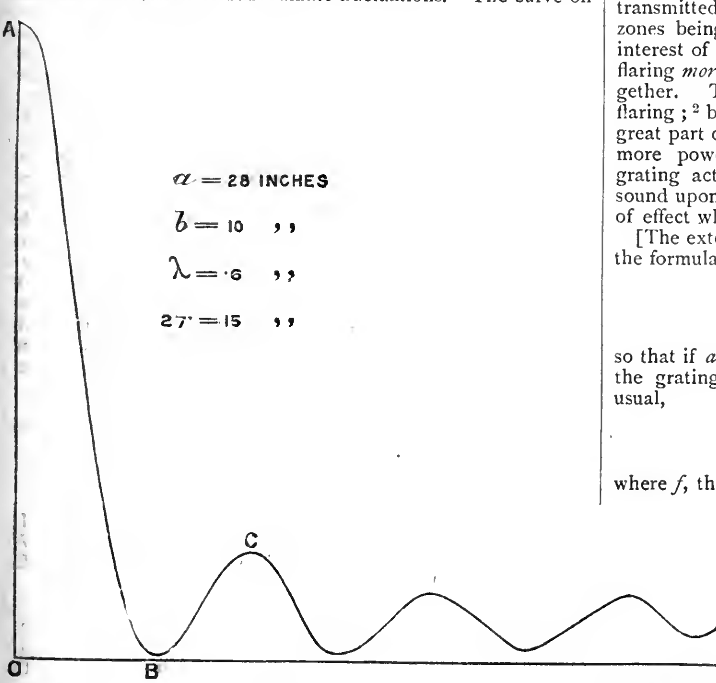


FIG. 5.

the other side of OA would of course be similar. This curve corresponds to the distances and proportions indicated. a is the distance between the source of sound and the disk; b is the distance between the disk and the flame, the place where the intensity is observed. The numbers given are taken from the notes of an experiment which went well. If we can get our flame to the right point of sensitiveness, we may succeed in bringing into view not only the central spot, but the revived sound which occurs after you have got away from the central point and have passed through the ring of silence. There is the loud central point. If I push the disk a little, we enter the ring of silence, B;[1] a little further, and the flame flares again, being now at C.

Although we have thus imitated the optical experiment, I must not leave you under the idea that we are working under the same conditions that prevail in optics. You see the diameter of my disk is 15 inches, and the length of my sound-wave is about half an inch. My disk is therefore about thirty wave-lengths in diameter, whereas the diameter of a disk representing thirty wave-lengths of light would be only about $\frac{1}{1000}$ inch. Still, the conditions are sufficiently alike to get corresponding effects, and to obtain this bright point in the centre of the shadow conspicuously developed.

I will now make an experiment illustrating still further the principle of Huygens' zones, which I have already roughly sketched. I indicated that the effect of contiguous zones was equal and opposite, so that the effect of each of the odd zones is one thing, and of the even zones the opposite thing. If we can succeed in so preparing a screen as to fit the system of zones, allowing the one set to pass, and at the same time intercepting the other set, then we shall get a great effect at the central point, because we shall have removed those parts which, if they remained, would have neutralized the remaining parts. Such a system has been cut out of zinc, and is now hanging before you. When the adjustments are correct, there will be produced, under the action of that circular grating, an effect much greater than would result if the sound-waves were allowed to pass on without any obstruction. The only point difficult of explanation is as to what happens when the system of zones is complete, and extends to infinity, viz. when there is no obstruction at all. In that case it may be proved that the aggregate effect of all the zones is, in ordinary cases, half the effect that would be produced by any one zone alone, whereas if we succeed in stopping out a number of the alternate zones, we may expect a large multiple of the effect of one zone. The grating is now in the right position, and you see the flame flaring strongly, under the action of the sound-waves transmitted through these alternate zones, the action of the other zones being stopped by the interposition of the zinc. But the interest of the experiment is principally in this, that the flame is flaring *more* than it would do if the grating were removed altogether. There is now, without the grating, a very trivial flaring;[2] but when the grating is in position again—though a great part of the sound is thereby stopped out—the effect is far more powerful than when no obstruction intervened. The grating acts, in fact, the part of a lens. It concentrates the sound upon the flame, and so produces the intense magnification of effect which we have seen.

[The exterior radius of the nth zone being x, we have, from the formula given above—

$$\frac{1}{a} + \frac{1}{b} = \frac{n\lambda}{x^2};$$

so that if a and b be the distances of the source and image from the grating, the relation required to maintain the focus is, as usual,

$$\frac{1}{a} + \frac{1}{b} = \frac{1}{f},$$

where f, the *focal length*, is given by—

$$f = \frac{x^2}{n\lambda}.$$

In the actual grating, eight zones (the first, third, fifth, &c.) are occupied by metal. The radius of the first zone, or central circle, is 3 inches, so that $x^2/n = 9$. The focal length is necessarily a function of λ. In the present case $\lambda = \frac{1}{2}$ inch nearly, and therefore $f = 18$ inches. If a and b are the same, each must be made equal to 36 inches.]

SCIENTIFIC SERIALS.

Revue d'Anthropologie, troisième série, tome iii., 1888 (Paris).—Stratigraphic palæontology in relation to man, by M. Marcellin Boule. Rejecting as unauthenticated all evidence of human existence in the Tertiary age, the author considers the

[1] With the data given above the diameter of the silent ring is two-thirds of an inch.

[2] Under the best conditions the flame is absolutely unaffected.

grounds on which we may assume that the so-called Saint Acheul flint instruments, found in alluvial beds of undoubted Quaternary origin, supply the most ancient testimony of man's presence on the surface of the earth. While attaching great importance to the careful elucidation of the chronological order in which the oldest traces of man appear relatively to the different series of the Quaternary formations, he points out the imminent risk of losing the few opportunities which still remain of studying this connection between the objects found and the nature and order of sequence of the beds in which they were deposited, owing to the most interesting finds having long been made to swell the collections of our Museums without reference to their value as exponents of the problems of our primitive history. M. Marcellin Boule considers that palæologists have erred in assuming that all beds containing the same fossil remains must necessarily belong to the same epoch, and that sufficient importance has not been attached to the fact that the same deposit often contains a mixture of animal forms belonging both to so-called northern and southern types. In explanation of these and many other anomalous phenomena, he thinks we may derive important help from a careful consideration of the intermittence and recurrence of glacial action. In regard to this point he recognizes the great value of the labours of British and American as well as Scandinavian and German geologists when compared with those of the majority of their French *confrères*; and, following the lead of our own palæontologists, he refuses to believe that any traces of human existence can be referred to pre-glacial ages, although some may perhaps be assigned to inter-glacial periods; while he considers that in certain northern lands, as Denmark and Southern Sweden, where there is a complete absence of Palæolithic objects, their non-appearance may be explained by the ice-covering not having been entirely removed in these regions till the dawn of the age of polished stone.—The tibia in the Neanderthal race, by Prof. Julien Fraipont. As a further exposition of the views which the author, in concert with M. Lohest, had expressed in regard to the effect on the maintenance of the vertical position of the obliquity and curvature of the femur in the "men of Spy," he now attempts to show, from the observations of others, and his own anatomical experiments, that in this inclination of the head of the femur we have a characteristic common to the anthropoids. An ingeniously devised series of determinations of the variations of the axis of the head of the tibia in recent man, the men of Spy, the gorilla, and other anthropoids, shows the gradual straightening of the axis as we ascend from the latter to existing man, in whom there is a well-marked tendency to the fusion of the axis of the head of the tibia with that of the body. From a careful comparison of the gradual anatomical changes presented in man since his earliest representative appeared in the Quaternary age, M. Fraipont believes we are justified in assuming that the human race has progressively acquired a more and more vertical posture.—On the population of the ancient Pagus-Çap-Sizun, "Cape du Raz," by MM. Le Carguet and P. Topinard. In considering the map of France from an ethnographic point of view, French anthropologists are generally agreed in regarding as specially Celtic the region which includes Brittany, Auvergne, and the entire mass of mountains extending through Central France and Savoy. The population of the eastern portion of this region is more brachycephalic than that of the western, which has been largely affected by admixture with the blonde, tall, dolichocephalic races whose presence is traceable everywhere in Europe, although more definitely the further north we go. This admixture of types is most strongly marked in Brittany, where French is the spoken tongue in Haute-Bretagne, and Breton (apparently a dialect derived from an ancient Kymric language) the predominant tongue in Basse-Bretagne. Among the many interesting localities of the latter region, special attention is due to Pointe du Raz, which, from the nature of its rocky boundaries on the land side, and its position further west than any other in France, has been virtually cut off from communication with the rest of the country, in consequence of which its population presents relatively fewer marks of mixed origin. M. Topinard supplies an interesting report on the geological, historical, and ethnological characteristics of the Cape du Raz district, and thus enhances the value of the series of observations regarding the population of this far west of France which have been supplied by M. Le Carguet, and may be generally summarized as leading to the inference that the "Capiste" race is essentially Breton in regard to the predominance of blue eyes with dark hair, and their generally low stature, these characteristics being associated with a disposition in which courage and energy are blended with strongly marked avarice and a love of greed; while in other respects they show evidence of a strongly-marked Celtic type.—Heredity in political economy, by M. de Lapouge. In this sequel to his former articles on "Inequality among Men," the author urges that it is the duty of the State to use all means at its disposal to eliminate the degenerate, and multiply the favoured elements of which the community is composed. As an ultra-pessimist in regard to the advance of inferior races through civilization, he points to the small effect which thousands of years have effected in the natives of the Black Continent. To him, equality and fraternity are mere delusive terms, based on an insufficient estimate of the force of the immutable laws of Nature, from amongst which we cannot exclude natural selection and survival of the fittest. As the avowed opponent of the doctrine of the amalgamation of types, and the production of permanent hybrids, he proclaims it as his opinion that, if the higher races are not to be exterminated by the lower, they must ally themselves solely with their own dolichocephalic, blonde, Aryan kindred. In treating of the question of selection he passes in review the bearing that religious opinions have had among different races in determining various degrees of consanguinity which were to be recognized as natural barriers against intermarriage among relatives. Considered generally, M. de Lapouge's article is a protest against futile attempts in the assumed name of philanthrophy to raise inferior types at the expense of those whom history from its earliest dawn has shown us to have been the leaders and pioneers in every path of human progress.—In a note on the recurrence among the Provençals of the present day of the myth of Ibicus, M. le Dr. Bérenger-Férand draws attention to the numerous characteristics derived from Hellenic antiquity which are still to be met with on the site of ancient Greek settlements. The modern tale of the detection of a murder through a reference by the murderers themselves to the birds which had been near the spot where the deed was done, is current both at Toulon and La Grasse. Both versions agree closely with the Greek original as to the course of the events, although the myth had been accepted as a true account of a contemporaneous occurrence in the latter place many years before it received its modern names of persons and places from the Toulonais.—In a note on the history of anthropology in 1788, M. Topinard has collected together various interesting details as to the precise meaning attached at that and earlier periods of the last century to the terms anthropology and ethnology. A doubt exists, however, as to the latter term, which is generally believed to have originated in its present sense with W. Edwards, when in 1839 he founded his so-called Ethnological Society. Dr. Topinard derives many curious facts from the manuscript work of Chavannes, Professor of Theology at Lausanne, whose speculative views as given in his writings he believes to have largely influenced the Encyclopædists no less than the author of "Emile."—Report by Dr. P. Topinard on the Neolithic skull, found at Feigneux (Oise) in 1887, which presents undoubted traces of having been trepanned both during life and after death.

SOCIETIES AND ACADEMIES.

London.

Royal Society, May 31.—"Colour Photometry. Part II. The Measurement of Reflected Colours." By Captain W. de W. Abney, R.E., F.R.S., and Major-General Festing, R.E., F.R.S.

In a previous paper we showed how the luminosity of different spectrum colours might be measured, and in the present paper we give a method of measuring the light of the spectrum reflected from coloured bodies such as pigments in terms of the light of the spectrum reflected from a white surface. To effect this the first named of us devised a modification of our previous apparatus. Nearly in contact with the collimating lens was placed a double image prism of Iceland spar, by which means two spectra were thrown on the focussing screen of the camera (which was arranged as described in the Bakerian lecture for 1886), each formed of the light which enters the slit. The light was thus identical in both spectra. The two spectra were separated by about $\frac{1}{8}$ of an inch when the adjustments were complete. A slit cut in a card was passed through this spectrum to isolate any particular portion which might be required. The rays coming from the uppermost spectrum were reflected by means of a small right-angled prism in a direction nearly at right angles to the original direction on to another right-angled prism. Both prisms were attached to the card. From this last prism the rays fell on a lens, and formed on a white screen an image of the face of the spectroscope prism in monochromatic light. The ray of the same wave-length as that reflected from the upper spectrum passed through the lower half of the slit, and falling on another lens formed another image of the face of the prism, superposed over the first image. A rod placed in front of the screen thus cast two shadows, one illuminated by monochromatic rays from the top spectrum, and the other by those from the bottom spectrum. The illumination of the two shadows was equalized by means of rotating sectors which could be closed and opened at pleasure during the time of rotation. The angle to which the sector required to be opened to establish equality of illumination of the two shadows gave the ratio of the brightness of the two spectra. When proper adjustment had been made, the relative brightness was the same throughout the entire spectrum.

To measure the intensity of any ray reflected from a pigment, a paper was coated with it and placed adjacent to a white surface, and it was so arranged that one shadow of the rod fell on the coloured surface and the other on the white surface. The illuminations were then equalized by the sectors, and the relative intensities of the two reflected rays calculated. This was repeated throughout the spectrum. Vermilion, emerald-green, French ultramarine were first measured by the above method, and then sectors of these colours prepared, which when rotated gave a gray matching a gray obtained by rotation of black and white. The luminosity curves of these three colours were then calculated and reduced proportionally to the angle that each sector occupied in the disk. The luminosity curve of the white was then reduced in a similar manner, and it was found that the sum of the luminosities of the three colours almost exactly equalled that of the white. The same measurements were gone through with pale-yellow chrome and a French blue, which formed a gray on rotation, with like results. It was further found that *the sum of the intensities* of vermilion, blue, and green varied at different parts of the spectrum, and the line joining them was not parallel to the straight line which represented white for all colours of the spectrum and which itself was parallel to the base. Since a straight line parallel to the base indicated degraded white, it followed that if the intensity of the rays of the spectrum were reduced proportionally to the height of the ordinates above a line tangential to the curved line (which represented the sum of the intensities of the three colours at the different parts of the spectrum) and were recombined, a gray should result. A method was devised of trying this, and the experiment proved that such was the case. The same plan enabled the colour of any pigment to be reproduced from the spectrum on the screen. The combination of colours to form a gray on rotation by a colour-blind person was also tried, and after the curve of luminosity of the colours had been calculated and reduced according to the amount required in the disk, it was found that the sum of the areas of the curves was approximately equal to the white necessary to be added to a black disk to form a gray of equal intensity as perceived by him. The spectrum intensity of gas-light in comparison with the electric light was also measured, and the amount of the different colours necessary to form a gray in this light was ascertained by experiment.

As before, it was found that the calculated luminosity of the colours was equal to the white which, combined with black, formed a gray of equal luminosity.

The question of the coloured light reflected from different metals was next considered, and the method of measuring it devised, as was also the method of measuring absorption spectra. The luminosity curves obtained by the old method were compared with those obtained by the present method, and so close an agreement between them was found to exist as to give a further confirmation that our former plan was accurate. A number of pigments that can be used for forming grays by rotation were measured, and the results tabulated in percentages of the spectrum of white light and on a wave-length scale.

Physical Society, June 9.—Prof. Reinold, President, in the chair.—The following papers were read:—On the analogy between dilute solutions and gases as regards Gay-Lussac's and Boyle's and Avogadro's laws, by Prof. van 't Hoff, presented by Prof. Ramsay, F.R.S. If a dilute aqueous solution of sugar (say 1 per cent.) be placed in a vessel, A (the walls of which are permeable to water, but not to sugar molecules), and immersed in a large quantity of water, B, water will pass from B to A until a certain difference of pressure exists between the inside and outside of A, that difference depending on the temperature and concentration of the solution. The pressure is called *osmotic pressure*, and the walls of A are said to be *semi-permeable*. Such a vessel may be artificially produced by depositing ferrocyanide of copper on unglazed porcelain; but many of the experiments dealt with in the paper have been made with the cells of plants, the walls of which form good *semi-permeable membranes*. At constant temperature the osmotic pressure is found to be proportional to the concentration of the solution, and for a given concentration the pressure is proportional to the absolute temperature. Similar results have been obtained with solutions of KNO_3, K_2SO_4, NaCl, &c., and Soret has found that if a solution be heated unequally at different parts, the warmer parts are less concentrated, just as in gases under similar conditions the warmer parts are more rarefied. The numerical results are in fair accordance with those deduced from the laws above stated. Theoretical proofs of the laws are given, in which reversible cycles and the second law of thermodynamics are made use of. By similar reasoning the author concludes that "under equal osmotic pressure, and at the same temperature, equal volumes of all solutions contain the same number of molecules, and moreover the same number of molecules which would be contained in a gas under the same conditions of temperature and pressure." These results are confirmed by Pfeffer's direct determinations of osmotic pressure, and Raoult's experiments on the "molecular lowering of vapour-pressure," and the "molecular depression of the freezing-point of the solvent." The latter part of the paper contains applications to chemical phenomena. Prof. Rücker regretted that the names Boyle's law and Gay-Lussac's law had been so persistently made use of in the paper, as he thought a wrong impression would be spread as to the nature of the phenomena. He also considered it probable that the proportionality observed was merely the result of the smallness of the ranges over which the experiments had been made. Mr. H. Crompton took exception to the imaginative character of the reasoning, and thought much more experimental proof was required before the results could be accepted for any but very small ranges of concentration. In answer to Prof. Reinold, Prof. Ramsay said the experimental data were not obtained by van 't Hoff himself, but were taken chiefly from Raoult's determinations.—On a method of comparing very unequal capacities, by Dr. A. H. Fison. One coating of each condenser is joined to earth, and to one end, A, of a high resistance (20,000 or 30,000 ohms), through which a current is flowing. The small condenser is charged to the P.D. existing between the ends A, B, of the resistance, and discharged into the large one. This is repeated a great number of times. If C be a point between A and B, the resistance between A and C may be varied until the P.D. between them is equal to that between the coatings of the condensers after n operations. If the insulated coatings be now joined to C through a galvanometer, no deflection will result. The relation between the capacities C_1 and C_2 of the large and small condensers is given by

$$\left(1 + \frac{C_2}{C_1}\right)^n = \frac{R_{AB}}{R_{BC}},$$

where R_{AB}, R_{BC} are the resistances between AB and BC respectively. Since time is required to perform the operation, the instantaneous capacities cannot be compared, and accordingly the measurements are taken after a definite time of electrification. A special rotating key was shown for performing ten operations per revolution, in which a trigger arrangement was provided for stopping the rotation after a predetermined integral number of revolutions. The method has been used for comparing a small air-condenser with a microfarad. The capacity of the former was also calculated electro-statically (correction being made for the edges), and that of the latter measured electro-magnetically by a ballistic galvanometer. The results give a value for v equal to $2{\cdot}965 \times 10^{10}$. In these experiments the capacity of the rotating key was allowed for. Under favourable conditions, capacities in the ratio of 1 to 1000 or 1 to 10,000 can be compared with an accuracy of $\frac{1}{4}$ per cent. Prof. Ayrton thought the novelty of the arrangement was in the rotating key, as the method of comparing unequal capacities by charging the smaller and discharging it into the larger a considerable number of times had been described and used by himself and Prof. Perry in their experiments on the specific inductive capacity of gases. —Mr. W. Lant Carpenter exhibited a new form of lantern, recently constructed by Mr. Hughes, of Dalston. The mahogany body is hexagonal, and each of the three front sides is provided with condensers and projecting arrangements. The back side opens to give access to the radiant, which in this case is a Brockie-Pell arc lamp, but if necessary a lime-light can be readily substituted. The lamp is fixed to the base-board, and the body can be rotated through 60° on either side of the central position, thus allowing any of the three nozzles to be directed towards the screen. The three sets of condensers are placed so that their axes intersect at a point about which the radiant is placed. The centre nozzle is fitted as a lantern microscope, with alum cell and various sets of condensing lenses and objectives, and a space in front of the main condensers is provided for polarizing apparatus. The focussing arrangement consists of a skew rack and pinion and a fine screw adjustment, and the whole microscope can be easily removed and a table polariscope substituted. The right-hand nozzle is arranged for the projection of ordinary lantern-slides, and the left-hand one is provided with an adjustable slit for spectrum work. A small table sliding on rails serves to carry the prisms, and the same rails support projecting lenses. Prof. S. P. Thompson congratulated Mr. Lant Carpenter on his selection of the Brockie-Pell lamp as the radiant, for, in addition to its being a focussing-lamp, it is unique in the fact that it works satisfactorily on either constant current or constant potential circuits.—Note on some additions to the Kew magnetometer, by Prof. Thorpe, F.R.S., and Prof. Rücker, F.R.S. In their magnetic survey of Great Britain and Ireland the authors have experienced considerable difficulty in making the necessary adjustments of the small transit-mirror used for determining the geographical N. point from observations on the sun. To make the required adjustments it is necessary to obtain an image of the cross-wires reflected from the mirror; and owing to the large amount of extraneous light, and the insufficient illumination of the cross-wire, the image is difficult to see. To exclude extraneous light, a tube is placed between the transit-mirror and the telescope, and a small screen placed behind the mirror. The cross-wires are illuminated by light reflected from a small platinum mirror introduced between the eye-piece and the cross-wires, which are viewed through a hole in its centre. The mirror is placed at 45° to the axis, and reflects a considerable quantity of light on the cross-wires when directed towards a bright part of the sky. In some cases it is advisable to take observations of the sun without first adjusting the transit-mirror, and afterwards correct the error introduced thereby. To do this a finely-divided scale is placed in the plane of the cross-wires, and from the position of the image, as indicated on the scale, the correction can be made. Observations taken with the mirror in adjustment and others taken when out of adjustment, and subsequently corrected, give very concordant results. The Rev. Father Perry said the improvements described were of great importance, for difficulties similar to those experienced by the authors had caused him to abandon the Kew magnetometer for field work, and to use a theodolite instead.

Linnean Society, June 21.—Mr. F. Crisp in the chair.—Mr. F. W. Oliver exhibited the aquatic and terrestrial forms of *Trapella sinensis*, of which he gave a detailed account, illustrated by diagrams.—Dr. R. C. A. Prior exhibited a branch of the so-called "Cornish elm," and described its peculiar mode of growth, which suggested its recognition as a distinct species. In the opinion of botanists present, however, it was regarded as merely a well-marked variety of the common elm.—On behalf of Mr. R. Newstead, of the Grosvenor Museum, Chester, photographs and drawings of the little grebe, *Podiceps minor*, were exhibited to illustrate a peculiarity observed in the mechanism of the leg-bones.—Mr. A. W. Bennett exhibited under the microscope, and made remarks upon, filaments of *Sphæroplea annulina* (from Kew), containing fertilized oospores.—Mr. Thomas Christy exhibited specimens of natural and manufactured Kola nuts, and explained how the latter might always be detected.—The following papers were then read:—Dr. P. H. Carpenter, on the *Comatulæ* of the Mergui Archipelago.—Prof. P. Martin Duncan and W. P. Sladen on the *Echinoidea* of the Mergui Archipelago.—Mr. W. P. Sladen, on the *Asteroidea* of the Mergui Archipelago.—Mr. W. Bolus, on South African *Orchideæ*.—Mr. R. A. Rolfe, a morphological and systematic revision of *Apostasiæ*.

Geological Society, June 7.—Dr. W. T. Blanford, F.R.S. President, in the chair.—The following communications were read:—A letter from H.M. Secretary of State for India, accompanying some specimens of rubies in the matrix from Burma.—On the Sudbury copper deposits (Canada), by J. H. Collins.—Notes on some of the auriferous tracts of Mysore Province, Southern India, by George Attwood.—On the Durham salt-district, by E. Wilson. In this paper the author described the new salt-field in the North of England, occupying the low-lying country bordering the estuary of the Tees, and situated partly in Yorkshire and partly in Durham. The history of the rise and progress of the salt-industry in South Durham was given, since the first discovery of salt by Messrs. Bolckow, Vaughan, and Co., at Middlesborough, in the year 1859. The stratigraphical position of the saliferous rocks of the Durham salt-district was considered in some detail. The diverse views which have been previously expressed on this head were referred to, and reasons given for concluding that all the beds of rock-salt which have been hitherto proved in this field, and the red rocks with which they are associated, belong to the upper portion of the Trias, viz. to the Upper Keuper series (Waterstones subdivision). The probable area of this salt-field, the limits of the distribution, and varying depths of the chief bed of rock-salt were indicated, and the extent of its supplies pointed out. In conclusion, the author called attention to the waste, as well as to certain other disadvantages resulting from the process of winning the salt now in operation.—On the occurrence of *Calcisphæræ*, Williamson, in the Carboniferous Limestone of Gloucestershire, by E. Wethered.—Second note on the movement of scree-material, by C. Davison; communicated by Prof. T. G. Bonney, F.R.S.

Anthropological Institute, May 29.—Francis Galton, F.R.S., President, in the chair.—A paper by Mr. G. H. Kinahan was read, on rubbings from ancient inscribed stone monuments in Ireland.—Dr. Stewart gave an account of the inhabitants of Paraguay.

June 12.—The Rev. H. G. Tomkins read a paper on Mr. Flinders Petrie's collection of ethnographic types from the monuments of Egypt. The author classified the collection under the four heads of Westerns, Southerns, Asiatics, and Egyptians; and examined, in order, the races mentioned under each of these heads. Among the Westerns are the Tahennu, or fair people, who, as Egyptian mercenary troops, founded, by a prætorian revolt, the famous twenty-second dynasty, to which Shishak, the invader of Palestine, belonged. The Lebu, or Libyans, fall under this head; and the author identifies with them the light-complexioned, fair-haired, and blue-eyed brickmakers of the celebrated tomb of Rekhmara. The want of the long side-locks is not surprising, since they were slaves employed in the lowest drudgery. The Shardina furnished highly-trained soldiers to the Egyptian army of Rameses II. They wore helmets with two horns, crested with a disk, and seem to have been Sardinians. Under the head of Southerns we have very various and interesting types. It is curious to find, in the paintings, blacks with red hair; but it seems probable that the colour was produced by the use of dye. Mr. Tomkins gave a full description of the race of Pûn, and dwelt particularly upon the terraced mountains covered with incense-trees that caused so much astonishment to the officers of Queen Hatasu. He also gave a probable explanation of the origin of the remarkable features of Amenhotep IV., the celebrated Khu-en-aten, whose mother, Queen Tua, was distinguished for her beauty.

Mathematical Society, June 14.—Sir J. Cockle, F.R.S., President, in the chair.—The Vice-Chancellor of Cambridge University (Dr. C. Taylor), read a paper on the determination of the circular points at infinity.—Prof. M. J. M. Hill followed with a paper on the c- and p-discriminants of integrable differential equations of the first order.—Mr. Tucker (Hon. Sec.), communicated papers by Lord Rayleigh, Sec. R.S., on point-, line-, and plane-sources of sound.—Note on rationalization, by H. Fortey.—Applications of elliptic functions to the theory of twisted quartics, by Prof. G. B. Mathews.—Prof. Greenhill, F.R.S., communicated remarks on coefficients of induction and capacity and allied problems, in continuation of a former paper (January 1879).—The following were taken as read: electrical oscillations, by Prof. J. J. Thomson, F.R.S.; and demonstration of the theorem "that the equation $x^3 + y^3 + z^3 = 0$ cannot be solved in integers," by J. R. Holt.

Zoological Society, June 5.—Dr. Edward Hamilton, Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of May.—Mr. H. E. Dresser exhibited a specimen of a new Shrike from the Transcaspian district of Central Asia, which he proposed to name *Lanius raddei*, after Dr. Radde, of Tiflis, its discoverer.—Mr. Sclater, on the part of Mr. F. M. Campbell, exhibited a pair of Pallas's Sand-Grouse (*Syrrhaptes paradoxus*), shot in Hertfordshire in May last, and made remarks on the recent immigration of this Central Asiatic bird into Western Europe.—The Secretary exhibited, on behalf of Prof. R. Collett, a nest, eggs, and two young ones in down of the Ivory Gull (*Larus eburneus*), belonging to the Tromsö Museum, which had been obtained in Spitzbergen in August 1887.—Mr. Warren communicated a paper on Lepidoptera collected by Major Yerbury in Western India in 1886–87, forming a continuation and completion of two previous papers by Mr. A. G. Butler on Lepidoptera collected by the same gentleman in similar localities. The present collection contained examples of over 200 species of Heterocera, of which about one-fourth were described as new. Mr. Warren remarked upon the abnormal development of separate organs, such as the antennæ and palpi, in tropical insects, as being rather specific aberrations from a generic type, than as warranting the erection of new genera.—A communication was read from Mr. Martin Jacoby, containing descriptions of some new species of Phytophagous Coleoptera from Kiukiang, China.—Mr. F. E. Beddard read some notes on the structure of a peculiar sternal gland found in *Didelphys dimidiata*.—Mr. G. A. Boulenger read a paper on the scaling of the reproduced tail in Lizards, and pointed out that the scaling of the renewed tails of Lizards may, in some cases, afford a clue to the affinities of genera or species to one another.—Mr. F. E. Beddard gave a preliminary notice of an apparently new form of Gregarine, found parasitic on an earthworm o the genus *Perichæta* from New Zealand.

CAMBRIDGE.

Philosophical Society, May 21.—Mr. J. W. Clark, President, in the chair.—On solution and crystallization, by Prof. Liveing. When a substance passes from a state of solution into the solid state, the new arrangement of the matter must be such that the entropy of the system is a maximum; and, other things being the same, the surface energy of the newly formed solid must be a minimum. If the surface tension be positive, that is tend to contract the surface, the surface energy will be a minimum when the approximation of the molecules of the surface is a maximum. The essential difference between a solid and a fluid is that the molecules of the former maintain approximately the same relative places, whereas the molecules of a fluid are subject to diffusion. Further, crystalloids in assuming the solid form assume a regular arrangement of their molecules throughout their mass, which we can usually recognize by the optical properties of the crystal, and by the cleavage. If we suppose space to be divided into equal cubes by three sets of parallel planes, each set at right angles to the other two, and suppose a molecule to be placed in every point where three planes intersect, we shall have an arrangement which corresponds with the isotropic character of a crystal of the cubic system. But of all the surfaces which can be drawn through the system the planes bounding the cubes meet the greatest number of molecules, those parallel to the faces of the dodecahedron meet the next greatest number of molecules, and those parallel to the faces of the octahedron meet the next greatest number. Also if we take an angular point of one of the cubes as origin, and three edges of the cube as axes, and the length of an edge of the cube as the unit of length, every plane which cuts the three axes at distances p, q, r respectively from the origin, where p, q and r are whole numbers, will be a surface of maximum concentration of molecules, but the concentration will be less as p, q and r are greater. Hence forms which are bounded by these planes, which follow the law of indices of crystals, will be forms of minimum surface energy and therefore of equilibrium. The tendency in general will be for substances with such a structure as is here supposed to take the form of cubes, since the cube will have the greatest concentration of molecules per unit of surface. But the total surface energy will depend on the total surface as well as on the energy per unit of surface, and for a given volume the surface will be diminished if the edges and angles of the cube are truncated by faces of the dodecahedron and octahedron, or by more complicated forms. When a solid is broken, two new surfaces are formed each with its own surface energy, and the solid must be more easily fractured when the new surfaces have the minimum energy. Hence substances with the structure supposed must break most easily in directions parallel to the sides of the cube, dodecahedron and octahedron; and these are the cleavages observed in this system. If we suppose the molecules placed at the centres of the faces of the cubes, instead of at the angles, the arrangement will still be isotropic, but the octahedron will be bounded by the surfaces of greatest condensation, and the cube will come next to it. It is probable that substances which cleave most readily into cubes, such as rock-salt and galena, have the former structure, while those which have the octahedral cleavage may have the latter arrangement of their molecules. For the pyramidal and prismatic systems we may suppose space divided not into cubes but into rectangular parallelopipeds with edges equal severally to the axes of the crystals, and molecules placed as before. For the rhombohedral system we may suppose space divided into rhombohedra, or in crystals of the hexagonal type into right prisms with triangular bases, and for the other systems into parallelopipeds with edges parallel and equal to the axes. In each case if the molecules be disposed at points of intersection of three dividing planes we shall have such an arrangement as satifies the optical conditions, and planes which follow the law of indices are surfaces of maximum condensation. Calculations show that whenever a crystal has an easily obtained cleavage the direction of cleavage corresponds to the surface of greatest condensation, and that the most common forms of crystals correspond in general to forms of minimum surface energy. The surface tension of a plane surface will have no resultant out of that plane, but where two plane surfaces meet in an edge, or angle, the tensions will have a resultant of sensible magnitude in some direction falling within the angle. Whenever all the faces of a crystallographic form are developed, every such resultant will be met by an equal and opposite resultant, and the form will be one of equilibrium. If one edge, or angle, be modified, the opposite edge, or angle, must either be similarly modified, or the resultant arising from the modification must be equilibrated by some internal forces produced by displacement of the molecules. In general, equilibrium is attained by similar modifications of similar edges and angles, but when only some of the edges or angles of a crystal are modified, while other similar edges or angles are not modified, we usually have evidence of the consequent internal strain. Thus cubes of sodium chlorate, which have half the angles truncated by faces of a tetrahedron, rotate the plane of polarized light, hemihedral tourmalines are pyro-electric, and so on. This theory therefore accounts for the plane faces of crystals, the law of indices, the most common combinations, and the cleavages. The same theory accounts for the development of plane faces when a crystalline solid of any shape is slowly acted on by a solvent. Solution will proceed so long as the entropy of the system is increased by the change, but when the solution is nearly saturated there will be an increase of entropy from the solution of a surface which has more than the minimum surface energy, while there will be no increase from the solution of a surface which has only the minimum energy.—On the effect of an electric current on saturated solutions, by Mr. C. Chree, M.A. This paper contains an account of experiments whose aim was to determine what effect, if any, an electric current may have on the quantity of salt required to form a saturated solution. Strong currents and a rapidly reversing commutator were employed. Certain chlorides were dealt with, and in no case did the existence of a current produce any sensible immediate effect. When heating was allowed to take place, the action of the current appeared to check the solution that would naturally have followed. This view was further supported by experiments on the effects of simple heating. These experiments showed, however, that an originally saturated solution when slowly heated can dissolve salt only with extreme slowness.

PARIS.

Academy of Sciences, June 18.—M. Janssen, President, in the chair.—Lagrange's hypothesis on the origin of comets and meteorites, by M. H. Faye. According to the author's calculations, this hypothesis, first submitted to the Bureau of Longitudes in 1812, does not hold good for the comets whose orbits do not quite approach any of the planetary orbits. But it would seem capable of being applied to the meteorites, whose fragmentary character, minute size, chemical and mineralogical identity with the constituent elements of the earth, combined with their great abundance, would seem to be absolutely incompatible with an extra-planetary origin. The earth alone with its satellite best satisfies all the conditions of the problem, while its orbit is continually intersected by millions of these bodies, as required by the hypothesis in question. Hence their origin is to be sought in the earth itself and in the moon, whence they were ejected under conditions which have long ceased to exist.—Fluorescence of ferruginous lime, by M. Lecoq de Boisbaudran. These experiments show that a small quantity of the sesquioxide of iron added to the carbonate of lime produces a green fluorescence after high calcination in the air. This fluorescence, which is occasionally somewhat intense, is very sensitive to the action of heat; hence it soon fades away in the presence of the electrode, retaining its brilliancy only in the parts of the tube furthest removed from the centre of action.—Experimental researches on the diseases of the vine, by MM. Pierre Viala and L. Ravaz. Having already shown that the different reproductive organs found on the parts affected by black rot belong to the fungus, cause of this disease, the authors here demonstrate the true parasitic character of the fungus itself. They once for all establish the filiation which exists between its various forms of reproduction, and thus make it evident that the blight on the leaves has the same origin as that of the grapes.—Researches on the accidental errors occurring in the observations of transits made by the method of eye and ear, by M. G. Rayet. In supplement to the studies of Struve, Robinson, Dunkin, Finlay, and others, the author here describes the results of special observations made on about seventy stars, or constellations, comprised between 20° of austral declination and the North Pole. He has thus determined the numerical value of the accidental errors relative to some dozen stars between 80° and 89° 22'·3 of declination.—On the rings of Saturn, by M. Perrotin. During the opposition of Saturn in the present year the author has made a series of micrometric measurements of the rings by means of the great equatorial of the Observatory of Nice. The results of these observations, made for the purpose of determining the dimensions of the system, are here fully tabulated for the whole period from February 2 to May 8.—On the planet Mars, by M. Perrotin. On presenting the already promised sketches of recent appearances in this planet, the author remarked that since his last communication the region of Libya has undergone fresh modifications. The sea which covered the surface of this insular mass has mostly receded, its present appearance being intermediate between that of 1886 and its condition a few weeks ago. The existence has also been determined of canals or channels, partly double, running from near the equator to the neighbourhood of the North Pole. They mainly follow the meridian, and merge in the seas encircling the white snow-cap of the Pole, and, strange to say, their course may be followed across the seas themselves right up to the snow-cap.—Heat of combination of the primary, secondary, and tertiary aromatic monamines with the acids, by M. Léo Vignon. In continuation of M. Louguinine's study of the primary monamines, the author here investigates the reactions of several acids on a series of primary, secondary, and tertiary monamines. He deals more especially with aniline, monomethyl aniline, and dimethylaniline in the presence of the hydrochloric, sulphuric, and acetic acids.—On the decomposition of the ferrate of baryta at high temperatures, by MM. G. Rousseau and J. Bernheim. In his researches on ferric acid, Fremy has indicated the analogy existing between the ferrates and the manganates, as established by the wet process. Here the authors endeavour to ascertain whether the parallelism is maintained in the reactions of the dry process and in their mode of decomposition under the action of heat.—On some new double phosphates in the magnesian series, by M. L. Ouvrard. The products here described have been obtained by the method already referred to in a previous note on the action of the alkaline phosphates on the alkaline earthy oxides. All the metals investigated are allied in their composition to the substances obtained with the pyro- and ortho-phosphates of potassa and soda.—On the poison of the Hymenoptera with smooth sting, and on the existence of a poison-cell in the honey-producing insects, by M. G. Carlet. In continuation of his researches on the barbed sting of bees, wasps, &c., the author here studies the smooth sting of Philanthus, Pompilus, &c. He describes the nature of the poison, which has merely a soporiferous effect, and clearly determines the presence of a poison-cell in bees and allied insects.—On a new bacterial disease of the duck, by MM. Cornil and Toupet. An examination of the bacteria of this disease ("duck cholera") shows that it is quite distinct from chicken cholera. The virus is fatal to the duck alone, sparing hens and pigeons, and killing rabbits only when an excessive dose is administered.—M. A. d'Arsonval contributes an elaborate paper on the relation between animal electricity and surface tension.

AMSTERDAM.

Royal Academy of Sciences, May 26.—M. Franchimont, communicating the results of experiments on nitro-ureides and nitramines, said that internal ureides, by their behaviour with nitric acid, may be distinguished into at least three sorts.—M. Sahols treated of the calculation of the moments of flexion and the shearing-forces in railway-bridges, in connection with the irregular distribution of the pressures exercised by the axles of locomotive-engines. He pointed out what elements of the engine are of especial influence on these, and arrived at very simple approximative formulæ for the calculation of the said moments and forces on bridges of not too insignificant length.—M. Pekelharing read a paper on the proliferation of endothelium-cells in arteries, stating, as the result of his experiments made upon them, that this proliferation is most probably caused by a diminution of the pressure upon the inner wall of the arteries.—M. van der Waals treated of the connection between the change in the density of the limiting layer between fluid and vapour, and the mode of action of the molecular forces.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Proceedings of the Royal Society of Edinburgh, Sessions 1883 to 1887 (Edinburgh).—Transactions of the Royal Society of Edinburgh, vol. xxx. Part 4, vol. xxxii. Parts 2, 3, 4, vol. xxxiii. Parts 1, 2 (Williams and Norgate).—Transactions of the Royal Society of Edinburgh, vol. xxxi. Botany of Socotra: Prof. I. B. Balfour (Williams and Norgate).—British Reptiles and Batrachians: C. C. Hopley (Sonnenschein).—Anleitung zu Wissenschaftlichen Beobachtungen auf Reisen, Bands 1 and 2: Dr. G. Neumayer (Oppenheim, Berlin).—Mathematical Drawing Instruments, sixth edition: W. F. Stanley (Spon).—Proceedings of the American Association for the Advancement of Science, New York Meeting, 1887 (Salem).—British Dogs, Parts 17 to 20: H. Dalziel (U. Gill).—Observations made at the Hong Kong Observatory in the year 1887: W. Doberck (Hong Kong).—Synopsis of the Aphididæ of Minnesota: O. W. Oestlund (St. Paul).—Report on Botanical Work in Minnesota for the year 1886: J. C. Arthur (St. Paul).—Preliminary Description of the Peridotytes, Gabbros, Diabases, and Andesytes of Minnesota: M. E. Wadsworth (St. Paul).—Palæolithic Man in Eastern and Central North America (Cambridge, Mass.).—Journal of the Royal Microscopical Society, June (Williams and Norgate).—Proceedings of the Society for Psychical Research, June (Trübner).—Sulla Forza Elettromotrice del Selenio, Memoria del Prof. A. Righi (Padova).

CONTENTS.

THURSDAY, JULY 5, 1888.

THE DECADENCE OF THE CHEMICAL PROFESSION IN GOVERNMENT OPINION.

THE Professorship of Chemistry in the Royal Naval College, Greenwich, is, or is about to become, vacant through the resignation of Dr. H. Debus, F.R.S., and it is currently reported that the authorities have been advised to discontinue the professorship, and to substitute for it a mere lectureship or readership. We trust that this rumour may prove unfounded, or that the Government may be led in time to see the folly of degrading a subject which, if properly handled, is of such extreme value and importance to the Navy. We say degrade, because in the first place it cannot be questioned that chemistry is a science which may claim to rank with any other which enters into the curriculum at Greenwich, both on account of its educational value and its direct usefulness; and because any such change must of necessity tend to lower the value of chemical knowledge in comparison with that of other subjects in the eyes of the students.

It is scarcely necessary to point out in how many ways a knowledge of chemistry may be of service in the Navy. Our sailors are stationed in all parts of the world, and the question of water-supply both for men and boilers is an ever-present one: a decision as to the quality of a water can only be given after it has been examined chemically. Again, the action of sea-water on metals, the corrosion of metals, the decay of timber, the economical use of fuel, are all matters in which the sailor nowadays is deeply interested, and these can only be rightly understood by those who have acquired a sound knowledge of chemical principles. There are very many other ways in which chemistry is of direct value to the sailor; but, most important of all, there is no subject which, if properly and practically taught, affords the same opportunity of training the student to observe accurately and to think correctly, and it is especially on this ground that chemistry should be assigned a high position in the course at a Naval College. It will, however, not suffice to require attendance at a course of lectures in which general chemistry is treated of in slow and measured cadences and no heed is paid to the requirements of the students: the subject must be taught technically, and almost exclusively with direct reference to matters familiar to sailors and to their future requirements; and the training must be to a very large extent carried on in the laboratory, and not in the lecture-room.

If the results thus far obtained at Greenwich have not been such as to lead the authorities to appreciate the value of the subject, the most short-sighted course they can possibly pursue in the hope of obtaining better results in the future will be to assign a lower rank to chemistry. In cases of grave disease, if a practitioner, guided by particular traditions, and operating under conditions which he takes no particular pains to control, be unsuccessful, it is not usual to call in another of lower grade; but on the contrary, if possible, one of equal or higher grade is summoned, holding different and perhaps wider views, and the effort is made to improve the conditions so as to give every opportunity for his treatment to be successful: and so may it happen, we trust, at Greenwich.

In these anxious times of fierce competition the nation cannot afford that the Government should act so as in the least degree to diminish the importance of so valuable a branch of science as chemistry. Moreover, a golden opportunity will be lost if occasion be not now taken to appoint at Greenwich a chemist who not only is known to have been thoroughly trained, but who has given proof, by his own researches and those of his pupils, that he is possessed of enthusiasm, and capable of extending our knowledge. In connection with explosives, and in many other directions, there is infinite opportunity for research; and it is a disgrace to the nation that the Navy at present has not a single chemist of repute in its service, especially as such invaluable service has been rendered to the War Department by its chemist, Sir Frederick Abel.

If the professorship at Greenwich be quashed, it is unlikely that a man of proper calibre will be attracted by a mere lectureship; and thus another step will have been taken to indicate that in this country we care little for science, that our Government is blind to facts so clearly recognized by foreign Powers. Among the noted men of science now in the House of Commons, besides Prof. Stokes, there are three chemists, Prof. Maskelyne, Sir Lyon Playfair, and Sir Henry Roscoe: we feel sure that they will not allow the Government to make a false move in so important a matter without publicly warning them, and without fully eliciting their reasons.

THE LAND AND FRESH-WATER MOLLUSCA OF INDIA.

Land and Fresh-water Mollusca of India. Edited by Lieut.-Colonel H. H. Godwin-Austen, F.R.S., &c. Parts I. to VI. (London: Taylor and Francis, 1882–88.)

ALTHOUGH much has been done to elucidate the fauna of our great Eastern dependency, very much more still remains to be accomplished: vast tracts have yet to be explored scientifically, even though, year by year, new areas are visited by the naturalist and collector, and fresh species are added to the list.

This is especially evident in the case of land and fresh-water Mollusca; whilst so scattered are the various descriptions of the species up and down the pages of different scientific journals and magazines, that the specialist himself has a hard task to ascertain whether a given example is new or not.

It is true that Hanley and Theobald,[1] in their now classical work, went some way towards remedying this state of things; but their task was never completed, and many new forms have been discovered since their publication was brought to a close.

Under these circumstances the present undertaking cannot fail to be most welcome. It is modestly described as "supplementary" to the work just named; but, in reality, it is something far more important, if we may

[1] The names are inadvertently reversed on the title-page of Colonel Godwin-Austen's book.

judge from the six parts (257 pp.), which, with their sixty-two hand-coloured plates, will, when the index is issued, complete the first volume. Each species figured is most thoroughly described, and, when not new, full quotations with the synonymy are given. The figures are also the handiwork of Colonel Godwin-Austen, and though they by no means attain to that standard of excellence with which Sowerby at his best made us familiar, they are effective and (fortunately) under, rather than over, coloured. The illustrations of the living animals, which are copied from drawings by a native artist, are extremely spirited and life-like. Anatomical details where obtainable are given, and, what is yet more important from the systematic point of view, the Radulæ are figured; for, whatever may be the case with marine forms, in the Pulmonates certainly it is of the greatest importance.

How truly gigantic the task Colonel Godwin-Austen has set himself, becomes apparent when it is seen that, disregarding political boundaries, under "India" are included "South Arabia, Baluchistan, Afghanistan, Kashmir, Nepal, Burmah, Pegu, Tenasserim, Malay Peninsula, Ceylon, and other islands of the Indian Ocean"; whilst, when necessary for purposes of comparison, genera from yet other countries are also described and figured (*e.g. Geomalacus*, *Africarion*)—of a truth there does not seem to be any probability that the author will ever, like some scientific Alexander, be in want of fresh fields for conquest!

The weak point of the work appears to be that "the genera and sub-genera are treated of in no particular order, ... but as data concerning them can be put together and the drawings completed." Nothing, we feel sure, but the necessity of doing so, if the work was to be published at all, can have induced the author to adopt such a course. Few things are more provoking to the student than the necessity of turning to many different pages in the same work when engaged on a particular subject, an inconvenience which even a good index does not obviate; whilst in its absence matters are not improved by such a table as the one given on p. 253, which professes to be "a classification of families and genera treated of in the preceding pages," but which only includes those placed by the author in Fam. Zonitidæ (or, as it is misprinted, Zon*a*tidæ).

This infringement of Nature's first law renders it hard to disabuse one's mind of the unfortunate impression derived on a first examination that the author had transcribed and enlarged his preliminary notes without previously sorting them. Nor does the reason alleged seem altogether sufficient: "the classification can be hereafter attempted; we shall then be better able to judge what weight, generic or sub-generic, to give to the many genera now recorded from the Indian region." This did not preclude the author from giving—as we trust he will do in a future part—what all who try to follow him would find of great assistance; namely, a provisional table of classification in which the main divisions at all events should be shown.

Such a scheme would be none the less useful seeing that he evidently, like most authorities, has his own notions on the subject, which at present can only dimly be guessed at by a careful perusal of the text. Thus at p. 165 he speaks of "the two great natural divisions of land Mollusca, . . . the Helicidæ and Cyclophoridæ." Again, he agrees with Fischer (v. p. 59) that *Hyalimax* belongs to the same group as *Succinea*; but on pp. 64–65 gives a "key to genera of Limacidæ and Arionidæ" in which *Hyalimax* figures.

Another drawback, if it may be so described, is the undue prominence given to minor differences, and the consequent elevation into genera of what in the eyes of the general conchologist are sub-genera, or even mere sections of sub-genera. This, however, raises a very wide and much vexed question, into which far be it from us to enter.

In thus briefly indicating what appear to us the shortcomings of this important work, we are by no means blind to its great value, and we most heartily wish success to its author in his arduous undertaking, which bids fair to prove as endless as that of Sisyphus.

RECENT MATHEMATICAL BOOKS.

A Chapter in the Integral Calculus. By A. G. Greenhill, M.A. (London: Hodgson, 1888.)

A Treatise on Plane Trigonometry: containing an Account of Hyperbolic Functions, with Numerous Examples. By John Casey, F.R.S. (Dublin: Hodges, 1888.)

A Higher Arithmetic and Elementary Mensuration. By P. Goyen, Inspector of Schools, New Zealand. (London: Macmillan, 1888.)

The Harpur Euclid. Book II. By E. M. Langley, M.A., and W. S. Phillips, M.A. (London: Rivingtons, 1888.)

THE first book in this list is intended to be used by way of supplement to any ordinary treatise on the calculus. It might almost be said that Prof. Greenhill is nothing if not *hyperbolic*, for he expatiates in seas of these functions and the kindred Weierstrassians. No one has done better work than he in his endeavours to make them "familiar as household words" to students, to whom, as Dr. Casey remarks in the preface to his "Trigonometry," they are very interesting and important, not only in pure mathematics but also in mathematical physics. Our author, who is quite in accord with this opinion, considers that "the hyperbolic functions have not received adequate treatment in ordinary text-books; to illustrate this importance, a digression has been made on their principal properties, illustrated by examples of their application."

In the course of thirty-six pages he gives an exceedingly clear sketch, and works out in detail several examples, viz. the different forms of the result of $\int dx/(x-p)\sqrt{R}$, where $R \equiv ax^2 + 2bx + c$, and several kindred forms. The analogies and properties of the hyperbolic functions are considered; three sections are given up to hyperbolic trigonometry; three more to relations connecting true, excentric, and mean anomaly in an elliptic and hyperbolic orbit; and a section to Abel's theorem and the general integral $\int \frac{N}{D} \cdot \frac{dx}{\sqrt{R}}$, and to the rectification of some curves.

There is a large collection of examples, and the whole pamphlet is "teres atque rotundus."

Dr. Casey's "Treatise on Plane Trigonometry" is quite independent of the "Elementary Trigonometry" by the same author. It is a most comprehensive work, and quite as exhaustive as any ordinary student will require. Dr. Casey shows his usual mastery of detail, due to thorough acquaintance, from long teaching, with all the *cruces* of the subject. He has embraced in his pages all the usual topics, and has introduced several points of extreme interest from the best foreign text-books. A very rigid proof is given of the exponential theorem, and a section is devoted to interpolation. Dr. Casey approves of, but does not at present venture to adopt, the practice of French authors who use log sin A instead of our old friend L sin A, *i.e.* he would prefer $\bar{1}\cdot859$ to $9\cdot859$.

Chapters V. and VI., which are devoted to triangles and quadrilaterals, are exceedingly interesting, and contain quite a crop of elegant propositions culled from many fields. Following the course adopted by other recent writers, he gives a systematic account of imaginary angles and hyperbolic functions. "The latter are very interesting, and their great and increasing importance, not only in pure mathematics but in mathematical physics, makes it essential that the student should become acquainted with them." We may remark that Dr. Casey adopts the following notation: sh, ch, th, coth, sech, cosech, for sinh, cosh, &c.; and has gone further than his English predecessors in introducing at this early stage the angle τ, Hoüel's *hyperbolic amplitude of* θ (τ = amh. θ). Numerous illustrative examples and tables afford practice to the student in this branch.

The modern geometry has a small niche, and here we note, as one of several small clerical errors come across, in addition to the list furnished, that (440) should have *cosecants* in place of *secants*. The special results, which on Dr. Casey's useful plan are numbered consecutively, reach 810. The book is rich in examples, and will be sure to find for itself a place on the mathematician's shelves within easy reach of his hand.

The object of the author of "A Higher Arithmetic and Elementary Mensuration" is to furnish a work suited to "the senior classes of schools, and candidates preparing for public examinations." A large number of typical exercises are worked out, and the student, being left to observe and think for himself, acquires, or should acquire, a sound practical knowledge of the subject, which the author rightly thinks will be more abiding than the knowledge of rules and definitions obtained by the mere committal of them to memory. For the benefit of beginners, in many of the examples the steps of the reasoning are given at some length, but the student is advised, as he grasps the details, to shorten the work as much as possible in the examples he subsequently works out. The text covers all the ordinary divisions under which arithmetic is discussed in the books, even our old friend alligation having a chapter assigned to it. The last two chapters are devoted to the mensuration of plane surfaces and of solids. There are 400 exercises at the end, in addition to a very great number scattered throughout the book. The whole is a vast storehouse of well-put matter, which should render a reader quite independent of any other text-book, and, we might say, of a teacher.

Book II. of "The Harpur Euclid" is on the lines laid down in the edition of Book I., and the subject is handled in an interesting manner. There is a sufficient number of good illustrative examples, with assistance enough to enable a thoughtful boy to work them out by himself. We are glad to see a few examples on antiparallels and symmedians. These lines must soon force their way to a foremost position even in a school curriculum. This is a useful and handy edition brought out in accordance with the Syllabus of the Association for the Improvement of Geometrical Teaching.

THE BOTANY OF THE AFGHAN DELIMITATION COMMISSION.

The Botany of the Afghan Delimitation Commission. By J. E. T. Aitchison, M.D., F.R.S., Naturalist attached to the Mission. Being Trans. Linn. Soc., Ser. 2, Bot. v. 3, pp. 1–139, tt. 1–48; with two Maps. (1888.)

OF this expedition Dr. Aitchison has already published, in the *Pharmaceutical Journal and Transactions*, Ser. 3, v. 17 (1887), a report on the drugs, and he is preparing a report on the zoology to appear in the Transactions of the Linnean Society.

In several previous collections and papers relating to the Punjab flora ("Flora of Jhelum," "Lahul, its Flora and Vegetable Products," "Flora of Hushiapore," "Hand-book of Trade Products of Leh"), and especially in his Report on the plants of the Kuram Valley, Dr. Aitchison had shown himself an excellent collector and an enthusiastic botanist; and by the knowledge of the Afghan flora he had acquired in this preceding work he was eminently qualified to make the most of the opportunities afforded on hasty marches and in rough camps. The Secretary of State for India, who employed Dr. Aitchison on this duty, may certainly be well satisfied with the present botanic section of the Report. In 28 quarto pages Dr. Aitchison describes the country traversed, and the general character of the vegetation, interspersed with many economic and agricultural remarks. The remainder of the Report consists of a list of the plants collected in order, with descriptions of the new species, most of which are figured. There are about 800 plants catalogued, whereof 53 are new to science. The whole forms a most valuable addition to our scientific knowledge of an interesting frontier region. Dr. Aitchison started from Quetta on September 22, 1884, and proceeding west struck the Helmund on October 19; following the course of the Helmund and Harut, he was close to Herat on November 4; the remaining nine months, up to September 1885, he was in Khorassan and Badghis, *i.e.* in North Cabul.

The dry region of South-West Asia extends into Western India—into Sind, the Punjab, Rajputana; but in his "Flora of British India," Sir J. D. Hooker accepts the political frontier of India as his western limit. It is impossible in local floras to find natural boundaries. Beluchistan and Cabul are thus excluded from the "Flora of British India." They are included in Boissier's "Flora Orientalis"; but Boissier had by no means plentiful material for this frontier. The additions now made by Dr. Aitchison are not to be estimated by the 53 new species alone, but by the further light thrown on numerous little-known species, and especially by the quantity of economic information collected.

Of the 800 plants enumerated, the richest orders are Leguminosæ with 78 species, Compositæ with 77 species, Gramina with 63 species, Cruciferæ with 57 species, Chenopods with 38 species. The large Umbelliferæ allied to Asafœtida are finely illustrated in plates 18 to 29; four new species are described. There remain still many points about these valuable gum-producing plants of Central Asia that are obscure. Of the 78 Leguminosæ, no less than 37 are of the genus Astragalus, and of these 13 are new. Of the petaloid Monocotyledons the most prominent are the Iridaceæ (2 new species of Iris), and the Liliaceæ (26 species, of which 3 are new).

The introductory narrative, with the lists of characteristic plants at different levels and localities, enables a phytographic botanist to apprehend the nature of the country and climate. Cabul is clearly a much richer country agriculturally than has been hitherto supposed. Corn can be cultivated without irrigation either above 3500 feet altitude, or in the vicinity of a river; and a large area between these levels is capable of irrigation. The dry and hot summer is, as was before well known, very favourable to the production of fruit, and it now appears almost equally so to the production of vegetables. Dr. Aitchison found "not uncommon," in clefts of rocks and escarpments of hill-sides, the common fig (*Ficus Carica*, Linn.), apparently wild; and collected both male and female branches, some of the male receptacles containing both male and gall flowers. Dr. Aitchison had few opportunities of examining the country above 5000 feet; at the spots he did visit he found a very scanty flora, and above 7000 feet absolute sterility.

Dr. Aitchison compared his collection in the Kew Herbarium, and had the assistance of Mr. W. B. Hemsley in the technical botanic work, and in arranging the plates; and the new species described are given as of "Aitchison and Hemsley," except a few Liliaceæ, &c., attributed to "Aitchison and Baker." By this plan Dr. Aitchison gives to botanists who cannot refer to the specimens a guarantee that the new species are "good," and that the list of names has been accurately worked out. It is indeed the closeness with which a list of the present kind is worked out that gives it more than a temporary value.

Praise is due to Dr. Murie, the Assistant Secretary of the Linnean Society, for the style in which this number of the Society's Transactions has been put out. Credit may certainly be given to the India Office for assisting in a publication of this class; somebody there must have discovered that the money spent by the old Company on Roxburgh and Buchanan-Hamilton, on Royle and on Wallich, was not money spent on ornamental books, but has been returned, many times over, to the Government coffers.

OUR BOOK SHELF.

The Principles of Agricultural Practice as an Instructional Subject. By John Wrightson, Professor of Agriculture and Principal of the College of Agriculture, Downton. (London: Chapman and Hall, 1888.)

THIS is a useful text-book, written in an interesting style, and by one who shows that in addition to being scientific he is thoroughly practical. The subject-matter of the book was first delivered as lectures to science teachers, and it deals with the duties of teachers as well as the defects of students under examination. It exposes in commendable language the narrow grooves into which agricultural teaching under the Science and Art Department has fallen. This is called "molecular and microscopic" in place of "bold and comprehensive," which ought to be the suitable form of description if the Department were properly constituted.

The book is the first of a series of text-books. It disposes in a clear and unmistakable manner of many knotty points of difficulty to the farmer and to the student, in matters relating to the nature and composition of soils, kinds and qualities of manures—"artificial and natural," "general and special,"—also to the cultivation of soil, and the growth and rotation of crops. Under these various headings many popular fallacies are exposed, connected with the classification of soils, the action of lime and nitrate of soda when applied to soil, the value of silica and of farm-yard manure, the sources of the supply of nitrogen to the growing plant, and the supposed ultimate exhaustion of soil—called a "store-house, a laboratory, a vehicle"—by systems of cropping.

The merits and methods of "autumn-cleaning" are duly introduced. The valuable work of the Rothamsted experiments is fully acknowledged and concisely explained.

This new contribution to agricultural literature comes at an appropriate season, when there is a growing demand for text-books of a trustworthy kind: so few can be found which are not simply the incoherent drivel of men who have but a very limited and imperfect knowledge of the subject.

The work is written in a style which will lead the student to think for himself, and but for one serious blunder in the later pages we should have pronounced it to be exceptionally perfect. Partial toleration is extended to the practice of sowing down land to pasture with seeds swept from the stable-loft. The loss sustained by the country through Miss Ormerod's warble-fly is thrown into the shade by the loss which has resulted from this exploded system of seeding down to grass. We hope to see the error corrected in a second edition, which, judging from the value of the book, cannot be long in making its appearance.

A Season in Sutherland. By J. E. Edwards-Moss. (London: Macmillan, 1888.)

THIS is a pleasant little book, though it affords no kind of information to the naturalist or to the sportsman, while it can hardly pretend to rank as a contribution to *belles lettres*. But Mr. Edwards-Moss is acquainted with certain districts in the north of Sutherlandshire; he has thrown a fly, and shouldered a breechloader; and he writes of his experiences in an unpretentious and graceful way which ought to commend the little volume as an accompaniment to an after-dinner cigar. He also quotes freely from contemporary and other authorities, including amongst these that profound thinker and teacher, Mr. Mallock. Mr. Mallock, as quoted by Mr. Edwards-Moss, tells us that we should "learn to love the sea, and the woods," and also "the wild smell of the heather"; from which we may gather that Mr. Mallock has probably discovered some portion of the country in which the heather smells of patchouli.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

"Sky-coloured Clouds" at Night.

IN NATURE, June 28 (p. 196), Mr. Backhouse notes the appearance of illuminated clouds to northward at night. Similar clouds are seen from here on almost every clear night near the summer solstice. For the last two years special note has been taken of them. In 1887 they were first seen at midnight on June 13, and last seen on July 20; this year their first appearance at midnight was on June 4, and they are still visible every clear night. The clouds are not, as far as I have observed, coloured, but shine with a pearly or silvery lustre. I have seen them at midnight as high as 30° altitude, but they are generally confined to the first 10° or so above the northern horizon. The facts that they vary greatly from night to night in appearance, being sometimes almost absent, and that one or two photographs that have been taken of them show them simply as ordinary cirrus clouds, all seem to indicate that they really are very high cirrus lighted by the sun.

I may add that the upper glows continue to be seen here, though with varying intensity, on every clear night both before sunrise and after sunset, but for the past year no reddish ring or glare has been observed round the sun in the day-time.

Ben Nevis Observatory, July 2. R. T. OMOND.

Micromillimetre.

THE Council and the Fellows in general meeting have taken into consideration the objection raised by Prof. Rücker to the term micromillimetre.

This term was in use by microscopists long before the British Association Committee formulated their system of nomenclature; but nevertheless the Society are unwilling, on a question of precedence only, to insist upon retaining a word which may give rise to confusion.

The Council have therefore directed the editors of the Journal to discontinue the use of the term "micromillimetre," and to substitute for it that of "micron," which has been in use for as long a time as the former word.

This resolution has been confirmed by a general meeting of the Society, who agree with the Council in thinking that the term "micromētre," proposed by Prof. Rücker, would give rise to considerable confusion from its similarity to "micrometer."

FRANK CRISP, Secretary.

Royal Microscopical Society, June 21.

A Prognostic of Thunder.

AMONG prognostics of thunder given in books and elsewhere I have never met with mention of what has for years been to me one of the most trustworthy of weather signs, viz. the formation of *parallel streaks or bars*, definite in form but limited in number, extent, and persistence, appearing chiefly in cirrus and cirro-stratus, but also on the surface (apparently) of nimbus. In cirrus they give often almost the first intimation of coming change after settled weather, and are almost, if not quite, invariably followed within twenty four or thirty-six hours by thunder. When they appear on nimbus the interval is much less, but they are not seen, I think, on the thunder-cloud itself. These small patches of definitely marked "parallel bars" are to be distinguished from the more general parallel arrangement which is often seen on a much larger scale, but which has not, so far as my observation goes, any very distinct value as a weather prognostic.

As the thundery season is now on, it would be interesting to have the observation confirmed by others, and the connection of this particular form of cloud with electric disturbance explained. I have no doubt of the fact, and have often, and several times within the present year, pointed out these "parallel bars" to friends who had never observed them, and hardly ever has my prediction of thunder failed to come true. In the very few cases in which thunder has not followed in the same locality, I think I may say that there have never been wanting instances of its occurrence within a moderate distance. B. WOODD-SMITH.

Branch Hill Lodge, Hampstead Heath, June 29.

Parasites of the Hessian Fly.

ALTHOUGH numbers of these most useful insects were bred last year from puparia of 1886 and 1887, there seemed to be a good deal of doubt among some entomologists as to whether the American species, *Merisus destructor*, had occurred. I bred a large number of various kinds, four of which appeared to me to agree in every respect as to form, colour, and marking with the description given by Prof. Riley.

During the present month (June) I have bred a very large number of this parasite, specimens of which (both male and female) I sent to Dr. Charles Lindeman, of Moscow, who has just replied that "the specimens of parasites sent, bred in England from the Hessian fly, seem to me to be *Merisus destructor* of Riley, &c." He thus fully confirms my opinion of last year, that the American parasite had occurred here. Early in the spring I bred several other parasites which, I am much inclined to think are *Platygaster herrickii* of Riley; and, if this is correct, it strengthens the opinion that part of the attack came from America.

The damp muggy weather appears to be decidedly favourable for the development of "the pest," the larvæ of which I found at the beginning of this week engaged in weakening the stems of barley; and on June 2 I observed a female Hessian fly ovipositing. The number of eggs laid was 158! Truly a most prolific "pest," requiring both natural and artificial means to check its increase. F. E. S.

Fact and Fiction.

As Mr. Grant Allen reads NATURE,—indeed this is evident from a sentence in his novel "This Mortal Coil," now in course of publication in *Chambers' Journal*—he will perhaps be good enough to satisfy my doubts upon the following practical points in electro- and thermo-physics. Firstly, in order to successfully attract a flash of lightning to a tree, is it necessary to bury beneath its roots a Rhumkorf coil? Secondly, do Rhumkorf coils exist which are *without* secondary wires? Thirdly, will an electric discharge ignite commercial petroleum oil?

While it is not undesirable that scientific fact should be imported into modern fiction, it is surely important that it should be fact: loose statements are apt to perpetuate themselves.

Mr. Allen is exceptionally well read and observant, and I am quite at a loss to understand why a simple solution of continuity in that part of his copper conducting wire which was immersed in the petroleum would not as well have served his purpose (if indeed, that purpose could have been effected in the way described), as the elaborate expedient of burying and destroying an expensive piece of apparatus.

Dublin, July. HARRY NAPIER DRAPER.

The Nephridia of Earthworms.

THE number of NATURE published on June 28 last contains (p. 197) an interesting paper by Prof. Baldwin Spencer, which deals with the excretory system of the gigantic Australian earthworm *Megascolides*. Prof. Spencer promises an extended memoir upon the anatomy of this earthworm, which has not hitherto received more attention than a superficial description. In the meantime the paper in NATURE contains an abstract of the results obtained by the author from his investigation of the nephridia.

This paper is particularly interesting to myself, as I am at present preparing an account of some further investigations into the anatomy of the excretory system of earthworms, which will supplement those already published by me in the *Quart. Journ. Micr. Sci.* (January 1888).

It appears from Prof. Spencer's paper that, as he himself points out, there is a considerable resemblance between the excretory organs of *Megascolides* and of *Perichæta aspergillum*, one of the species investigated by me; there are at the same time certain important differences between the two types.

In my paper upon *P. aspergillum* I described only the nephridia of the anterior segments of the body. I have since found that the nephridia of the posterior segments are in some respects different. In both cases, however, the external orifices are more numerous than I was at first inclined to suspect. They are not limited to the area of the segments which lie between the setæ, but are found all over the body, scattered irregularly; *they have, in fact, no relation whatever to the segmentation of the body.*

The tufts of tubules in the posterior segments of the body are not so abundantly developed as in the anterior segments, where they not only form a layer covering the body-wall and septa but occupy nearly the whole of the cœlomic space available.

Again, *they are furnished with numerous ciliated funnels*; I have not detected them in the nephridia of the anterior segments, but they have been possibly overlooked. These funnels are very abundant; for example, I counted five in one section on one side of the body. Some of them are distinctly larger than others; the larger ones were occasionally observed to be connected with a duct which perforated the septum and joined the nephridia of the segment behind.

In the posterior segments there is a distinct tendency for the nephridial system to become broken up into isolated clumps. It by no means always happened that this tendency to segregation was in relation to the metamerism of the body. On the contrary, the tufts are scattered irregularly in the segments; and the intersegmental septa do not always isolate the nephridial tufts which are connected by intraseptal tubules.

In fact the nephridial system of *Perichæta* and *Megascolides* forms a strong support for that view of the origin of the segmented from the unsegmented worms that has been so ably argued by Arnold Lang.

With regard to the ciliated funnels of *Perichæta*, it is right to mention that they have been already observed by Dr. Benham in a species from Luzon, though no description has been published. Prof. Spencer has made the observation that in the posterior region of the body of *Megascolides* there are a pair of much larger nephridia, which are furnished with a ciliated funnel opening into the segment in front of that containing the nephridium. He believes that these have arisen from the smaller nephridial tufts, and that from them are derived the paired nephridia of such earthworms as *Lumbricus*. I am quite disposed to agree with Prof. Spencer with regard to these points. I had already made some observations upon another earthworm which exhibits a closely analogous structure.

In *Perichæta aspergillum*, as I have mentioned above, some of the ciliated funnels are larger than the others, and are connected with a nephridial tuft lying in the segment behind that which contains the funnels. I could not, however, notice a very marked difference in the size of the nephridial tubules themselves.

In another species of *Perichæta*, viz. *P. armata*, which was characterized some years ago (*Ann. Mag. Nat. Hist*, 1883) by myself, the nephridial system is rather different from that of *P. aspergillum*. Mr. W. L. Sclater, of the Calcutta Museum, has kindly sent me some specimens of this worm which were well preserved. The worm has been lately re-described by Dr. D. Rosa (*Ann. Mus. Civ. Genova*, 1888), who states that each segment contains a pair of nephridia, opening internally by a funnel which lies in the segment anterior to that which contains the nephridium. So far Dr. Rosa's description is accurate, but there are also innumerable tufts of minute tubules which may or may not be provided with funnels. *These appear to be for the most part quite distinct from the large pair of nephridia.* The calibre of the tubules of the large nephridia is many times greater than that of the small tufts. The latter open by numerous orifices on to the exterior.

In the present state of our knowledge it appears to me permissible to derive the paired nephridia of *Lumbricus*, &c., from the network of *Perichæta* in two ways, which may both have actually taken place:—

(1) By the gradual development of a pair of large nephridia, in the way suggested by Prof. Spencer, out of the minute nephridial network, and the gradual disappearance of the latter (which is in the process of disappearance in *Perichæta armata*).

(2) By the gradual breaking up of the nephridial network into tufts of tubules specially connected with the setæ, as in *Acanthodrilus multiporus*, and by the disappearance of all but two of these. Dr. Benham's interesting form, *Brachydrilus*, which has two pairs of nephridia in each segment, offers an intermediate condition in this reduction.

To assume that the ordinary condition of the nephridial system of earthworms has been derived in these two ways, renders the mutual affinities of certain earthworms easier to understand. For example, *Perionyx* (which is so nearly allied to *Perichæta* in most respects, but differs in having nephridia of the *Lumbricus* pattern) may have been derived from *Perichæta* directly *via* some such form as *P. armata* without having passed through an "*Acanthodrilus* stage"; again, *Deinodrilus*, which is intermediate in many characters between *Perichæta* and *Acanthodrilus*, is also, as I shall hope to show later, intermediate in the arrangement of its nephridia, and may therefore represent a stage in the evolution of *Acanthodrilus*.

Zoological Gardens, N.W. FRANK E. BEDDARD.

THE "AVOCET" ROCK.

THE circumstances attending the loss of the s.s. *Avocet* and *Teddington* towards the southern end of the Red Sea in the year 1887, and the subsequent finding of the small coral patch on which it is probable they both struck, are of interest, and deserving of record as showing the necessity for very close examination of seas where corals flourish, and the difficulties experienced in finding a small patch at a distance from land, when neither discoloration nor break of sea aid the searcher. It should be premised that the area between the Zebayir Islands and Jebel Zukur, in which this rock lies, had never been properly sounded, only a few scattered depths having been obtained. It is crossed yearly by hundreds of steam-ships—the majority of them British—and has always been accounted as deep, safe water.

On the 4th of March the *Avocet* was steaming southwards—with another steamer, the *St. Oswald*, with which she had kept company for some hours, not far from her—a strong head-wind and heavy short sea prevailing at the time. At about 8 a.m. a shock was felt, succeeded by two others, and shortly afterwards water was found to be coming in. It being evident that the ship would go down, the *St. Oswald* was signalled, and after a little time the crew of the *Avocet* were taken off by her, and the latter sank. A Court was held at Aden, and the evidence taken before it showed that the shock had been slight, one witness stating that he thought something had gone wrong in the engine-room; and another, that it was a heavy sea that had struck the ship. The verdict was that the ship had struck on an unknown rock in latitude 14° 21′ N., longitude 42° 38′ E., the position given by the master. No evidence was given to prove this position; but the fact of the *St. Oswald* being in company, and of other steam-vessels passing on either side of the two ships both just before and just after the accident, seemed to show that they must have been in the straight track, and that the position was not far wrong in longitude at any rate. H.M. surveying-ship *Flying Fish*, arriving at Aden shortly after the inquiry, spent some days on the suspected ground, and found nothing but deep water, over a hundred fathoms being found in the position given.

Those who have the responsibility of the issue of charts for the guidance of navigators may be pardoned if they are extremely sceptical and difficult to convince in the matter of new rocks in the great highways of traffic. So many instances occur of reports which on investigation prove to be erroneous—sometimes in the whole, sometimes in part (as of the position, for instance)—that very good evidence is required before a report, which seems in itself improbable, can be accepted, and one of Her Majesty's ships sent—perhaps from a long distance, and from other important duties—to spend many days in a search. In this case there was no doubt of the ship having foundered; but the cause of the disaster was somewhat doubtful, and her position was unsubstantiated. It was evident, however, that if she had struck bottom it must be a very small rock, as the presence of other vessels prevented the supposition of a wrong course.

The *Avocet* was partly laden with railway iron, she was pitching in a heavy sea, and the evidence of external injury was not convincing. Altogether it seemed more probable that some of this heavy material had fetched way and injured the ship from inside than that a rock could exist in the very track of the heavy trade of the Red Sea. The Admiralty therefore announced that they would order no further search until these points were cleared up, and the Board of Trade consented to order a further inquiry.

The witnesses were collected, and the Court sat on June 10, but before any further proceedings could take place a telegram was placed in the hands of the President announcing that the s.s. *Teddington* had foundered after striking an unknown rock 5 miles north-east of the *Avocet's* rock, or in latitude 14° 23′ N., longitude 42° 42′ 30″ E. This seemed sufficient, and the Court dissolved without any attempt to cross-examine the *Avocet's* officers on her position. The Admiralty telegraphed for a ship of war to proceed from Aden to examine the spot. The *Griffon*, therefore—whose captain had sat on the Court held there, and had concurred in the finding that the *Teddington* had struck on an unknown rock—spent over a week in traversing the area including both positions, sounding and dragging a chain cable suspended from her quarters, but found no sign of shallow water or rock. On her return to Aden, a fisherman announced that he knew the rock, and the *Griffon* returned with him, only to find that his rock was a well-known one 40 miles from the spot required.

Any further action was then suspended until the full report of the *Teddington* disaster was received. The official report of the Court held at Aden was long before it arrived in England, though the protest of the master was received before many weeks.

This stated that the *Teddington* was on her way north, and on June 9, at 6 a.m., she passed 5¼ miles eastward of Abu Ail, where she got a good position and the error of her compass, and thence steered to pass 5 miles east of the position given for the *Avocet* danger; calm, and weather fine. At 8.30 she struck heavily, nothing being seen under the stern, and no land in sight. Course was at once steered to the south-west, into the track of steamers, when the s.s. *Cairo* was met with, and the crew taken off, the *Teddington* foundering shortly afterwards. The master gave his position as in latitude 14° 24′ 30″ N., longitude 42° 42′ 30″ E., or 1½ mile north of the telegraphed position; but cause was afterwards seen to prefer the latter.

A statement was shortly after received from another ship that they passed the *Teddington*, abandoned and low in the water, at a time four hours later than that given for her foundering. This contradiction seemed to require explanation.

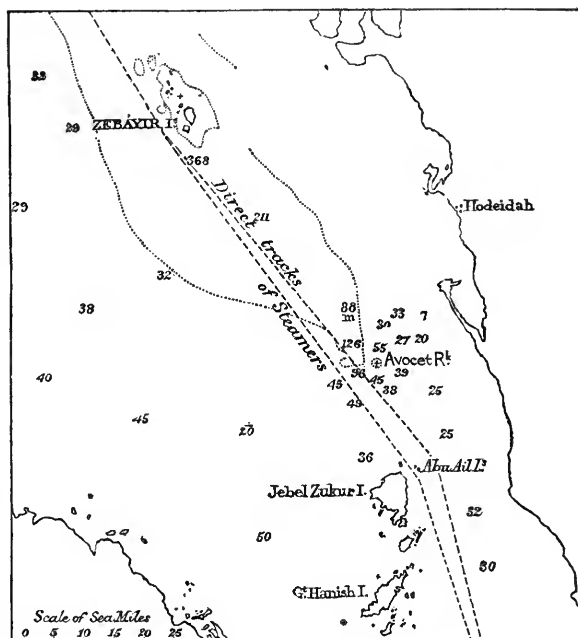
Before the official report arrived, the master of the *Teddington* called at the Admiralty by desire on August 4, and gave his account by word of mouth. His relation was so straightforward, and it was so evident on cross-examination that the ship had been navigated with great care, that it was clear that another and closer search must be made. Captain Free explained that the *Teddington* had been lost sight of in the haze, as the *Cairo* steamed away; and that it was believed she had then sunk.

The position now given, being 5 miles from the straight track that steamers usually endeavour to follow, gave much more probability to the existence of a rock than the *Avocet's* report, which placed it exactly in that direct route. Orders were therefore at once sent to H.M. surveying-vessel *Sylvia*—then in the Mediterranean—to proceed to the spot, and institute a minute search early in October, when the climatic conditions are most favourable to that work.

In September a reply was received to inquiries made of the master of the *St. Oswald* as to the position of his ship when signalled by the *Avocet*. This showed that the *St. Oswald* had found, when Jebel Zukur was sighted, that she was considerably to the eastward of the correct course, and that the position given by the *Avocet* was some four miles in error. The position now given was 14° 21′ N., 42° 41′ E., placing the *Avocet* within 1½ mile of the *Teddington's* danger. This greatly strengthened the evidence, and showed that a general strong cross-set must have existed on the morning that the *Avocet* was lost, sweeping the whole trade to the eastwards.

Unfortunate occurrences delayed the *Sylvia*, and when she arrived on the scene, the strong southerly winds had already set in. Nevertheless, a close search was accomplished, especially of the ground embracing the two best positions of the *Avocet* and *Teddington*, and extending far on either side. Six weeks were spent in this search, but no danger nor considerable shoaling of the water could be found. The heavy sea which caused her to part her cable, carry away anchor stocks, and do other damage, and also placed considerable difficulties in the way of marking the area with beacons, seemed also to afford a means of sighting the rock—had it existed—by the break that would probably be seen on it. When, however, the chart of the search was received, it was noticed that in one spot, nearly midway between the *Avocet* and *Teddington* positions, there was a slight shoaling of the water; a small area of 28 and 30 fathoms existing among the general depths of 35 fathoms. The *Sylvia* had anchored on this, and had commenced to search it carefully with the boats, but the freshening gale drove her from her anchors before the whole area was examined.

The indication afforded by this area, the slope of the sides of which was only a few degrees, was very slight,



but it was evidently necessary to re-examine it before it could be certainly stated that no small danger existed. H.M. surveying-vessel *Stork* was therefore directed to make a fourth search on her way to the East Indies.

Steering out from the mainland to the eastward, the *Stork* struck a depth of 28 fathoms at 8 a.m., April 25; but passing over it, the spot was not again found until late in the afternoon. The ship was then anchored with a light anchor in 26 fathoms of water, and the boats began to search. Just before dusk 6 fathoms was found. The night was luckily fine, and next morning the search was renewed, concluding in finding—not 100 yards from the ship—a small coral mound on which in one spot was a depth of only 15 feet at low-water summer level of the sea. Before, however, the examination was quite complete, the wind suddenly freshened, causing the ship's anchor to drag, and the ship to drift directly towards the rock. To clear this the cable had to be slipped, and the *Stork* thus narrowly escaped passing over the rock that she had just found.

The position of the rock is in latitude 14° 22′ 8″ S., longitude 42° 41′ 32″ E., 18 miles from the island of Jebel Zukur, and the same from the eastern shore of the sea, and out of sight of land except in clear weather, when Jebel Zukur is visible. The dangerous portion of the rock is only about 40 yards in diameter, but the soundings round for about 100 yards give indications of its presence.

Its slope is not so very steep as in some other instances of coral banks in this sea. Assuming that coral after it attains within a certain distance of the surface grows mainly outwards, and that the almost perpendicular sides of some of the Red Sea reefs are mainly the result of such outward growth, the comparatively gentle slope of the *Avocet* rock may be taken to show that it is in an early stage of its development; a view which its small size also supports.

The rock lies on the bank of soundings on the eastern side of the deep-water gully up the centre of the Red Sea, near its edge, and close to the point where it comes to an end. It has frequently been noticed that coral patches most readily form on the edges of such steep submarine slopes—witness other parts of the Red Sea itself—but they generally take the form of a scattered line along such an edge, and it is not usual for one small and isolated patch to alone make its appearance.

This rock is nearly midway between the *St. Oswald's* position for the *Avocet* and the telegraphed position of the *Teddington*, and is about 350 yards from where the *Sylvia* was at one time anchored. It lies about 5½ miles off the direct line between the Abu Ail channel and a point 3 miles west of the Zebayir Islands—the course generally taken by ships.

Seeing that transverse currents are by no means rare in the Red Sea, and also that many vessels—especially when bound north at night—habitually pass outside Abu Ail, it is a cause for marvel that no ship has ever struck this small danger before. One of the telegraph cables passes close to it—so close that it is doubtful on which side it lies, and the ship laying it may therefore be considered to have had a narrow escape. On the very morning of the *Avocet's* loss, a large troopship passed *east* of that vessel an hour before she struck. Evidence is already forthcoming of many ships having been swept to the eastward at different times, so that they must have passed very close to the *Avocet* rock.

The absence of a marked break on the rock is another somewhat curious fact, and shows how a short heavy sea without the accompaniment of an ocean swell can pass over as little water as 15 feet without showing more than the white horses which crown every wave when the wind is strong.

MAGNETIC STRAINS.

It has long been known that when an iron rod is magnetized its length is in general slightly increased. This phenomenon was first studied by Joule about the year 1847, and most of his experimental results have been confirmed by other physicists, among whom may be mentioned the names of Tyndall, Mayer, and Barrett.

Joule enunciated the law that the elongation of a magnetized rod is proportional to the square of its magnetization, a law which seems to have been pretty clearly supported by his experiments so far as they went. Now, when iron is subjected to the action of continually increasing magnetizing force, a point is at length reached when further increase of the force produces comparatively little effect upon the magnetization. The iron is then, in popular language, said to be "saturated," and is (or until lately was) commonly supposed to have attained a condition of magnetic constancy, so that none of the properties of the metal connected in any way with its magnetism would be materially affected by any increase of magnetizing force, however great, beyond what was necessary to produce saturation.

Joule carried many of his observations up to the so-called "saturation point," and then, perhaps naturally, seems to have assumed that nothing would be gained by going any further, and accordingly discontinued his experiments. It is, however, a somewhat remarkable fact that although his interesting discovery was soon widely known, an account of it appearing in almost every text-book dealing with electricity, while an exhibition of the phenomenon in question became a familiar lecture illustration, yet for the thirty-seven years following the publication of Joule's paper it seems never to have occurred to any experimenter to try what would be the effect of subjecting an iron rod to stronger magnetizing forces than those applied by Joule himself. Perhaps I may be pardoned if I refer to the accidental circumstance which led me to do so.

In 1884, a reprint of Joule's scientific papers was issued by the Physical Society, and I then read, for the first time, his original memoir on the effects of magnetism upon the dimensions of iron and steel bars. I had recently been engaged in an investigation of the heat-expansion of sulphur, changes in the length of rods of that substance being indicated by their action upon a small movable mirror which reflected the focussed image of a wire upon a distant scale; and it struck me that a similar method would be well adapted for the exhibition of magnetic expansions. Wishing to have the satisfaction of witnessing some of these effects, I put together a rough apparatus, in which the mirror principle was applied. The battery employed consisted of five large bichromate cells, the zinc plates of which were immersed in the solution by the action of a treadle, and withdrawn by an opposing spring when the pressure on the treadle was removed. The circuit included the magnetizing coil, a galvanometer, and a contact-key.

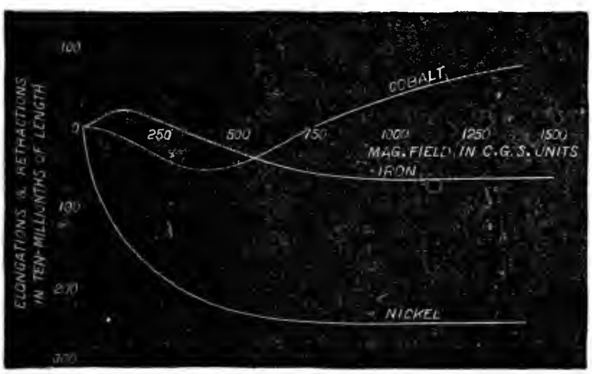
The first results of experiments made with this apparatus were disappointing. Everything appeared to be quite right: the mirror worked perfectly, as was shown by its deflection when the temperature of the iron rod was slightly varied; the iron was well annealed, and there could be no doubt that the magnetizing force used was more than sufficient to "saturate" it (in the popular sense). Yet the elongation indicated when the circuit was closed was only a small fraction of what had been expected, the movement of the focussed index upon the scale being, indeed, scarcely perceptible.

The arrangement was varied in several details, and further attempts were made, but without any better success. In these perplexing circumstances I happened to remove my foot from the battery treadle while the contact key was still depressed, and at the moment of doing so I noticed a curious "waggle" of the focussed image. A movement of the same kind was found upon trial to occur if the zincs were lowered into the liquid while the key was down. The operation was then performed very slowly, and the exact nature of the waggle became clearly revealed. As soon as the zinc plates touched the surface of the liquid the index immediately jumped into a position indicating a certain small elongation of the magnetized rod. As the zincs went in deeper, this elongation at first steadily increased, but only up to a certain point, after which it was *diminished*; and when they were completely immersed in the liquid, the focussed index had returned nearly to the zero position, showing that the elongation had almost entirely disappeared. When the zincs were again slowly raised, the same cycle of changes occurred in inverse order.

The conclusion obviously suggested by these observations was one that could not be readily accepted. It appeared as if the magnetizing force which had been used in the first instance was *too great* to produce Joule's effect, and that it was only when the current was diminished by increasing the resistance of the battery that the elongation of the iron became well developed. This view clearly involved the assumption that the common notions as to magnetic saturation must be at least in part erroneous, and I therefore endeavoured to find some other explanation of the apparent anomaly. In particular, I suspected that it might be due to electro-magnetic action of the kind known as "solenoidal suction" between the iron rod and the coil; but a few careful experiments convinced me that, although this might well have been the case, yet in fact it was not so. Nor did any other hypothesis present itself which would bear examination, and I accordingly fell back upon the first and natural interpretation of the facts, which implies that magnetizing force may exert an important molecular influence upon iron even when its magnetism is saturated.

A fuller investigation of the phenomenon was then made with very delicate apparatus and greater battery power, and the results were communicated during the next year to the Royal Society, the principal conclusion arrived at, so far as regards iron, being the following: When an iron rod is subjected to a continually increasing magnetizing force, its length at first increases to a maximum and then diminishes, ultimately becoming actually *less* than when the rod is unmagnetized.

I have since published accounts of further experiments, and amongst others of a series in which iron rings



surrounded by magnetizing coils were used instead of straight rods. The changes produced by magnetization in the diameter of the rings were of exactly the same nature, showing conclusively that the effects before observed could not have been due to any unexplained action of the ends of the rods.

By the kindness of Mr. W. H. Preece, F.R.S., who placed at my disposal the large secondary battery used in lighting his house at Wimbledon, I have recently been able to repeat some of my experiments with magnetic fields of exceedingly high intensity. Rods of iron, nickel, and cobalt were thus tested, and the results are clearly shown in the accompanying curves, where the abscissæ represent the magnetic fields due to the coil in C.G.S. units, and the ordinates the elongations and retractions of the rods in ten-millionths of their lengths.

The retraction of iron, it will be seen, becomes ultimately greater in amount than its maximum elongation, and reaches a limit in a field of 1000 or 1100 units, after which its curve becomes sensibly parallel to the horizontal axis. Nickel, unlike iron, retracts[1] from the very commencement, rapidly at first and afterwards more slowly, until in fields of 800 units and upwards its length becomes apparently constant. Cobalt behaves in a very remarkable manner. While the field is comparatively weak, no sensible change in either direction can be detected. After about 50 units of magnetizing force, the rod begins to contract, attaining its minimum length with 300 or 400 units. But instead of remaining unchanged in fields stronger than this, it again becomes longer. At 750 it regains its original length, and thence up to 1400, the highest field reached in the experiment, it continues to elongate steadily.

[1] The retraction of nickel under magnetization was first observed by Prof. Barrett (NATURE, xxvi. 585).

It should be understood that so far as mere details are concerned the curves in the diagram relate only to particular specimens of the metals in question. With different rods there will be certain small variations, dependent upon the purity of the metals and their physical condition. But I have always found that under increasing magnetizing force iron is at first extended and then contracted, nickel is contracted from the beginning, while cobalt is first contracted and afterwards extended.

My best thanks are due to Mr. Preece, not only for having given me the opportunity of carrying out the experiments described above, but also for the exceedingly kind and cordial manner in which he did so.

SHELFORD BIDWELL.

A METEOROLOGIST AT THE ROYAL ACADEMY.

ARTISTS and poets are supposed to draw their inspirations from communing with Nature; but it is well known that painters in oil are rarely successful with the cloud portion of their pictures.

For some reason or other, skies and clouds are always far more satisfactory in the water-colour exhibitions than in galleries devoted to oils. The transparency of the former medium enables a painter to put an amount of detail into his clouds which would make the sky far too heavy if attempted in oil; so there is no doubt that oil as a medium is peculiarly unsuitable for the reproduction of cloud-forms, and that the utmost skill is required to give even passable results.

Painters generally do pretty well when they only try to represent shading of the sky, or what Mr. Lockyer has called the zoning of colour in the heavens. They can paint the blue sky overhead gradually getting whiter and grayer as you approach the horizon, or the red round the horizon at sunset surmounted by a zone of orange shading through green into the blue above, as only shade and colour have to be rendered. But when artists try to delineate the form, and, still more, the texture, of clouds, the difficulties are so great that few painters attain excellence in this branch of their work.

Few have yet learnt that, putting the difficulties of the medium aside, the structure of a cloud has an anatomy as definite as that of a man; and that the perspective of cloud-forms obeys the same laws as that of bodies on the earth's surface. Everybody paints ordinary objects so as to show a characteristic texture or structure: the silk dress, the woollen carpet, the wooden floor, are all carefully distinguished; but how many realize the essentially different structure of cloud-forms—the hairy cirrus, the lovely fleecy sky, or the rocky masses of cumulus? Nobody would dare to draw a building, a road, or a tree out of its due perspective; but many seem to think that the forms and distances of the sky can be rendered by daubing white and blue and gray promiscuously over the canvas.

The chapters on clouds in Mr. Ruskin's "Modern Painters" sin against every canon of literature. They are disjointed, discursive, irrelevant, and wander into many by-paths; for one note brings in the causes of the failure of the Reformation in Germany, and the whole ends with a commentary on the nineteenth Psalm. But, in spite of all this, they preach in brilliant and poetic language the two great truths that clouds have distinctive characteristic structures, and that their perspective must be as carefully drawn as that of a building. In one of the illustrations—that of the tower of Beauvais Cathedral in front of a thundery sky—is the finest delineation of cloud in line that has yet been produced; and though Mr. Ruskin's writings have had a powerful influence on contemporary art in England, the following notes on the pictures now hanging on the walls of the Royal Academy will show that much still remains to be done before British artists have exhausted the possibilities of cloud-painting.

The great landscape of the year is undoubtedly Sir J. Millais' "Murthly Moss" (No. 292), and readers will naturally ask, Is the sky good? The answer is, unequivocally, Yes. Our great artist has selected a somewhat rare form of sky, but one which is most useful in giving distance and perspective to a picture. As a whole, the sky is covered with a sheet of thin flat cloud; but, while the top of the picture appears uniform, the sky lower down looks as if it were composed, or made up, of parallel bars, which get thinner and thinner as they approach the horizon.

If a series of disks stretched in a line from nearly overhead to the horizon, at a uniform height of about 10,000 feet, we should see the whole under-surface of those overhead; and progressively less and less, till on the horizon the thin edges of the disks only were visible, like straight bars. This is exactly what happens in Nature when a thin flat sheet of cloud is broken into irregular flakes. Above, there is only visible the flat, formless under-surface, while in the distance the thin edges of the flakes appear more and more like bars. Thus the picture of the sky alone gives instinctively the idea of retreating distance.

Turner, curiously enough, hardly ever painted cumulus, but almost always a coarser form of this flaky sky growing into thinner and thinner bars towards the horizon; and I have seen pictures by Mr. Leader, in which the same device was used for giving distance, with great effect. In Millais' picture the artist has painted the sky with consummate skill, true to Nature, and true to art in not destroying the balance of relative distance.

Another important landscape is No. 102, Mr. G. Boughton's "A Golden Afternoon." But in this the sky is scarcely satisfactory. The clouds are rather spotty, but yet not of the kind which come in flocks of little cloudlets; and it is difficult to make out either the precise form which it is intended to delineate, or the perspective of the whole sky. The reproduction of representative structure is simply nowhere, for at a distance neither form nor structure are discoverable; while close at hand the brush-marks are so apparent that the lower clouds appear to have a fibrous structure. This would be practically impossible, for though the summit of a rocky cumulus is often combed out into hairy cirrus, the rest of the cloud remains firm, and this would not occur on "a golden afternoon."

Mr. Leader can be complimented on sending three first-rate skies in the three pictures which he contributes to the exhibition. In No. 408 he not only paints "An Old English Homestead," but also a truly English sky. A wisp of cirrus floats over a well-painted cumulus, while the ideas of relative height and distance are well given. Cloud forms are essentially the same all over the world, but the details differ; and if the sky in this picture were alone, I could say that it was nowhere in the tropics, but somewhere in a temperate zone. In No. 638, "A Summer Day"—

> "When the south wind congregates in crowds
> The floating mountains of the silver clouds"—

Mr. Leader again paints the same kind of sky very beautifully; but in No. 421, over "The Sands of Aberdovey," he gives a totally different type of cloud. Here the clouds float as a thin white fleece on the sky, with some small raggy, evaporating cloud of a totally different structure at a lower level. The effect is very striking, and the accurate drawing of the forms gives height and distance to the picture.

Mr. V. Cole's "The Pool of London" (No. 350) has been purchased under the Chantrey bequest. This is a large, fine picture, in which the artist has employed a device for giving distance that was sometimes used by Turner. A dark mass of cumulus cloud on either side of the sky leaves a sort of bright vault running down the centre, in which high white clouds lead the eye to the dome of St. Paul's in the distance. The painting of all the clouds, and the effect of their floating at different levels, are very good; but somehow the scale of distance in the picture is scarcely satisfactory. Artists are conventionally allowed to diminish the size of objects in the foreground, and to increase that of distant objects so as to improve the effect; but the modern eye, which is trained to the accurate projection of objects at different distances given by photography, knows that in this picture the ships in the foreground should be bigger, and the Cathedral dome smaller than they are here delineated.

"Then came the Autumn, all in yellow clad," is the poetic title of Mr. G. Lucas's picture (No. 342). A beautiful, finely-painted shower-cloud, in the shape of a rising, driving cumulus, gives such an idea of space and height that it is a pleasure to look at such a truthful transcript of Nature. This is one of the best skies in the exhibition.

Close by, and in great contrast to the above, but fortunately well skied, hangs a small landscape which contains a sky of the worst possible description. White and blue and gray are patched about the canvas promiscuously, regardless of form or drawing or perspective; and the artist seems to consider that any mixture of these colours represents a cloud-covered sky.

Artists do not often break a lance with men of science, but Mr. J. Brett has run a tilt against the astronomers and geologists. One of his pictures of this year is an ambitious subject—"The Earth's Shadow on the Sky: the Rising of the Dusk." A short time after sunset in fine weather, the shadow of the earth appears to rise from the eastern horizon, like the segment of a leaden-gray arch; but there is little to suggest this on Mr. Brett's canvas, though the general effect of the picture is very pleasing. A bright green sea fills up the foreground, then comes a line of gray mist in shadow, with blue hills above; while the zoning of a gilded sunset sky from red through orange to blue is very skilfully handled. But the low mist is more characteristic of sunrise than sunset; and the sky appears to us very bright to be opposite the sun. This artist also shows a well-painted shower-cloud in "A Heavy Squall off the Start Lighthouse," and a confused cumulus in a slightly finished work entitled "The Bristol Channel."

In "Nearing the Needles—Return of Fine Weather after a Gale," Mr. H. Moore exhibits a pretty picture, with a lovely sea and sunlit chalk cliff; but the clouds are not very well defined; and are rather soft for the rear of a gale. The Needles appear to lie to the east of the observer, while the sea and ships appear to be running from south-east. If this is so, the sky has far more the character of a north-west than of a south-east wind. Another of Mr. Moore's pictures—"A Breezy Day in the Channel"—brings into evidence the great difficulty of painting clouds carefully, and yet of maintaining the balance of the picture. Here the clouds—irregular cumulus—are very good in form, and beautifully painted; but this careful work makes them so heavy that they appear rather too near. An artist's scale of distance is to a certain extent a scale of distinctness; so that when clouds are painted in minute detail, it is very difficult not to make them appear too near. The same criticism applies to this painter's "Westward," where another beautiful sky, correct both in form and perspective, is a good deal too heavy.

The low, ill-defined cumulus in Mr. Hook's "Low-Tide Gleanings" are not more finished than the rest of the picture, but are correct both in form and drawing; and the same remarks apply to his work, "A Day for the Lighthouse."

"Thanet Cliffs in the Time of Peace," by Mr. S. Cooper, shows a good cumulus with cirrus overhead; but in Mr. C. Hunter's "Fishers of the North Sea" the cumulus cloud is not satisfactory.

Mists on a mountain, with a gray sky, are very well painted in Mr. Faed's "And with the Burden of Many Years," and make an effective background to a striking work of art; while in "The Approach to Bealloch-na-ba" Mr. H. Davis has delineated mountain mist with equally good effect.

Mr. P. Graham's "A Norfolk River" contains a very good showery sky, but the brush-marks give an appearance of fibrous structure which would not be in Nature; while his "Driven by the Wind" contains an effective mass of gray nimbus or rain-cloud.

Mr. W. Shaw paints a good misty yellow-tinted sky in his "Tide Race"; but the great mass of cumulus behind Sir F. Leighton's central figure of the "Captive Andromache" is not very satisfactory.

The sky in "The Old Water-Way," by T. Liddell, is good so far as form is concerned, but is painted so heavily that the clouds look like clods. Philologists say that the word cloud is really derived from clod, but artists should not express that idea in their works. There is a rainbow in this picture, so ill defined that it is difficult to make out the succession of tints; though I think the red is meant to be outside, which is correct.

Mr. R. Rouse's "Pasture-land in Kent" would be much more pleasing if the clouds were more carefully painted, and not so like patches on the sky. In No. 553, Mr. H. Wells is to be complimented on having painted rays diverging from the sun from exactly the proper kind of sky. These rays are rarely seen except through a peculiar, flat, broken cloud; but they are usually associated with a firmer, harder sky than is here depicted.

Lastly, Mr. C. Johnson paints the "Plain of Arundel" under two well-drawn layers of cloud; and Mr. J. MacWhirter has hit off with great skill and accuracy a flat, broken cloud, lit from below by a setting sun, beside the picturesque castle of "Edinburgh."

Such are some of the more notable skies in our great national exhibition of pictures, and it will be seen at once that the best skies are painted as a rule by those who have achieved the greatest success in the other elements which make up a good picture. May we not therefore fairly conclude that part of their success is due to their faithful rendering of skies and clouds; and that it behoves those who wish to attain a high place among landscape painters to study the form, the structure, and the perspective of those clouds which give life, and height, and distance, to every picture? RALPH ABERCROMBY.

THE OXFORD UNIVERSITY OBSERVATORY.

THE following are the principal parts of the Thirteenth Annual Report of the Savilian Professor of Astronomy to the Board of Visitors of the University Observatory, read June 6, 1888:—

I. *Lectures.*—In addition to the requisite statutable lectures, Prof. Pritchard has offered some others of a more elementary and quasi-public character on descriptive astronomy, and expressed as far as possible in untechnical language. He has been so much encouraged by the interest manifested in these lectures that he proposes to offer another and perhaps more extended series on the recent speculations as to the origin of the Cosmos from meteoric collision and on matters cognate therewith.

II. *Instruments.*—The De La Rue equatorial is in excellent order; its mechanical mounting is now equal to the delicate purposes of stellar parallax to which it has been uninterruptedly applied during the last twelve months. Although the mirror is perhaps somewhat dimmed with age, its figure, which has been recently tested by comparison with the presumed best productions of the day, retains its original very remarkable character.

The two mirrors mentioned in the last Report have been mounted temporarily on the large equatorial for the purpose of the comparison of their photographic action. An efficient electric control contrived by Sir H. Grubb has also been added with the view of securing the great accuracy necessary in the movement of the telescope. The work for which the mirrors were intended having been completed, they have now been dismounted.

Dr. De La Rue having provided the funds necessary for a photographic telescope of 13 inches aperture and of the pattern suggested at the Paris Conference of 1887, the large equatorial has been sent to Sir H. Grubb at Dublin, for the purpose of attaching thereto the instrument in question, and of carrying out the other considerable alterations necessary for the photographic charting of the heavens, as proposed at the aforesaid Conference.

The transit-circle is in perfect order.

III. *Buildings.* — Mr. Nasmyth has presented his magnificent picture map of the moon for the service of the Observatory. This very beautiful work of art (6 feet in diameter) was completed by Mr. Nasmyth from actual observation with a large telescope of his own construction in 1849.

IV. *Astronomical Work.*—During the past year this has been twofold. In the first place continuous attention has been devoted to the photography of small portions of the heavens with the view of determining the parallax of certain selected stars. In the first instance a careful trial of the method was made on the parallax of 61^1 and 61^2 Cygni, because the parallax of the point midway between the two stars had been determined, with presumedly great accuracy, by Bessel in 1838, whereby effective means of comparing the two methods were supplied. The general agreement of the result obtained from photography with that determined by this most able astronomer, together with the remarkable consistency of the individual photographic measurements, satisfied Prof. Pritchard not only of the great convenience, but also of the unimpeachable accuracy of the method. Dr. Pritchard has consequently much extended these operations for stellar parallax, and before the termination of the present year he hopes that the computation of the parallaxes of altogether some ten or twelve stars will be completed. The list will comprise 61^1 and 61^2 Cygni, μ Cassiopeiæ, and Polaris, which four stars may be regarded as already completed. Three more parallaxes have been *provisionally* determined from observations of *six months*,* viz. α, β, γ Cassiopeiæ; four others also are in a forward state. Experience has suggested that these stellar parallaxes will be most readily and efficiently determined by confining the photographic work on each star to those four periods of the year which, in respect of each parallactic ellipse, are the most effective for the purpose. It should be stated that for the purposes of accuracy four stars of comparison are selected, instead of the two with which astronomers have hitherto been generally contented. This photographic process enables Prof. Pritchard also, without much consumption of time, to measure from night to night the distance between the stars of comparison themselves, thus furnishing a check to the unavoidable variability of the scale of the focal field and of the photographic film. These operations are at present restricted to a systematic catalogue of stars of the second magnitude. It appears that astronomical work like this is well adapted to an Observatory connected with a great

University. It may be interesting to record the results of the computations so far obtained, viz.:—

61[1] Cygni	0″·4289	±0″·0180	α Cassiopeiæ	0″·072	±0″·042*
61[2] Cygni	0·4353	±0·0152	β Cassiopeiæ	0·187	±0·039*
μ Cassiopeiæ	0·0356	±0·0250	γ Cassiopeiæ	<0·05	±0·047*
Polaris	0·052	±0·0314			

The last result is peculiarly interesting, as it seems to furnish an instance where the resources of modern astronomy have arrived at the limits of their present possibility. The total number of plates taken for the purposes of the above investigation is approximately 700, and each plate has been measured with 120 bisections of the necessary stars, amounting altogether to about eighty-four thousand observations. Independently and concurrently with the preceding work Dr. Pritchard undertook for the Photographic Committee of the Royal Society the examination of two silver on glass mirrors of the same aperture but of very different focal lengths, with the view of ascertaining the practical effects of focal length on the photographic field. This work, owing to the temporary character of the mounting and the imperfection of the mechanical movement of the telescope, has been attended with great labour and personal endurance on the part of the observer, but at length it was brought to a successful conclusion, and the results have been communicated to and printed by the Royal Society. The expenses of the instrumental appliances connected with this investigation have been defrayed partly from a grant from the Royal Society, and partly by the generosity of Dr. De La Rue, to whom this Observatory owes so much, not only in the matter of pecuniary aid, but by his kindly encouragement and appreciation of our labours. The general result of the investigation alluded to above is the comparative unsuitability of any mirror for an extensive charting of the heavens, and particularly as regards mirrors of short focal length; but at the same time it leaves no doubt as to their capacity for the singularly accurate delineation of small portions of the heavens, and for such operations as those connected with stellar parallax, or the charting of the moon. Preparations were made for the necessary observations of the lunar eclipse of January 28 of this year; but, as was the unfortunate case with this and many other Observatories, they were rendered ineffectual by a clouded sky.

The above astronomical operations made under Dr. Pritchard's direction were skilfully and sedulously carried out by the two Observatory assistants, Mr. Plummer and Mr. Jenkins.

NOTES.

We learn that Dr. Guppy left England for Batavia on the 30th ult. with the intention of spending some time in the examination of the living and upraised coral reefs of the Indian Archipelago. Mr. John Murray has provided the necessary funds for the first six months of his sojourn in that region, and has directed Dr. Guppy in the first place to make as complete an examination as he can of the geological structure of Christmas Island. Judging from the important notes and collections made by Captain Aldrich and Mr. Lister during the recent visit of H.M.S. *Egeria*, this island would seem to be one of the oldest of the upraised coral islands, and as such it is likely to prove of considerable geological interest. At the last meeting of the Geographical Society, Captain Wharton, the Hydrographer, read a short paper on this subject.

Apparently we have missed our chance of solving the many interesting problems relating to the Antarctic regions. The matter has now been taken in hand by Germany, and we may be sure that she will not fail to carry out the enterprise in an energetic and thoroughly scientific spirit. The expedition is being organized by Dr. Neumayer, of the Hamburg Observatory.

Mr. Jesse Collings is to be congratulated on the result of his efforts to secure for the parish of West Lavington, Wiltshire, the full benefit of the Dauntsey Charity, a part of which the Charity Commissioners proposed to use for the establishment of a High School in some other place in Wiltshire. It is now proposed—with the approval of the Mercers' Company, the principal trustees and patrons of the Charity, who have agreed to undertake a liability of £60,000—not only that the children of the poorer inhabitants of West Lavington shall be provided with an ordinary elementary education, but that a fully-equipped Lower School for technical training in horticulture and agriculture shall be created for their benefit. It is intended that the latter school shall be adapted to the needs of persons who cannot afford to attend such institutions as those at Cirencester and Downton. If the scheme is carried out, land will be provided for the more thorough instruction of pupils, and classes will be formed for the teaching of the various sciences and arts which especially relate to agriculture.

On July 16, Prof. W. E. Ayrton will begin, at the City and Guilds of London Institute, a course of six lectures (to be delivered on Mondays, Wednesdays, and Fridays) on the construction, testing, and use of electrical measuring instruments. This course will include experimental lectures and special laboratory work. The lectures will comprise the principles and practice of the construction, calibration, and testing for faults of ammeters, voltmeters, ohmmeters, wattmeters, coulombmeters, and ergmeters as used for direct and alternating current systems. The students' practical work will be conducted in a laboratory specially fitted with accumulators, standard instruments, &c., for electrical instrument testing; and they will have the opportunity of examining and practically trying all the more important electrical meters at present in ordinary use.

An interesting Exhibition of hygiene and life-saving apparatus has been opened in the Park Léopold at Ostend. The exhibits are divided into the following sections:—Applications of geological, meteorological, and medical science to hygiene, industrial hygiene, maritime hygiene, domestic hygiene, hygiene of infancy, publications relating to hygiene, and life-saving apparatus.

At Messrs. Stevens' Sale Rooms on Monday, the 25th ult., a specimen of *Papilio caunus* from Assam was sold for £10. Mr. William Watkin, of Croydon, was the purchaser.

At the meeting of the Scientific Committee of the Royal Horticultural Society on June 26, Prof. Church contributed a summary of his highly interesting and important researches upon the presence of aluminium in the ashes of plants. This substance, instead of being peculiar to the species of Lycopodium, as once supposed, is found in minute traces in the ashes of very many others, a circumstance not to be wondered at, considering the abundant distribution of the element in many soils. It occurs in all the species of Lycopodium examined, except those which are of epiphytic habit, and which, consequently, do not directly derive their food from the soil. It does not occur in the allied genus Selaginella. It occurs in the ashes of some tree ferns in large proportions, sometimes forming as much as 20 per cent. of the ash, as in *Alsophila australis*, *Cyathea medullaris*; while from others it is all but absent. In the British species of ferns little or no alumina has been found.

At the same meeting Mr. McLachlan called attention to the notion that cold winters are injurious to insects—a notion he stated to be erroneous, although, no doubt, severe alternations of cold, heat, drought, or moisture, were prejudicial to insect life. During the present season it was noticed generally that great destruction of foliage occurred from caterpillars which destroyed the succulent portions of the leaf and tied the framework and fragments together by a web of fine threads comparable with spiders' webs. These caterpillars were different in different cases. In the oak they were species of Tortrix; in the apple the winter moth was destructive; while in other cases the larva of the Ermine moth was exceedingly hurtful to leaves.

THE *American Meteorological Journal*, desiring to attract the attention of students to tornadoes, in hopes that valuable results may be obtained, offers the following prizes:—For the best original essay on tornadoes or description of a tornado, 200 dollars will be given; for the second best, 50 dollars. Among those worthy of special mention 50 dollars will be divided. The essays must be sent to either of the editors, Prof. Harrington, Astronomical Observatory, Ann Arbor, Michigan, or A. Lawrence Rotch, Blue Hill Meteorological Observatory, Readville, Mass., U.S.A., before the first day of July, 1889. They must be signed by a *nom de plume*, and be accompanied by a sealed envelope addressed with the same *nom de plume* and inclosing the real name and address of the author. Three independent and capable judges will be selected to award the prizes; and the papers receiving them will be the property of the journal offering the prizes. A circular giving fuller details can be obtained by application to Prof. Harrington.

THE United States Congress has been discussing the question whether the Weather Bureau should be transferred to the proposed new Department of Agriculture. *Science* advocates the maintenance of the existing system. "The observations," it says, "upon which the Weather Bureau bases its calculations are now all made by enlisted men of the army, who have been specially instructed and trained for the work. No political influence whatever has been allowed to operate for their appointment, promotion, or retention in the service. It has been the aim of the Chief of the Signal Office to send to all important stations men who will be acceptable to the communities in which they are to live and do their work, but no member of Congress has been able to secure the transfer or removal of an observer sergeant in order that some favourite might be put in his place. The security which the observer sergeants have felt for the terms of their enlistment has certainly had a beneficial effect upon the character of the service they have rendered. It may seem an anomaly to the people that a duty that is in no respect of a military character should be done by soldiers rather than by civilians, but the military organization of the Weather Bureau has certainly resulted in keeping political influence from dictating in regard to the *personnel* of a class of men whose appointment and promotion it was very desirable to keep free from this influence."

THE Report of the Director of the Hong Kong Observatory for 1887 shows that the meteorological inquiries are being pushed on with vigour, and that the amount of information collected respecting the typhoons of the past year has been much greater than in previous years. Some of these results have been published in an appendix on the "Results of Further Researches concerning Typhoons"; and another work on the subject, with maps exhibiting the paths of the typhoons, is in preparation. This investigation will throw light on the cause of the frequency of these storms in the China Sea in September, and will enable masters of vessels to escape damage from them, and to make quicker voyages.

WE have received from Dr. Hellmann a very comprehensive and careful discussion of the rainfall of the Iberian Peninsula, being an excerpt paper from the Berlin *Zeitschrift der Gesellschaft für Erdkunde*, vol. xxiii. The principal results of the investigation were communicated to the Berlin Meteorological Society in January last (see NATURE, vol. xxxvii. p. 312). Dr. Hellmann, to whom we are indebted for many laborious inquiries, took advantage of his stay in Andalusia, in 1875–76, to collect all available materials, but found them insufficient for trustworthy results; the present discussion has therefore been delayed until the observations of ten more years could be added. The work deals with the monthly and yearly values for sixty-seven stations, for which a sufficiently long series could be got, and contains a map showing the yearly distribution of rainfall. The yearly and daily periods of rainfall, the monthly and yearly extremes, and the frequency, are also all fully and ably discussed. The annual fall is very various, being no less than 138 inches on the Serra da Estrella, and as little as 11 inches at Lérida, in Catalonia. In the yearly period the minimum fall at all stations occurs in July and August, and the maximum, generally speaking, about May or October, according to locality. Snow falls only in a few of the more elevated districts.

THE vapour-density of sulphur has been re-determined by Dr. Biltz in the laboratory of Prof. Victor Meyer, with unexpected results. It has hitherto been generally accepted that at a temperature (524° C.) not very far removed from its boiling-point (447° C.) the molecule of sulphur is built up of six atoms. This assumption is based upon vapour-density determinations by Dumas and Mitscherlich, who obtained values about this temperature pointing to a hexatomic molecule. However, the work of the last few years upon the chlorides of aluminium, tin, and iron, has opened the eyes of chemists to the fact that the double formulæ Al_2Cl_6, Sn_2Cl_4, and Fe_2Cl_6, resting as they did upon a few experiments performed within a very limited range of temperature, are erroneous, and have no foundation in fact. The older work upon the constitution of sulphur molecules was notably of this class. The experiments themselves were irreproachable, and completed with all the skill for which the experimenters were famous; but unfortunately the temperatures at which they worked were not sufficiently removed from each other, there being only a difference of 27° C. between their maxima and minima. It is now, moreover, a demonstrated law that the existence of molecules of fixed composition can only be assumed when the vapour-density remains constant within a notable interval of temperature. Hence a series of fresh determinations have been undertaken in the case of sulphur. Experiments conducted at 518° in a bath of vaporized pentasulphide of phosphorus by Dumas's method gave values averaging about 7·0, which are nearly coincident with Dumas's own. At the higher temperature of 606°, using a bath of stannous chloride vapour, the density had diminished to 4·7. At 860°, as is well known, sulphur vapour attains the normal constitution of two atoms to the molecule, and the density remains constant for about 200° higher still. Hence, in order to finally set the question at rest, a series of ten determinations were made at intervals of about 10°–15° from 468° to 606°, with the conclusive result that the density regularly diminished from 7·9 at the former to 4·7 at the latter temperature. Hence the notion of S_6 is completely dissipated; there is no more experimental reason for it than there is for the existence of molecules of the constitution S_5 or S_n. None but the value corresponding to the normal composition, S_2, stands the test of interval of temperature, therefore we must conclude that sulphur obeys the usual law, and that its molecules when completely vaporized are each composed of two atoms.

Science says that the logs from the great raft abandoned off the coast of New England a few months ago have drifted in a direction about east by south, and that the greater part of them are now in the region between the 33rd and 38th parallels and the 30th and 50th meridians. The reports lately received at the Hydrographic Office would seem to show that the general drift of the logs has been about east by south, and that most of them are now west-south-west from the Azores. Very few, if any, have drifted north of the 40th parallel. A great deal of timber has been reported further north, to the westward of the 20th meridian, but, from the descriptions given, it does not seem to be a part of the great raft.

In the twenty-first Annual Report of the Provost to the Trustees of the Peabody Institute, Baltimore, it is stated that whereas the number of readers during the past year declined, the number of books used increased. Thus the library "is being gradually converted into that real reference library for scholars which its founder intended to establish." A table included in the Report gives some interesting and suggestive information as to the subjects studied. Antiquities, philology, and theology seem to be the most popular subjects. On the first of these subjects 2894 volumes were read; on the second, 2336; on the third, 2212. Biography comes next; but there were readers for only 1,964 volumes under this heading.

The Register, for 1887-88, of the Johns Hopkins University of Baltimore has been sent to us. In an introductory statement it is explained that this University was opened in 1876; that thus far the Faculty of Philosophy has alone been fully organized; and that the formation of a Medical Faculty has been begun, and will soon receive further development. In the Faculty of Philosophy, instruction is carried on by University methods and by Collegiate methods corresponding with the requirements of students at different stages of their advancement. University instruction is offered to those who have already taken an academic degree, or who have otherwise fitted themselves to pursue advanced courses of study.

From the Report, just issued, of the trustees of the South African Museum for the year ended December 31, 1887, we learn that the condition of the collection generally has been satisfactorily maintained by dint of regular and frequent inspection. The donations during the year numbered 3125 specimens, presented by 78 donors, as compared with 1298 specimens, presented by 58 donors, in 1886. The trustees make an urgent appeal for the extension of the Museum buildings. "Each year," they point out, "has of necessity increased the overcrowding of the very limited available space, and this has now become a most serious hindrance to the usefulness of the Museum, and indeed an absolute barrier to its due development. The trustees have been disappointed to find that their repeated written representations on this important matter failed to meet with the favourable consideration of the Government, as they have thus been placed in the highly unsatisfactory position of inability to promote the normal growth of the institution, or even to insure the proper preservation of much of the valuable public property for which they are trustees."

The annual reports of the Aëronautical Society of Great Britain for the years 1885-86 have been issued in one small volume. Among the contents are the following papers, read at the annual meeting of the Society on December 11, 1886:—Gravity and wind-pressure on auxiliary powers in flight, by Sidney Hollands; balloon-signalling in war, by Eric Bruce; experimental ballooning, by F. W. Breary; an aërial boat, by Mr. Green; and jet-propulsion for aëronautical purposes, by Captain Griffiths.

We have received No. 5 of the first volume, fourth series, of the Memoirs and Proceedings of the Manchester Literary and Philosophical Society. It contains the following memoirs:—Descriptions of twenty-three new species of Hymenoptera, by P. Cameron; a survey of the genus Cypræa (Linn.), its nomenclature, geographical distribution, and distinctive affinities, with descriptions of two new species and several varieties (with two plates), by James Cosmo Melvill; a catalogue of the species and varieties of Cypræa, arranged on a new circular system, in accordance with true sequence of affinity, by James Cosmo Melvill; memoir of the late Prof. Balfour Stewart, F.R.S., by Prof. A. Schuster, F.R.S. To the last of these memoirs a list of the titles of papers by Prof. Balfour Stewart is appended.

A sixth edition of Mr. William Ford Stanley's "Mathematical Drawing and Measuring Instruments" (E. and F. N. Spon) has just been issued. It contains descriptions of twenty-five new instruments mounted or brought out since the publication of the fifth edition ten years ago. Among the instruments invented by the author himself is the oograph, designed for the purpose of enabling oologists to draw eggs of birds in their natural sizes and proportions.

A useful little volume on "Landscape Photography," by Mr. H. P. Robinson, has been issued as one of the series of "Photographic Handy-Books" (Piper and Carter). It consists of letters written to a friend "whose study of photography enabled him to produce a technically perfect negative, but who did not know how to put his knowledge to pictorial use." "They were not intended," the author explains, "to point out a royal road to art, but rather to act as a stimulus to activity in the search for subjects for the camera, and to teach how readiness of resource may help good fortune in turning them into agreeable pictures."

An interesting pamphlet on Pallas's sand grouse, by Mr. W. B. Tegetmeier, has just been issued (Horace Cox). It is illustrated with a coloured plate and woodcuts. "It is greatly to be regretted," says the author, "that a bird so beautiful in its form, harmless in its habits, valuable as an article of food, interesting to the sportsman as a game bird, and to the naturalist as the type of a most singular genus, should not be protected. The present pamphlet has been compiled as an endeavour to make the bird better known, to interest the public at large in the species, and thus, if possible, to aid in its preservation and naturalization as a British game bird."

Science states that Mr. William Walter Phelps has introduced into Congress a Bill to purchase from Stephen Vail, of Morristown, N J., the original telegraphic instrument, or recording receiver, invented by his father, Alfred Vail, and used upon the first telegraphic line ever constructed,—that between Washington and Baltimore,—and to transmit the first message ever sent: "What hath God wrought?" The purchase of this instrument is strongly recommended by the officers of the Smithsonian Institution. The price is ten thousand dollars.

According to an official notification of the Trustees of the Schwestern Fröhlich Stiftung at Vienna, certain donations and pensions will be granted from the funds of this charity this year, in accordance with the will of the testator, Miss Anna Fröhlich, to deserving persons of talent who have distinguished themselves in any of the branches of science, art, or literature, and who may be in want of pecuniary support, either through accident, illness, or infirmity consequent upon old age. The grant of such temporary or permanent assistance in the form of donations or pensions is, according to the terms of the foundation deed, primarily intended for natives of the Austrian Empire, but foreigners of every nationality—English and others—may likewise participate, provided they are resident in Austria. Information as to the terms and conditions of the foundation deeds, &c., may be obtained from the Austro-Hungarian Embassy in London.

The additions to the Zoological Society's Gardens during the past week include two Tasmanian Wolves (*Thylacinus cynocephalus*), two Bennett's Wallabys (*Halmaturus bennetti*), a Black and Yellow Cyclodus (*Cyclodus nigro-luteus*) from Tasmania, nine Silky Bower Birds (*Philonorhynchus violaceus*) from New South Wales, ten Laughing Kingfishers (*Dacelo gigantea*), ten Blue-cheeked Parrakeets (*Platycercus cyanogenys*), two Cereopsis Geese (*Cereopsis novæ-hollandiæ*), seven Maned Geese *Bernicla jubata*), two Black-backed Piping Crows (*Gymnorhina tibicen*), two Lace Monitors (*Varanus varius*), two Gould's Monitors (*Varanus gouldi*), a Gaimard's Rat Kangaroo (*Hypsiprymnus gaimardi*) from Australia, deposited; a Smooth

Snake (*Coronella lævis*), European, presented by Mr. Walter C. Blaker; a Dark-Green Snake (*Zamenis atrovirens*) from Dalmatia, an Æsculapian Snake (*Coluber æsculapii*), European, purchased; two Triangular-spotted Pigeons (*Columba guinea*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

AMERICAN OBSERVATORIES.—The Trustees of the Lick bequest formally made over the Lick Observatory to the University of California on June 1. The staff of the Observatory consists of Prof. Holden, Director; Messrs. Barnard, Burnham, Keeler, and Schaeberle, astronomers; and Mr. Chas. B. Hill, librarian and assistant-astronomer.

The Lick Observatory is not to be the most elevated of American Observatories. Mr. H. B. Chamberlin, of Denver, Colorado, is providing the University of that city with a new equatorial refractor of 20 inches aperture. The site chosen for the erection of this telescope is 5000 feet above sea-level, some 800 feet higher than the Lick Observatory.

Mr. W. R. Brooks, so well known for his cometary discoveries, has removed to the Observatory provided for him by the generosity of Mr. William Smith, of Geneva, New York. His present address is therefore "Smith Observatory, Geneva, N.Y."

The instruments of the Dearborn Observatory, Chicago, have been dismounted, and the old site abandoned, and a new building is to be erected at Evanston, about 16 miles north and 3 miles west of the old site, and some 250 feet from the shore of Lake Michigan, on grounds belonging to the North-Western University, with which institution the Observatory is in future to be connected, but without affecting its relationship to the Chicago Astronomical Society. The new building, which is to cost about £5000, and which will include a dome and tower for the 18½-inch refractor, a transit-room, library, and about eight other rooms, is the gift of Mr. James Hobbs.

Rochester, New York, has no fewer than seven Observatories, of which the Warner Observatory is the most important.

MINOR PLANETS.—The object discovered by M. Borelly on May 12 has proved to be Sironia, No. 116; the difference between the observed and predicted places being due to the omission of perturbations in the computation of the ephemeris. Herr Palisa's discovery of May 16 thus remains No. 278 as given in NATURE, vol. xxxviii. p. 89, at first. No. 272 has been named Antonia; No. 274 Philagoria.

THE RINGS OF SATURN.—Dom M. Lamey, Director of the Observatory of the Priory of St. John, Grignon, claims to have discovered four new rings around Saturn, outside those previously known. The first of these rings is said to commence at the extreme edge of that now known as the outer ring; the next reaches to the orbit of Enceladus; the third, which is the brightest, touches the orbit of Tethys; whilst the fourth and faintest lies between Dione and Rhea.

The distances from Saturn of the known rings have been measured by M. Perrotin, at Nice, with the following results:—

	Outer limit.	Cassinian division.	Dark ring. Outer limit.	Dark ring. Inner limit.
E. Ansa ...	11″·22 ...	8″·50 ...	4″·08 ...	1″·46
W. Ansa ...	11·12 ...	8·43 ...	4·07 ...	1·41

with an average probable error for each determination of ± 0″·02. These results agree well with those of Profs. O. Struve and Meyer, except in the case of those in the last column. The distances in the E. ansa appeared almost always greater than those in the W. ansa for the two outer points measured, but the measures of the dark ring are sometimes greater on one side, sometimes on the other. This is probably due to the revolution of the perisaturnium of the dark ring, which would appear to revolve round the planet in an elliptic orbit. The dark line known as Encke's division has not been seen in 1888, though seen in previous years; but on the other hand the inner part of the ring B has shown three faint divisions separating it into three nearly equal parts. The dark ring has appeared of a uniform tint, and no division has been detected in it.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 JULY 8–14.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on July 8

Sun rises, 3h. 56m.; souths, 12h. 4m. 51·1s.; sets, 20h. 14m.: right asc. on meridian, 7h. 12·3m.; decl. 22° 25′ N. Sidereal Time at Sunset, 15h. 23m.

Moon (New on July 9, 6h.) rises, 3h. 16m.; souths, 11h. 25m.; sets, 19h. 36m.: right asc. on meridian, 6h. 32·5m.; decl. 21° 11′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	4 30 ...	12 7 ...	19 44 ...	7 14·7 ...	17 35 N.
Venus.....	3 46 ...	12 1 ...	20 16 ...	7 8·8 ...	23 12 N.
Mars......	13 5 ...	18 18 ...	23 31 ...	13 26·5 ...	9 58 S.
Jupiter....	16 6 ...	20 30 ...	0 54*...	15 39·0 ...	18 38 S.
Saturn....	5 42 ...	13 30 ...	21 18 ...	8 38·2 ...	19 9 N.
Uranus ...	12 1 ...	17 41 ...	23 21 ...	12 49·8 ...	4 39 S.
Neptune..	1 6 ...	8 52 ..	16 38 ...	3 58·7 ...	18 51 N.

* Indicates that the setting is that of the following morning.

Comet Sawerthal.

July.	h.	Right Ascension. h. m.	Declination. ° ′
8 ...	0 ...	1 5·2 ...	49 22 N.
12 ...	0 ...	1 6·7 ...	50 10

July.	h.	
9 ...	5 ...	Mercury in conjunction with and 3° 34′ south of the Moon.
9 ...	5 ...	Venus in conjunction with and 1° 57′ north of the Moon.
9 ...	5 ...	Mercury in inferior conjunction with the Sun.
9 ...	6 ...	Mercury in conjunction with and 5° 32′ south of Venus.
9 ...	— ...	Partial eclipse of the Sun: not visible in Europe.
10 ...	20 ...	Saturn in conjunction with and 0° 1′ north of the Moon.
11 ...	19 ...	Venus in superior conjunction with the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4 ...	81 16 N. ...	July 10,	21 52	*m*
Algol	3 0·9 ...	40 31 N. ...	,, 11,	0 42	*m*
U Monocerotis ...	7 25·5 ...	9 33 S. ...	,, 13,		*m*
R Crateris	10 55·1 ...	17 43 S. ...	,, 11,		*M*
δ Libræ	14 55·0 ...	8 4 S. ...	,, 13,	1 10	*m*
U Coronæ	15 13·6 ...	32 3 N. ...	,, 9,	0 1	*m*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,, 14,	1 18	*m*
Z Sagittarii... ...	18 14·8 ...	18 55 S. ...	,, 12,	1 0	*M*
R Scuti	18 41·5 ...	5 50 S. ...	,, 10,		*M*
S Sagittæ	19 50·9 ...	16 20 N. ...	,, 9,	1 0	*M*
X Cygni	20 39·0 ...	35 11 N. ...	,, 10,	23 0	*M*
T Vulpeculæ ...	20 46·7 ...	27 50 N. ...	,, 10,	22 0	*M*
			,, 11,	23 0	*m*
W Cygni	21 31·8 ...	44 53 N. ...	,, 8,		*m*
δ Cephei	22 25·0 ...	57 51 N. ...	,, 7,	0 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near 102 Herculis ...	271° ...	21° N. ...	Very slow.
	283 ...	14 S. ...	,,
,, π Cygni	330 ...	35 N. ..	Swift. Red streaks.
	352 ...	38 N. ...	Swift.

ELECTRICAL NOTES.

PROF. NICHOLS, of the Cornell University, has suggested the use of carbon and copper combined to form a compensated resistance standard. The resistance of metals increases with temperature, but that of carbon diminishes. The movement of copper is + 0·384, that of carbon − 0·0235 per cent. per degree Centigrade. For every ohm of carbon, 11·544 ohms of copper are needed to secure complete compensation for temperature.

Prof. Nichols electroplates a strip 1 millimetre wide of the carbon rod parallel to the axis with copper to the required thickness. The influence of temperature up to 100° C. is then entirely imperceptible.

MESSRS. GLAZEBROOK AND FITZPATRICK have once more utilized the resources of the Cavendish Laboratory to determine the specific resistance of mercury, and therefore the value of the ohm (10^9 C.G.S. units of resistance). The result, together with the most recent determinations, is given in the following table:—

Observer.	Date.	Value of Siemens unit in B.A. units.	Value of ohm in centimetres of mercury at 0°.
Lord Rayleigh and Mrs. Sidgwick ...	1883	0·95412	106·23
Mascart, Nerville, and Benoit... ...	1884	0·95374	106·33
Strecker	1885	0·95334	106·32
L. Lorentz	1885	0·95388	105·93
Rowland	1887	0·95349	106·32
Kohlrausch	1888	0·95331	106·32
Glazebrook and Fitzpatrick	1888	0·95352	106·29
Wuilleumier	1888	0·95355	106·27

The specific resistance of mercury at 0° C. is therefore 95352 C.G.S. units.

THE mean of the values in centimetres of mercury—106·3—omitting Lorentz's, must be considered a very close approximation to the true ohm. We thus have

B.A. unit	104·808 cm.
Legal ohm	106
Ohm	106·3

The B.A. unit is thus 1·347 per cent. wrong.

WHAT is the specific resistance of pure copper? is a curious question to ask in 1888, but Mr. G. P. Prescott asks it in the *Electrical Engineer* of New York. He points out that Ayrton gives it as 1·599, and Stewart and Gee 1·616, legal microhms, at the same temperature, 0° C. He also shows that Matthiessen and Jenkin did not agree; they differed 2·3 per cent. Messrs. Glazebrook and Fitzpatrick, who have done such good work with mercury, might well turn their attention to copper. It is well known that Matthiessen's standard for pure copper is wrong. It was one English standard mile of pure annealed copper wire 1/16 inch in diameter at 15°·5 C., having a resistance equal to 13·59 B.A. units. It is a common thing to get copper giving better results than this.

THE magnetic elements for 1887 as determined at Greenwich were—

Mean declination	17° 47′ W.
Mean horizontal force	181·75
Mean dip	67° 26′ 20″

Why does the Astronomer-Royal retain British and metric units when nearly all the world uses C.G.S. units?

THE MICRO-ORGANISMS OF AIR AND WATER.

EVER since the great importance of micro-organisms in the economy of Nature was pointed out by Pasteur now some twenty-five years ago, the presence of these minute living forms in the two great fluid media—air and water—with which we are surrounded, has formed the subject of elaborate investigations. As these investigations are thus co-extensive with the period during which micro-organisms have been made the subject of careful study, a review of them becomes particularly instructive as illustrating the gradual development of the methods of bacteriology from the earliest times up to the high degree of perfection to which they have attained at the present day.

It was Pasteur himself who first instituted a systematic inquiry into the presence and distribution of micro-organisms in the atmosphere in connection with his well-known researches disproving the spontaneous generation of life. The experiments which he undertook for this purpose are as remarkable for their extreme simplicity as for the striking results which they yielded. Thus the apparatus with which Pasteur set about exploring the distribution of microbes in the air consisted simply of a number of small flasks, each partially filled with a putrescible liquid such as broth; the necks of these flasks were drawn out and sealed before the blow-pipe whilst the fluid contents were in active ebullition. The flasks thus prepared were both vacuous and sterile, and could be preserved for an indefinite length of time without the contained liquid undergoing change. A number of these flasks were then momentarily opened in various places—in Paris, in the open country, at various altitudes in the Jura Mountains, and at an elevation of 6000 feet at the Montanvert, near Chamonix. Each flask on being opened became instantly filled with the air of the place in question, whilst, by sealing the flask directly afterwards, the further access of air was prevented. On preserving these flasks which had been thus opened, the liquid of some was found to become turbid and lose its transparency owing to the development of bacterial life within it, whilst in others it remained perfectly clear and translucent. It was further observed that the proportion of flasks becoming so affected varied greatly according to the places where they had been exposed. Thus, of twenty flasks exposed in the open country near Arbois, eight developed living organisms; of twenty opened on the lower heights of the Jura Mountains, five became affected; whilst of the twenty opened at the Montanvert, close to the Mer de Glace, only one broke down. The proportion of flasks which became affected on being similarly exposed in Paris, was considerably greater than in the case of the experiment made at Arbois.

The results of these simple experiments thus convey a most vivid picture of the great density of microbial life in the air of towns, and of its attenuation in the higher regions of the atmosphere, although they can give no account of the *actual* numbers present in the air under examination.

Miquel and Freudenreich [1] made the first step in the quantitative estimation of aërial microbes by aspirating air through plugs of glass-wool, thus taking advantage of a fact long known—that it is impossible for micro-organisms to pass through sufficiently tightly constructed plugs of such materials.

Without entering into a detailed account of this method, the merits and demerits of which have been fully discussed by German investigators, it is sufficient to state that a very large number of experiments have been carried out by the authors which can lay claim to a fair degree of accuracy. However, since solid nourishing media for the cultivation of micro-organisms were introduced by Koch, the importance of substituting the latter for the liquid media hitherto exclusively employed has led experimenters to devise processes which shall render their use possible in the examination of air.

The advantages possessed by solid over fluid media are very great, for whereas in fluid media, such as broth, the organisms are in no way restricted in their movements, and their multiplication can take place indiscriminately throughout the entire liquid, on the other hand, if they are introduced into gelatine-peptone which has been first melted, they can be evenly dispersed throughout the culture-material by gentle agitation, and by subsequently allowing it to solidify they are not only isolated, but rigidly confined to one spot. Thus each individual organism becomes a centre round which extensive multiplication takes place, and in a few days definite points of growth are visible to the naked eye, which are appropriately described as "colonies," and which can be easily counted with the aid of a low magnifying glass. Although each colony consists of many thousands or even millions of individual microbes, yet as in the first instance they owe their origin to a single organism or indivisible group of organisms, it is correct to regard the number of colonies as representing the number of micro-organisms. These colonies have often very beautiful and characteristic appearances, [2] and it is exceedingly remarkable how constant and distinct for one and the same organism these appearances are. In many cases they give rise to magnificent patches of colour—deep orange, chrome yellow, brown, various shades of red, green, black, &c. Often under a low magnifying power they are seen to spread over the surface of the gelatine, producing tangled networks of threads, sometimes they resemble the petals of a flower, sometimes the roots of a tree or its branches; in fact, one is constantly startled by the novelty and beauty of their modes of growth.

Koch [3] and, later, Hesse [4] have devised methods by which the organisms in the air become deposited on a solid surface of gelatine-peptone, and by there producing colonies render their estimation possible. A large number of experiments have been

[1] "Annuaire de l'Observatoire de Montsouris," 1879–86.
[2] "Studies on some New Micro-organisms obtained from Air," Phil. Trans., vol. clxxviii. p. 257.
[3] *Mittheilungen aus dem kaiserlichen Gesundheitsamte*, 1881, Bd. i.
[4] *Ibid.*, 1883, Bd. ii.

made with Hesse's method, which consists in aspirating air through glass tubes about 3 feet in length, coated internally with a film of gelatine-peptone. The organisms, owing to the property they possess of rapidly subsiding in the absence of disturbing influences, fall on the surface of the gelatine, and give rise to colonies.

The following series of observations was made by this method in 1886[1] on the roof of the Science Schools, South Kensington Museum, in order to trace the seasonal variations in the number of micro-organisms present in the air of one and the same place. The following are the averages obtained for each month during which these observations were made:—

Number of Micro-organisms found in Ten Litres (Two Gallons) of Air.

January	4	August	105
March	26	September	43
May	31	October	35
June	54	November	13
July	63	December	20

From these figures it will be seen that it is during the summer that the largest number of micro-organisms are found in the air, whilst the smallest average number was recorded in the month of January.

The air at sea, the air at higher altitudes, and the air in sewers, have all been explored by means of Hesse's method.

Thus Dr. Fischer,[2] in experiments carried on at sea, found that beyond a distance of 120 sea miles from land micro-organisms were invariably absent. And, inasmuch as micro-organisms are abundantly present in sea-water, it thus appears that no micro-organisms are communicated to the air from the water even when the latter is much disturbed. Moreover, as might have been anticipated, this complete freedom from micro-organisms was attained even in close proximity to land, provided the wind had passed over the above-mentioned distance of sea.

As regards the air at higher altitudes, experiments have been made on the dome of St. Paul's, in London, and on the spire of Norwich Cathedral, which show that even in ascending to such modest elevations in densely-populated centres, the number of micro-organisms suspended in the air undergoes very marked diminution.

Thus, on the top of Norwich Cathedral spire, at a height of about 300 feet, I found in ten litres (two gallons) of air only seven micro-organisms, and on the tower, about 180 feet high, I found nine, whilst at the base of the Cathedral, in the Close, eighteen were found. These results are fully confirmed by another series of experiments made at St. Paul's Cathedral. In this case the air examined from the Golden Gallery yielded in the same volume eleven, that from the Stone Gallery thirty-four, whilst in the churchyard there were seventy micro-organisms present.

The contrast between town and country air, and even between the air of the London parks and streets is also exceedingly sharp. In Hyde Park—the place selected for the experiment being as far removed from roads and traffic as possible—I found eighteen, whilst on the same day, June 7, the air in the Exhibition Road, South Kensington, yielded as many as ninety-four. On the following day, however, when the traffic was very great, and the air was consequently heavily laden with dust, the number rose to 554. This is in marked contrast to the microbial condition of country air, for on the Surrey Downs in the same volume only two micro-organisms were found; and in the case of an extensive heath near Norwich only seven.

Within doors we find that the number of micro-organisms suspended in the air depends, as we should have expected, upon the number of people present, and the amount of disturbance of the air which is taking place. Thus, on examining the air in the large entrance hall of the Natural History Museum in Cromwell Road it was found to yield under ordinary conditions from fifty to seventy organisms in the same volume (two gallons), but on Whit Monday, when an immense number of visitors were present in the building, I found as many as 280. Again, on a paying day at the South Kensington Museum, about eighteen micro-organisms were found, but on the Saturday, when no entrance fee is charged, there were as many as seventy-three in the same volume of air.

[1] "The Distribution of Micro-organisms in Air," Proc. Roy. Soc., No. 245, 1885; "Further Experiments on the Distribution of Micro-organisms in Air," Proc. Roy. Soc., vol. xlii. p. 267, 1886.

[2] "Bacteriologische Untersuchungen auf einer Reise nach Westindien," *Zeitschrift für Hygiene*, Bd. i. Heft 3.

The air of sewers has been shown by Carnelley in this country, and by Petri in Berlin, to be remarkably free from micro-organisms, the number being almost invariably less than in outside air. That this should be the case is only natural when the moist nature of the walls and the absence of dust in these subterranean channels is borne in mind, and although their liquid contents is teeming with bacterial life, there is no reason why the latter should be carried into the air provided no effervescence or splashing takes place. On the other hand, if the contents of a sewer enter into fermentation and bubbles of gas become disengaged, minute particles of liquid with the living matter present may be carried to great distances, and it must not, therefore, be too hastily concluded that because sewer air is generally remarkably free from micro-organisms, that, therefore, a visit to the sewers should be attended with such beneficial results as a trip to sea or the ascent of a mountain summit!

During the use of Hesse's method I became acquainted with several serious defects which it possessed, and in order to overcome these disadvantages I was led to devise a new process[1] for the examination of air. This consists essentially in aspirating a given volume of air through a small glass tube, not more than 4 inches long and ¼ inch in width, which is provided with two filter-plugs, the first of which is more pervious than the second, and consists of glass-wool coated with sugar, whilst the second contains, in addition, a layer, ⅛ inch in thickness, of fine sugar-powder. On these plugs the microbes suspended in the aspirated air are deposited, and each of these plugs is then introduced into a separate flask containing a small quantity of melted gelatine-peptone; with this the plug is agitated until it becomes completely disintegrated, and since the sugar-coating of the glass-wool dissolves in the liquid gelatine, the microbes become immediately detached. The contents of the flask are then made to congeal in the form of a thin film over its inner surface. The flasks are then preserved at a suitable temperature, and in the course of a few days the colonies derived from the organisms, which were collected by the plug, make their appearance and can be counted and further studied. Now, if the plug has been properly constructed, the flask into which the second or more impervious plug has been introduced will be found to remain quite sterile, clearly showing that the first plug has arrested all the microbes suspended in the aspirated air. This method yields results which agree not only very closely amongst themselves, but also with those obtained by Hesse's method, if the experiments are made in still air, which is the condition necessary for an accurate result being obtained with a Hesse tube. As this new method is equally applicable in disturbed air, it possesses great advantages over Hesse's, and is, moreover, considerably more convenient, as it renders possible the examination of a far larger volume of air in a very much shorter space of time, the apparatus required being also exceedingly portable.

Of the presence of pathogenic or disease-producing micro-organisms in air, there is little or no direct evidence so far; it must, however, be remembered that it is just in the case of those extremely infectious diseases, such as measles, whooping-cough, &c., in which the virus might be expected to be carried through the air, that the exciting organized poisons have not yet been discovered and identified.

The investigations on aërial microbia, so far as they have as yet been carried, are of service in indicating how we may escape from all micro-organisms, whether harmful or harmless; and secondly, how we may avoid the conveyance of micro-organisms into the atmosphere from places where pathogenic forms are known or likely to be present. This acquaintance with the distribution of micro-organisms in general, and the power of controlling their dissemination which it confers, is really of far wider practical importance than discovering whether some particular pathogenic form is present in some particular sample of air. It is this knowledge which has led to the vast improvements in the construction and arrangement of hospital wards and of sick-rooms generally, and which has directed attention to the importance of avoiding all circumstances tending to disturb and distribute dust. It is, moreover, this knowledge of the distribution of micro-organisms in our surroundings which has formed one of the foundations for the antiseptic treatment of wounds—that great step in surgery with which the name of Sir Joseph Lister is associated.

[1] "A New Method for the Quantitative Estimation of the Micro-organisms present in the Atmosphere," *Phil. Trans.*, vol. clxxviii. p. 113.

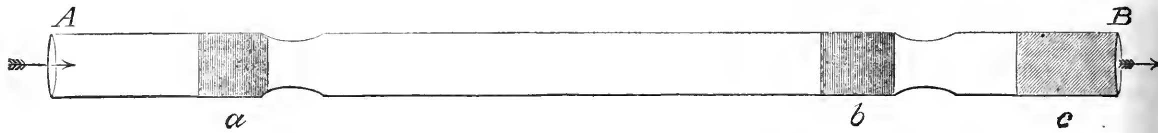


FIG. 1.—Glass tube through which the air is aspirated: about the original size. A, aperture of tube through which the aspirated air enters; B, exit of aspirated air; *a*, first filter-plug, consisting of glass-wool; *b*, second filter-plug, consisting of glass-wool and powdered glass or sugar; *c*, cotton-wool plug to protect plug *b*.

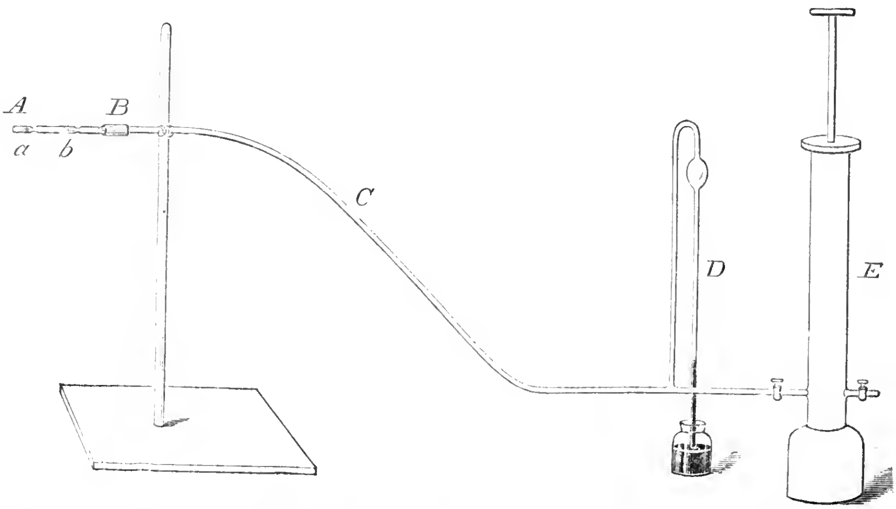


FIG. 2.—Arrangement of the apparatus for taking a sample of air. A, B, the filter tube; C, lead tubing, about 10 feet in length; D, mercury pressure-gauge; E, air-pump.

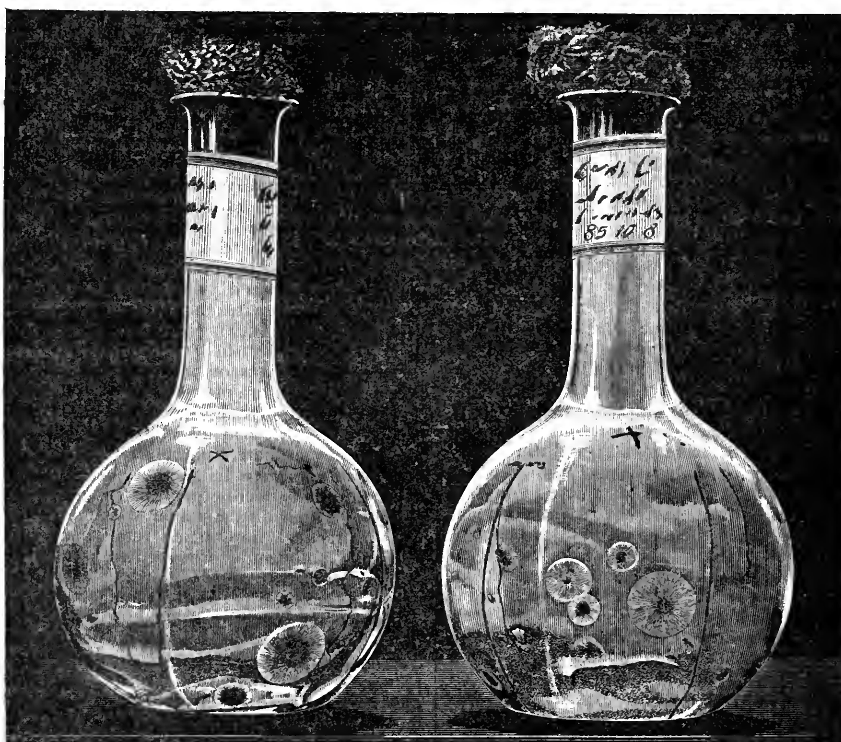


FIG. 3.—Flasks after incubation, showing colonies on film of gelatine with which the inner surface is coated.

Micro-organisms in Water.

The micro-organisms present in water have long been studied by direct observation with the microscope. Such observations can, however, only be made in the case of foul waters in which bacterial life is very abundant, and even in such cases the information gained by the microscope alone has but little value. It is to the modern methods of cultivation, more especially those in which solid media are employed, that our increased knowledge concerning these primitive inhabitants of water is due. Thus the beautiful process of plate-cultivation introduced by Koch,[1] and to which more than to anything else the recent advances in bacteriology are due, has been of the greatest service in the investigation of a number of questions bearing on the micro-organisms in water. The method of plate-cultivation consists, as is well known, in taking some of the liquid or other substance under examination for micro-organisms and mixing it with melted gelatine-peptone in a test-tube, the mixture being then poured out on a horizontal plate of glass and allowed to congeal, the plate being then preserved in a damp chamber at a suitable temperature. In the course of a few days colonies make their appearance in the gelatine film, and can be counted and further studied as required. This process is of extremely wide application, for by this means pure cultivations of the various organisms in a mixture can be readily obtained. If a definite volume of water be submitted to this method of plate-cultivation, the resulting colonies on the plate clearly indicate both the number and the character of the organisms present in it.

From numerous investigations made by means of gelatine plate-cultivations, it appears that whilst surface waters, such as rivers, contain an abundance of microbial life, waters, which like those from springs and deep wells have undergone filtration through porous strata, contain but very few micro-organisms. Now since such underground waters have at some time or other been surface waters, it is obvious that in passing through the porous strata of the earth they have been deprived of those microbes which they contained whilst at the surface. This removal of micro-organisms from water[2] also takes place in a very marked manner when it is submitted to some kinds of artificial filtration, such as that through very finely divided coke or charcoal, as well as in the filtration of water on the large scale through sand. The process of filtration, however, which absolutely removes microbes with the greatest degree of certainty is that introduced by Pasteur, in which the water is forced through porous porcelain. It is especially noticeable that the efficiency exhibited by these various materials in removing micro-organisms stands in no sort of relationship to their chemical activity, *i.e.* power of removing organic matter from water. Thus the porous porcelain produces practically no change whatever in the chemical composition of the water, whilst it deprives it entirely of micro-organisms.

The relative abundance of bacterial life in surface water, in deep well water, as well as in surface water after filtration through sand on the large scale, is well illustrated by the following results.

Thus the average number of micro-organisms obtained during the past year from a cubic centimetre (about twenty drops) of the raw water as abstracted from the Rivers Thames and Lea by the metropolitan water companies was 21,500 and 13,200 respectively. The same water, however, after having undergone storage and filtration contained on an average respectively 500 and 450 micro-organisms in 1 cubic centimetre. It is at once apparent, therefore, what striking results can be obtained by sand filtration as at present carried out, and there is no doubt that with the introduction of fresh improvements and increased care an even greater reduction will be effected.

In deep well water obtained from the chalk, which has undergone no artificial filtration, we find the remarkably low number of eighteen as the average for the year. Thus the artificial filtration through sand is far surpassed by the exhaustive filtration through vast thicknesses of porous strata.

Another point which has been brought to light through investigating the micro-organisms of water by means of the improved methods which we now possess is that many of the microbes found in natural waters are capable of the most abundant multiplication[3] in the absence of practically any organic matter whatever. Thus, if the deep well water referred to above is preserved for several days thoroughly protected from contamination through the air, and is then examined for micro-organisms, it will be found that these have undergone an enormous increase, 1 cubic centimetre containing many thousands instead of the ten or twenty usually present in the water at the time of pumping. It has been found, moreover, that some of the water-organisms are even capable of such abundant multiplication in water which has been several times redistilled, and which is, therefore, almost absolutely pure. From what source such organisms obtain their necessary nourishment under these circumstances has not yet been determined. The following figures serve to illustrate the extent to which multiplication of this kind may take place:—

Number of Micro-organisms obtained from 1 cubic centimetre of water.

	Day of Collection.	Standing 1 day at 20° C.	Standing 3 days at 20° C.
Sample of water from Kent Co.'s deep well in chalk	7	21	495,000

It is often urged that the bacteriological examination of water is of little practical importance, inasmuch as the micro-organisms found are not necessarily prejudicial to health, and that the method of examination does not aim at the detection of harmful forms. A little more mature consideration, however, will show that the actual detection of harmful or pathogenic forms is a matter of very little importance, and that if methods of water purification are successful in removing micro-organisms in general, and more especially those which find a suitable home in natural waters, there can be no serious doubt that they will be equally successful in removing harmful forms, which are not specially adapted for life in water. Could it be, for instance, reasonably contested that a method of purification which is capable of removing the *Bacillus aquatilis* from water, would be incapable of disposing of the *Bacillus anthracis* when suspended in the same medium? The supposition is, on the face of it, absurd, and not a particle of experimental evidence can be adduced in its favour. It is, therefore only rational to conclude that those methods of water purification, both natural and artificial, which succeed in most reducing the total number of micro-organisms, will also succeed in most reducing the number of harmful forms should they be present.

As a matter of fact, however, pathogenic forms can and have been discovered in waters by the process of plate-cultivation; thus the "comma-bacillus," which is by many authorities regarded as the cause of Asiatic cholera, was found by Koch in some tank-water in India, and the bacillus which with more or less probability is identified with typhoid fever has by Chantemesse and Widal been discovered in the drinking-water which had been consumed by persons suffering from that disease.

On the other hand, the examination of water for the number of micro-organisms present can have no value if the multiplication referred to above has taken place. Thus, if the number of micro-organisms present in a water is to throw light on the natural purification it has undergone, the sample for examination must be taken as near as possible to the point where it issues from the water-bearing stratum, and, in the case of artificially purified water, as soon as possible after it has left the purifying apparatus.

Of much more importance than the discovery of pathogenic organisms in particular waters is the problem of ascertaining the fate of pathogenic forms, when these are introduced into waters of different kinds. A considerable amount of work has been done in this direction with a number of typical pathogenic forms,[1] and some very remarkable results have been obtained. Thus it has been found that the bacilli of anthrax do not survive many hours on being introduced into ordinary drinking-water; their spores, however, are not in any way affected by such immersion, and even in distilled water the latter retain their vitality for practically an indefinite length of time. In polluted water, such as sewage, on the other hand, not only do the bacilli not succumb, but they undergo extensive multiplication. Similarly Koch's "comma-bacillus" was found to flourish in sewage, being still present in very large numbers after eleven months' residence in this medium. In deep-well and filtered Thames water, on the other hand, although the "comma-bacilli" were

[1] *Mittheilungen aus dem kaiserlichen Gesundheitsamte*, Bd. i., 1881.

[2] "The Removal of Micro-organisms from Water," Proc. Roy. Soc. No. 238, 1885.

[3] "On the Multiplication of Micro-organisms," Proc. Roy. Soc., No. 245, 1886. "Ueber das Verhalten versch'ied. Bacterienarten im Trinkwasser," Meade Bolton, *Zeitschrift für Hygiene*, Bd. i. Heft 1.

[1] "Die Vermehrung der Bacterien im Wasser" Wolffhügel und Riedel, *Arbeiten a. d. kaiserlichen Gesundheitsamte*. "Ueber das Verhalten, &c.," Meade Bolton. "On the Multiplication of Micro-organisms," Proc. Roy. Soc.; also "Recent Bacteriological Research in connection with Water Supply," Soc. Chem. Ind., vol. vi. No. 5.

still demonstrable after nine days, they were only present in small numbers. Much less vitality is exhibited by the micrococcus of erysipelas when introduced into waters of various kinds, for even in sewage this organism was not demonstrable on the fifth day. In fact, all the pathogenic micrococci which have been experimented with in this manner exhibit but little vitality under similar circumstances.

From these experiments it appears, therefore, that whilst ordinary drinking-water does not form a suitable medium for the extensive growth and multiplication of those pathogenic forms which have hitherto been made the subject of investigation in this respect, yet, that in the condition of spores, they are extremely permanent in any kind of water, however pure, and that even those of which no spores are known may often be preserved for days or even weeks.

Thus the investigations which have hitherto been made on the micro-organisms both of air and water, by the light which they throw on the behaviour of micro organisms in general in these media, the manner in which they may be preserved and the manner in which they may be removed, are of great service in indicating how the spread of zymotic diseases through these media is to be avoided.

Until we are fully acquainted with all pathogenic forms of microbes, a consummation which is certainly not likely to be attained in the near future, it is obvious that in endeavouring to exclude dangerous organisms we must attempt to exclude *all* organisms, *e.g.* in the purification of water which has been exposed to possibly noxious pollution, that process of purification which insures the removal or destruction of the greatest proportion of micro-organisms must be regarded as the most efficient. In just the same way as in the antiseptic treatment of wounds, the preventive measures employed by surgeons are of such a nature as to destroy or preclude the possibility of growth of *any* microbes whatever, and not only of those known to be capable of causing mischief.

PERCY F. FRANKLAND.

THE OPENING OF THE MARINE BIOLOGICAL LABORATORY AT PLYMOUTH.

THE Laboratory erected at Plymouth by the Marine Biological Association of the United Kingdom, of which a full account was given last week in NATURE, was opened on Saturday, June 30. The weather was fine, and at ten o'clock a large and distinguished company were present. Having viewed the tanks, the company assembled in the Laboratory, where Prof. W. H. Flower, C.B., F.R.S., Director of the Natural History Department of the British Museum, delivered an address, in the course of which he said :—"The necessity for such institutions as this has been felt almost simultaneously throughout the cultivated nations of the world. The British Isles, with their extensive and varied seaboard, offering marvellous facilities for the investigation of marine life, with their vast economical interests in the denizens of the waters that lave their shores, have been rather behind some other countries in adopting this line of research. Let us hope, however, that being so, we may profit by example and the experience of others, and ultimately, as in so many other similar cases, may outstrip our neighbours in a department of work for which our maritime and insular position seems so specially to fit us. That our country should be alone in neglecting this branch of scientific inquiry was impossible. Stations for the investigation of the phenomena of marine life have been founded at several places on the northern coasts of our island, but all on a very limited scale. An institution commensurate with the importance of the subject and of the nation had to be established sooner or later ; the only questions to be solved were when it was to be founded and where it was to be placed. Much of the success of an enterprise must depend upon the particular time selected for embarking upon it. If delayed too long, the world is a loser by the non-existence of the knowledge that is to be gained from it. On the other hand, premature attempts before sufficient interest in the subject is awakened, or before sufficient information as to the best means of carrying it out has been gained, often end in failure. I think that in this respect we have taken the right medium." After a reference to the Fisheries Exhibition, Prof. Flower continued:—"The question as to the place at which our head-quarters were to be established was at first one of considerable difficulty. Many were the rival claimants, but Plymouth was finally chosen as best approaching the requisite physical and geographical surroundings for such an institution ; and the cordiality with which the Association was welcomed by its leading citizens was in itself a ground of justification for the choice. Though a portion of the old military defences of the town has been given up to our peaceful enterprise, we trust the safety of the inhabitants will not suffer. The Laboratory now stands beneath the Plymouth Citadel and the sea, and an enemy entering the town by the most direct route would have to march over the ruins of the building. That consideration alone should be enough to secure your safety in a war with many of the enlightened science-loving nations of Europe, should such an event unhappily arise. As to the institution itself, few words are needed to show how excellent is its adaptation to the purpose for which it is founded. Although still not in all respects in full working order, we have been all enabled to see to-day how carefully it has been planned, and how well the design has been carried out. We have secured a capable and energetic working staff, students are already taking their places at our laboratory tables, and already a commencement has been made in their original investigations and contributions to knowledge, which we hope will be of such a character and of such abundance as to give this Laboratory a high place among the scientific institutions of the world. Our present financial position and our future needs are fully set forth in the report of the Council, just issued. This shows that of our capital already subscribed the greater part has been expended on the building and the necessary apparatus for its equipment. We still want a steam-vessel for the use of the staff in exploring the fishing grounds of the neighbourhood and for collecting materials to stock our tanks ; and for the means of providing this, and for the annual maintenance of our establishment in a state of efficiency, we shall require further pecuniary assistance. But as the report is, or shortly will be in your hands, I need not detain you longer by enlarging upon its contents. I will therefore, in the name of the President and Council of the Marine Biological Association of the United Kingdom, thank all those who have, by their generous contribution of money or by expenditure of their time, labour, and thought, brought us so far on our way, and declare the Laboratory of the Association open for work. May we all join in the earnest hope that the expectations which have been raised of its future usefulness may never be disappointed."

The company, after being photographed, adjourned to the Grand Hotel on the Hoe, where they sat down to a *déjeûner* given by the Fishmongers' Company. Sir James Lawrence, Prime Warden of the Fishmongers' Company, presided, and was supported by the Earl of Morley, Prof. Flower, Sir H. W. Acland, K.C.B., F.R.S, the Mayor of Plymouth (Mr. H. J. Waring), the Mayor of Devonport (Mr. J. W. W. Ryder), the Chairman of the Stonehouse Local Board (Mr. E. A. Lyons), Prof. E. Ray Lankester, LL.D., F.R.S., Sir Edwin Saunders, Sir George Paget, K.C.B., F.R.S., the Ven. Archdeacon Wilkinson, Prof. A. Milnes Marshall, F.R.S., Prof. Charles Stewart, Mr. J. Evans, P.S.A., F.R.S., Captain Wharton, R.N., F.R.S., the Vice-Chancellor of Cambridge, Sir Edward Watkin, M.P., Prof. J. W. Groves, Rear-Admiral H. D. Grant, C.B., Major-General T. C. Lyons, C.B., Mr. Thiselton Dyer, C.M.G., F.R.S., Mr. A. Sedgwick, F.R.S., Mr. W. Pengelly, F.R.S., Mr. F. Crisp, F.R.S., Colonel Hewet, R.E., Rev. J. Hall Parlby, Dr. A. Günther, F.R.S., Major-General Barton, R.E., Captain Inskep, R.M., Mr. Robert Bayly, Prof. F. Jeffery Bell, Prof. D'Arcy Thompson, Prof. G. B. Howes, Mr. C. Spence Bate, F.R.S., Prof. M. Foster, Mr. W. Lant Carpenter, Mr. E. W. N. Holdsworth, Mr. E. L. Beckwith, Fishmongers' Company, Mr. Gilbert C. Bourne, and Mr. J. Solly Foster and Mr. John Hall, Wardens, Fishmongers' Company.

The health of "The Queen" having been given by the Chairman, Lord Morley proposed "The Marine Biological Association of the United Kingdom." He said he was sure that his friends the Mayors of Plymouth and Devonport would join with him in wishing a hearty welcome to the Association, and in sincerely hoping that the Laboratory would prove a success. Any doubt as to the practical value of the Laboratory was dissipated by the fact that the Chairman was one of its main founders, and also that many well-known gentlemen, including the Chairman of the National Association at Kensington and Kew, anticipated good results therefrom. Since there was such a consensus of opinion as to the importance of the scheme from a practical and scientific point of view, the thing which surprised him was why it was not done before. We reaped the richest harvest from the sea, and yet we had never inquired scientifically into the source of this great industry. We had lagged behind other nations in this respect. France had no less than four institutions of a similar kind; Austria, with its small coast, had one at Trieste; and the German Government endowed their Laboratory at Naples, which was the most complete in existence, with £1500 a year. From certain statistics recently given to Parliament by the Board of Trade, they learned that the production of fish in the United Kingdom of Great Britain and Ireland last year amounted in value to six and a quarter millions, and if they took the retail value and not the wholesale value, as put in the statistics, it would amount to not less than thirteen millions per year. The east coast was by far the most fruitful of all our coasts as regards the fishing industry, Grimsby, Hull, Lowestoft, and Yarmouth producing £2,800,000 worth of fish. Plymouth with its £96,000 worth of fish per year, Brixham with its £56,000, and Penzance with its £41,000, gave some idea of what the sea produced in the shape of food. Comparing these figures with other countries, it would be found that Canada did not produce four millions worth of fish, and France even less. Then they ought to consider the immense amount of traffic our fishing industry gave to our railways. From Plymouth alone there were sent on two lines of railway 50,000 tons of fish annually. It seemed to him an extraordinary thing that so many years should have elapsed before scientific methods were adopted for learning the conditions under which fish live. If they read the interesting Reports of the Trawling and Fishing Commission, they would be surprised at the ignorance of fishermen as to the habits of fish, their modes of existence, their food, and the climatic and other effects which influenced their existence and modes of living, and he was afraid that ignorance was not confined to fishermen. The great want was, he hoped, about to be supplied in the establishment of this Laboratory. In heartily wishing success to the Marine Biological Association of the United Kingdom, he had the greatest possible pleasure in coupling with it the name of Prof. Ray Lankester.

Prof. Ray Lankester said it was with feelings of pride that he rose to return thanks. It was the great Fisheries Exhibition which suggested the movement for the formation of a laboratory where fishery studies could be carried on. The idea they had in view at that time, or rather the institution existing elsewhere which they wished to copy, was that established by Dr. Dohrn at Naples, with which they were all familiar. The question was, How could such a laboratory be put up on the British coast? And it was to his friend Dr. Günther, of the British Museum, that they owed the suggestion of the formation of an Association. It was to the officers of the Royal Society that they owed the opportunity of starting the Association. A meeting was called in the rooms of that Society, and presided over by the illustrious President of the great scientific institution, which was also the first public body to support the funds of the Association with a large and handsome subscription, and was very largely attended by men of science and gentlemen interested in fisheries, while the late Earl of Dalhousie, one of their most ardent supporters, the Duke of Argyll, and other public men took part in it. The newspaper Press had all along helped them in a most admirable and cheering manner. The *Times* had been their warmest friend, and he hoped it would continue to be so for years to come. No sooner had the first start been made at the meeting in the rooms of the Royal Society and the subscription list put forward than many other big societies came in and individuals throughout the country put down their money, as did also the Universities of Oxford and Cambridge. Subscriptions had been received from purely scientific bodies and individuals to the amount of £3000, and from various sources a total sum of £16,000 to £17,000 had been obtained. The most important item of support given to the Association was the grant from Her Majesty's Government of £5000 and £500 a year. The remaining £10,000 they owed to the great civic Companies and to munificent individuals, among whom he must not omit to mention with hearty gratitude their friends Mr. John Bayley and Mr. Robert Bayley, of Plymouth. No sooner had the enterprise been set on foot than His Royal Highness the Prince of Wales expressed his desire to become the patron of the institution, and support came in from every side. The Inspector-General of Fortifications and the Earl of Morley were instrumental—were, in fact, the actual causes of their receiving the grant of the splendid site on which the building had been erected; and the co-operation and consent of the Town Council of Plymouth, who had certain rights over the area, were cheerfully given. They had now arrived at a definite stage in their work: the building was completed, the laboratory was equipped, the naturalists were on the spot, and they had thus, as he had said, accomplished what he considered to be the first step in the work of the Association. But it was only the first step. Beyond the mere existence of the laboratory building, they had still to justify themselves in the eyes of their supporters by the work that was done within it. He thought they might rely upon the staff they had been fortunate enough to obtain. He had the greatest confidence in the work that would be done in the institution, and in the direction which would be given to that work by his friend Mr. Gilbert Bourne, assisted by the experience of his friend Mr. J. T. Cunningham, who had come to them fresh from his work in Scotland, and students of all ages. He would mention once more a subject which had been already alluded to. They wanted a yacht of their own—not a pleasure-yacht, but a steam sea-going vessel which could accompany the trawlers on their expeditions, and should be a thoroughly seaworthy boat. He hoped that those who were able to place additional funds at their disposal, and who had been pleased and gratified with the way in which they had expended the money already intrusted to them, would not delay to add to the resources of the Association so as to enable them to purchase this steamer.

The Prime Warden then proposed "Prosperity to Plymouth," and the Mayor of Plymouth replied.

Sir George Paget, K.C.B., proposed the health of the Prime Warden, who responded, and three cheers having been given for the Fishmongers' Company, the guests dispersed.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The following have been placed in the first class in the Natural Sciences Tripos, Part I. (the names are in alphabetical order):—Baily, Joh.; Daniel, Trin.; Falkener, King's; Hankin, Joh.; Horton-Smith, Joh.; Jones, King's; R. Langdon-Down, Trin.; Locke, Joh.; Long, Caius; Morrell, Caius; Newstead, Christ's; Perkins, Emman.; Phear, Trin.; Schott, Trin.; H. Simpson, Joh.; H. Smith, Trin.; W. A. L. Smith, Trin.; Thornton, B.A., Christ's; Whetham, Trin.; G. Wilkinson, jun., Emman.

Women.—Class I.—L. Ackroyd, Newnham; D. Alford, Girton; A. G. Earp, Newnham; L. R. Howell, Girton; M. Kennedy, Girton.

The following have been placed in the first class in the Natural Sciences Tripos, Part II.:—Ds. Anderson, Caius (physiology); Barber, Christ's (botany); Ds. D'Arcy, Caius (physics); Ds. Francis, King's (human anat. and physiology); Fry, King's (botany); Hardy, Caius (zoology); Hutchinson, Christ's (chemistry); E. R. Saunders, Newnham (physiology).

Mr. A. C. Seward, B.A., Scholar of St. John's College, has been elected Harkness Scholar in geology and palæontology.

Mr. W. W. Watts, M.A., has been elected to a Fellowship at Sidney-Sussex College. Mr. Watts graduated in the Natural Sciences Tripos, 1881, and was placed in the first class for proficiency in geology.

At Downing College the following have been elected to minor Scholarships of £50 each open to the competition of persons not yet in residence: H. Brownsword, for physics, Manchester Grammar School; C. Swift, for chemistry, University College, Liverpool; and H. Widdicombe, private tuition, for botany. G. Dodson has been elected Foundation Scholar for Natural Science.

At Christ's College the following undergraduates have been elected to Natural Science Scholarships: A. H. L. Newstead, £60; C. Krishnau, £50; R. H. Luce, £30; H. M. Stewart, £30.

At King's College, R. C. Fry has been elected Natural Science Scholar, and G. L. Rolleston to an Exhibition of £40, and L. Falkener to £30.

At Gonville and Caius College, H. B. Brunner, Berkhampstead School, has been elected to an Entrance Scholarship of £50 for natural science.

The following Natural Science Scholars have been elected at St. John's College: H. Simpson, Hankin, Horton-Smith, Locke, Baily, Blackman, Schmitz. Turpin, B.A., has been elected Hutchinson Student for organic chemistry.

SCIENTIFIC SERIALS.

THE *Journal of Botany* continues, in its numbers for April, May, and June, Mr. J. G. Baker's Synopsis of the Tillandsieæ, and MM. Britten and Boulger's valuable biographical index of British and Irish botanists (deceased).—Students of diatoms will be interested in Mr. J. Rattray's paper on *Aulacodiscus*, in which the many and singular abnormalities of this genus of fossil diatoms are described and illustrated by a plate.—Mr. G. Massee contributes a revision of the genus *Bovista*, in which several new species of this genus of Fungi are described, also illustrated by a plate.—We have also biographies of Prof. Asa Gray, and Mr. John Smith, of Kew (the portrait of the former is not a pleasing one), and several papers on local or descriptive botany.

IN the *Botanical Gazette* for February, March, and April, we have no important papers of original research or observation such as sometimes reach us in this record of the doings of botanists in the Far West (published at Crawfordsville, Indiana). The original papers in these numbers relate almost entirely to the distribution of plants in the Western States of America, and to the description of American species.

THE number of the *Nuovo Giornale Botanico Italiano* for April contains the conclusion of Prof. A. N. Berlese's monograph of the genera of Fungi *Pleospora*, *Clathrospora*, and *Pyrenophora*, with the ten coloured plates which serve to illustrate the whole paper; and a description by Sig. C. Massalongo of a number of instances of teratology, chiefly relating to the flower. It serves further as the medium of publication of the proceedings of the Italian Botanical Society, reports being appended of a number of smaller contributions in various departments of botany.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 21.—"Muscular Movements in Man, and their Evolution in the Infant: a Study of Movement in Man, and its Evolution, together with Inferences as to the Properties of Nerve-centres and their Modes of Action in expressing Thought." By Francis Warner, M.D., F.R.C.P., Physician to the London Hospital, and Lecturer on Botany in the London Hospital Medical College. Communicated by Prof. J. Hutchinson, F.R.S.

Before proceeding to give an account of the visible evolution of voluntary movement in man, it is necessary to define the different classes of movements seen, indicating the criteria by which the observer may be guided in the examples before him.

The new-born infant presents constant movement in all its parts while it is awake, and this is not controlled by impressions from without. Graphic tracings of such movements are given This spontaneous movement in the infant appears to be of great physiological importance, and is here termed "microkinesis." It is argued that the mode of brain action which produces microkinesis is analogous to the action producing spontaneous movements in all young animals, and to the modes of cell-growth which produce circumnutation in young seedling plants. It is argued that as circumnutation becomes modified by external forces to the modes of movement termed heliotropism, geotropism, &c, so microkinesis in the infant is replaced by the more complicated modes of brain action as evolution proceeds.

The conditions of movement are then described, as seen at successive stages of development of the child, and it is shown that they become less spontaneous, and more under control of stimuli acting upon the child from without, while the phenomena termed memory and imitation are evolved.

From observations made, two hypotheses are put forward. It is suggested that when a well co-ordinated movement follows a slight stimulus, the impression produces temporary unions among the centres, preparing them for the special combinations and series of actions which are seen to follow. Such unions among nerve-centres appear to be formed when a period of cerebral inhibition, produced by a word of command, is seen to be followed by a co-ordinated series of acts. A graphic tracing indicating suspension of microkinesis to the stimulus of sight and sound is given. It is further suggested that the brain action corresponding to thought is the formation of functional unions among cells, whose outcome is seen in the movements which express the thought, or its physical representation. Properties similar to those described in brain-centres may be illustrated in modes of growth.

"Evaporation and Dissociation. Part VIII. A Study of the Thermal Properties of Propyl Alcohol." By William Ramsay, Ph.D., F.R.S., and Sydney Young, D.Sc.

In continuation of our investigations of thermal properties of pure liquids, we have now determined the vapour-pressures, vapour-densities, and expansion in the liquid and gaseous states of propyl alcohol, and from these results we have calculated the heats of vaporization at definite temperatures. The compressibility of the liquid has also been measured. The range of temperature is from 5° to 280° C., and the range of pressure from 5 mm. to 56,000 mm.

The memoir contains an account of the purification of the propyl alcohol; determinations of its specific gravity at 0°, and at 10°·72; and of the constants mentioned above.

The approximate critical temperature of propyl alcohol is 263°·7; the approximate critical pressure is 38,120 mm., and the approximate volume of 1 gramme is 3·6 c.c. The first two of these constants must be very nearly correct; the third cannot be determined with the same degree of precision.

The memoir is accompanied by plates, showing the relations of volume, temperature, and pressure in a graphic form.

Royal Meteorological Society, June 20.—Dr. W. Marcet, F.R.S., President, in the chair.—The following papers were read:—First Report of the Thunderstorm Committee. This Report deals with the photographs of lightning-flashes, some sixty in number, which have been received by the Society. From the evidence now obtained it appears that lightning assumes various typical forms, under conditions which are at present unknown. The Committee consider that the lightning-flashes may be arranged under the following types: (1) stream, (2) sinuous, (3) ramified, (4) meandering, (5) beaded or chapletted, and (6) ribbon lightning. In one of the photographs there is a *dark* flash of the same character as the bright flashes, but the Committee defer offering any explanation of the same until they get further examples of dark flashes. As the thunderstorm season is now coming on, the Committee propose to publish their Report at once, along with some reproductions of the photographs by the autotype process, in order that observers may be prepared to notice the various forms of lightning.—The cold period from September 1887 to May 1888, by Mr. C. Harding. The mean temperature for each of the nine months from September 1887 to May 1888 was below the average, whilst in the case of October there has been no corresponding month as cold during the last half century, and only three colder Aprils. In London the mean temperature for the period was only 42°·4, and there has been no similarly low mean for the corresponding period since 1854-55, which will be remembered as the time of the Crimean War, and only three equally cold periods during the last 50 years. The temperature of the soil at Greenwich at 3 feet below the surface was below the average in each month from October to April; in October and April the temperature at this depth was the coldest on record, observations being available for the last 42 years, and in November it was the coldest for 37 years.—Observations on cloud movements near the equator; and on the general character of the weather in the "Doldrums," by Hon. R. Abercromby. The author gives the results of observations made during four voyages across the equator and the "Doldrums," with special reference to the motion of clouds at various levels. Two voyages were across the Indian Ocean during the season of the north-west monsoon, and two across the Atlantic in the months of July and December. The nature of the general circulation of the atmosphere near the "Doldrums" is discussed as regards the theory that the Trades, after meeting, rise and fall back on themselves; or, according to the suggestion of Maury, that the Trades interlace and cross the equator; or, as following the analogy of Dr. Vettin's experiments on smoke. It is shown that the materials at present available are insufficient to form a definite conclusion, but details are given of the general character of the weather and of the squalls in the "Doldrums," with a view of showing what kind of observations are required to solve this important problem. The old idea of a deep Trade—with a high opposite current flowing overhead—is certainly erroneous; for there is always a regular vertical succession of the upper currents as we ascend, according to the hemisphere.

Zoological Society, June 19.—Prof. Flower, F.R.S., President, in the chair.—A letter was read addressed to the President by Dr. Emin Pasha, dated Tunguru Island (Lake Albert), October 31, 1887, announcing the despatch of further collections of natural history objects, and promising for the

Society some notes on European migratory birds observed in that country.—An extract was read from a letter addressed by Mr. E. L. Layard to Mr. John Ponsonby concerning the occurrence of a West Indian Land-Shell (*Stenogyra octona*) in New Caledonia.—Mr. Tegetmeier exhibited and made remarks on the feet of an Australian Rabbit, supposed to have acquired arboreal habits.—Prof. Bell exhibited and made remarks on a specimen of a tube-forming Actonian (*Cerianthus membranaceus*) in its tube; obtained by Mr. John Murray at a depth of 70 fathoms in Loch Etive.—A communication was read from Prof. W. Newton Parker, on the poison-glands of the fishes of the genus *Trachinus*. This paper showed the existence of glands in connection with the grooved dorsal and opercular spines of the two British species of Weever. The glands were stated to be composed of large granular nucleated cells, which are continuous with those of the epidermis. An account of the observations of previous authors, both as regards the structure and physiology of the poison-organs of these fishes, was also given.—A communication was read from Mr. H. W. Bates, F.R.S., containing the description of a collection of Coleoptera made by Mr. J. H. Leech, during a recent visit to the eastern side of the Corean Peninsula.—A second communication from Mr. Bates treated of some new species of Coleoptera of the families Cicindelidæ and Carabidæ from the valley of the Yang-tsze-Kiang, China.—Mr. J. B. Sutton read a paper on some abnormalities occurring among animals recently living in the Society's Gardens.—Prof. Bell read an account of a collection of Echinoderms made at Tuticorin, Madras, by Mr. Edgar Thurston, Superintendent of the Government Central Museum, Madras.—A communication was read from Mr. F. Moore, containing the second portion of a list of the Lepidoptera collected by the Rev. J. H. Hocking, chiefly in the Kangra District of the North-Western Himalayas. The present paper contained the descriptions of seven new genera and of forty-eight new species. An account of the transformations of a number of these species was also given from Mr. Hocking's notes.

Geological Society, June 20.—Dr. W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—On the occurrence of marine fossils in the Coal-measures of Fife, by Jas. W. Kirkby; communicated by Prof. T. Rupert Jones, F.R.S.—Directions of ice-flow in the North of Ireland, as determined by the observations of the Geological Survey, by J. R. Kilroe; communicated by Prof. E. Hull, F.R.S.—Evidence of ice-action in Carboniferous times, by John Spencer.—The Greensand bed at the base of the Thanet sand, by Miss Margaret I. Gardiner, Bathurst Student, Newnham College, Cambridge; communicated by J. J. H. Teall.—On the occurrence of *Elephas meridionalis* at Dewlish, Dorset, by the Rev. O. Fisher.—On perlitic felsites, probably of Archæan age, from the flanks of the Herefordshire Beacon, and on the possible origin of some epidosites, by Frank Rutley. The author has previously described a rock from this locality in which faint indications of a perlitic structure were discernible. In the present paper additional instances were enumerated and a description was given. The perlitic structnre is difficult to recognize, owing to subsequent alteration of the rock. Decomposition-products, apparently chiefly epidote, with possibly a little kaolin, have been found in great part within the minute fissures and perlitic cracks. The author suggested, from his observations, that felsites, resulting from the devitrification of obsidian, quartz-felsites, aplites, &c., may, by the decomposition of the felspathic constituents, pass, in the first instance, into rocks composed essentially of quartz and kaolin; and that by subsequent alteration of the kaolin by the action of water charged with bicarbonate of lime and more or less carbonate of iron in solution, these may eventually be converted into epidosites. He regarded it as probable that the rocks are of later Archæan or Cambrian age.—The ejected blocks of Monte Somma, Part I., stratified limestones, by Dr. H. J. Johnston-Lavis.

Palæontographical Society, June 22.—Annual Meeting. Dr. H. Woodward, F.R.S., Vice-President, in the chair.—The Report of the Council, presented by the Secretary, Prof. Wiltshire, stated that since the date of the last annual meeting the volume for 1887 had been issued, and that the volume for the present year was in progress. It would contain the following parts of monographs: the Stromatoporoids, Part II., by Prof. Alleyne Nicholson; the Cretaceous Echinodermata, Part I., by Dr. W. P. Sladen; the Jurassic Gasteropoda, Part III., by Mr. W. H. Hudleston; the Inferior Oolite Ammonites, Part II., by Mr. S. S. Buckman. It was stated that the arrangement by which members had been enabled to procure parts of finished monographs as well as the complete monographs, distinct from the annual volumes, had been found to work very efficiently. It was further stated that the financial position of the Society was much better than on the previous occasion. This was due in part to a grant of £50 made by the General Committee of the British Association at the Manchester meeting, and in part to the very considerable increase in the number of subscribers, which had resulted from the efforts made during the past and preceding year to bring before geologists, palæontologists, and all interested in science, the work which was carried on by the Society. If the present improvement could be maintained, there need be no fears for the future.—Sir R. Owen was re-elected President; Mr. Etheridge, Treasurer; and Prof. Wiltshire Secretary. Messrs. W. E. Balston, C. J. A. Mëyer, G. H. Morton, and W. P. Sladen were elected members of the Council, in the place of Messrs. S. S. Buckman, J. Evans, C. H. Gatty, and W. C. Lucy, who retire by rotation.

PARIS.

Academy of Sciences, June 25.—M. Janssen, President, in the chair.—On the canals of the planet Mars, by M. Fizeau. The various circumstances connected with these appearances, as lately described by MM. Perrotin and Schiaparelli, suggest a strong analogy with certain phenomena of glaciation—parallel ridges, crevasses, rectilinear fissures often of great length and at various angles—observed in the regions of large glaciers in Switzerland and especially in Greenland. This leads to the hypothesis of a vast development of glaciation on the surface of Mars, where, the seasons being relatively longer and the temperature much lower, the conditions must also be more favourable than on the earth for these manifestations. The reading of the paper was followed by some remarks by M. J. Janssen, who gave a guarded assent to M. Fizeau's "very ingenious and very beautiful" theory.—On the vapour-density of the chloride of aluminium, and on the molecular weight of this body, by MM. C. Friedel and J. M. Crafts. The recent experiments of MM. Nilson and Pettersson tended to show that this substance should receive the formula $AlCl_3$ rather than the double formula Al_2Cl_6 proposed by MM. Sainte-Claire and Troost. The fresh researches of MM. Friedel and Crafts, undertaken to settle the point, lead to the conclusion that the density corresponds to Al_2Cl_6, which would accordingly represent the molecular weight of the chloride of aluminium. The experiments of MM. Louïse and Roux on methyl and ethyl aluminium are in harmony with this inference.—Progress of the Roscoff and Arago Laboratories, by M. de Lacaze-Duthiers. Both of these important biological stations have lately been inspected by the author, who is able to speak most favourably of their present state. Zoologists will find concentrated at Banyuls during the winter and at Roscoff in summer all the conditions best adapted for the study of the lower forms of animal life.—Some remarks relative to the representation of irrational numbers by means of continuous fractions, by M. Hugo Gyldén. From the points here discussed flows a thesis of great importance connected with the convergence of certain trigonometric series employed in the calculation of perturbations. The thesis is thus worded: The probability of finding a value for a beyond a given limit is in inverse ratio to the number expressing this limit.—On the degrees of oxidation observed in the efflorescing compounds of chromium and manganese, by M. Lecoq de Boisbaudran. In this first paper on the subject the author deals mainly with the carbonate of lime in combination with an oxide of chromium (or chromate of ammonia), and highly calcined in the air. He shows that chromium produces with lime a fluorescence which seems to present no analogy with those yielded by it in combination with alumina, gallina, or magnesia.—On orthogonal substitutions and the regular divisions of space, by M. E. Goursat. The divisions here determined may be connected with the regular figures of space of four dimensions. Thus may readily be found the six regular figures discovered by Stringham. But the question may be pushed further, and, by following Poinsot's method, in space of four dimensions the existence may be shown of regular figures analogous to the regular starred polyhedrons of space of three dimensions. These results, here merely indicated, will be fully developed in a memoir which will shortly be published.—On a theorem of Kummer, by M. E. Cesaro. This is in connection with a recent paper by M. Jensen, who is stated to defend himself from inaccuracies of which he was not accused. In the author's communication of April 16, nothing was questioned except the *novelty* of M. Jensen's theorem, which does not differ essentially from that of Kummer, as modified and completed by Dini in 1867.—On the hydrochlorates of trichloride of antimony, of trichloride of bismuth, and of pentachloride of antimony, by M. Engel. The researches here described fully confirm the existence of these bodies, which are described as well-defined salts that may be easily isolated. Like all the other hydrochlorates of chloride hitherto prepared, they all contain water of crystallization, and there are in each case at least two molecules of water for each molecule of hydrochloric acid fixed by the chloride.—On the reproduction of phenacite and the emerald, by MM. P. Hautefeuille and A. Perrey. The conditions are described under which the authors have effected the synthesis of two substances whose properties are identical with those of natural phenacite and the emerald. The analysis of the artificial emerald yielded silica 67·7, alumina 19·6, and glucine 13·4, which are nearer to the calculated proportions than those given for the composition of most natural emeralds. The analyzed crystals, whose density was 2·67, were colourless; but greenish-yellow and green crystals were easily obtained—the former by the oxide of iron, the latter by the oxide of chromium.

BERLIN.

Physiological Society, June 8.—Prof. du Bois-Reymond, President, in the chair.—Prof. Kossel spoke on a new constituent of tea. Inasmuch as the presence of caffein in tea does not suffice to explain its physiological action, he had examined it for other bases, and found in the leaves of tea, in addition to adenin, a new well-characterized base whose composition is $C_7H_8N_4O_2$, to which he has given the name of theophyllin. Theobromin and paraxanthin have the same chemical composition as theophyllin, but the latter differs from the former by a series of well-marked chemical reactions. One question of special interest was as to the constitution of the new base, which belongs to that class of substances known as the xanthin-bodies. Fischer has shown that xanthin yields alloxan and urea when oxidized; and, similarly, it is known that theobromin is dimethylxanthin, yielding, by oxidation, methylalloxan and methylurea; as also that caffein is trimethylxanthin, yielding, by oxidation, dimethylalloxan and monomethylurea. The question hence arose as to the constitution of the new base, which, since it is isomeric with theobromin, is also presumably a dimethylxanthin. Since the speaker was in possession of so limited a quantity of the substance that he could not proceed to oxidize it, he proceeded by a different method, and introduced a methyl group into the molecule of theophyllin: on performing this experiment he obtained caffein, from which it must be concluded that theophyllin contains one methyl group united to a residue of urea, and one to a residue of alloxan, and has therefore a constitution identical with that of theobromin. It still remains to investigate the physiological action of the new base.—Dr. Will spoke on the alkaloids of the Solanaceæ, of which at present only atropin, hyoscyamin, and hyoscin are known as distinct substances with reference to their mydriatic action. The first two of the above are of special interest, as possessing the same chemical composition ($C_{17}H_{23}NO_3$), but differing as regards their melting-point, the salts which they form with gold, and their specific rotatory powers. It had been noticed long ago that sometimes much atropin and but little hyoscyamin, and, *vice versâ*, much hyoscyamin and but little atropin, is obtained from the roots of Belladonna. This difference in the relative amounts of the two substances obtained was noticed when portions of the same sample of roots were treated in the same way; as the result of which the chemical factory of Schering had requested the speaker to investigate the cause of this difference in the relative amounts of the several products. The first fact which he determined was, that when hyoscyamin is heated to 109° C.—that is to say, to a temperature slightly above its melting-point—it changes into atropin. This is, however, of no significance in the preparation of the alkaloids, as carried on in a factory, inasmuch as no such temperatures are employed. Dr. Will further found that, when a few drops of alkali are added to a solution of hyoscyamin which possesses strong rotatory powers, in a few hours the rotatory power is lost, and the solution no longer contains hyoscyamin, but atropin. According to this, during the extraction of Belladonna roots in the factory, the amount of hyoscyamin which may have become converted into atropin is dependent upon the time of action and the concentration of the alkaline solution employed in the process: by treatment with alkali, the whole of the hyoscyamin can always be converted into atropin. The fact that, by the extraction of the roots of Hyoscyamus, onl hyoscyamin and no atropin is obtained, was explained by th speaker as being due to the employment of ammonia in th process, which has only a feeble power of converting the on alkaloid into the other. The speaker intends to employ thi conversion of hyoscyamin into atropin, which is measurable b means of change in rotatory power, to the determination of th combining affinities of the alkalies. Dr. Will is inclined t believe that relations similar to the above exist in the case o quinine and cinchonine, which are also obtained in varying rela tive amounts from the bark.—Dr. Koenig gave an account o some experiments, undertaken at his suggestion by Isaacksen with a view to testing Holmgren's statement that very smal coloured dots can only be seen as one of the primary colours o the Young-Helmholtz theory—namely, red, green, or violet. This statement was not, however, confirmed when the necessar precautions were taken, and it was found that small dots of an colour, even yellow and blue, were perceived as possessing thei own objective colour; this had also been observed by Hering Isaacksen had, further, investigated the power which the ey possesses of distinguishing between minute dot-like light which are so small that their image on the retina only falls o one cone, and found that it was as fully developed as for th colours of large surfaces.

BOOKS, PAMPHLETS, and SERIALS RECEIVED

Chemical Problems: Grabfield and Burns (Heath, Boston).—The Move ments of Respiration and their Innervation in the Rabbit: Dr. M. Marck wald, translated (Blackie).—Natural History and Epidemiology of Cholera Sir J. Fayrer (Churchill).—The Photographer's Note-book and Index: Sir D Salomons (Marion).—Short Lectures to Electrical Artisans: J. A. Fleming second edition (Spon).—Whence comes Man; From Nature or from God? A. J. Bell (Isbister).—*Challenger* Expedition Report, vols. xxiii., xxiv. Parts, and xxv., Zoology (Eyre and Spottiswoode).—Annual Report of th Geological and Natural History Survey of Canada, vol. ii. 1886 (Dawson Montreal).—Another World, or the Fourth Dimension: A. T. Schofiel (Sonnenschein).—Changes of Level of the Great Lakes: J. K. Gilbe (Washington).—The Construction and Maintenance of School Infirmaries an Sanatoria (Churchill).—Electricity *versus* Gas: J. Stent (Sonnenschein).—Annalen der Physik und Chemie, 1888, No. 8*a* (Leipzig).

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THURSDAY, JULY 12, 1888.

ELECTRICITY AND MAGNETISM.

A Treatise on Electricity and Magnetism. By E. Mascart and J. Joubert. Translated by E. Atkinson. Vol. II. (London: De La Rue and Co., 1888.)

THE English translation of the second volume of the valuable work of MM. Mascart and Joubert is a welcome addition to the class, none too large, of really substantial English books on electricity. We have already directed the attention of the readers of NATURE to the first volume of this work; and we took occasion to point out that in their exposition of the subject the authors follow very closely the general methods of Clerk-Maxwell. That they do so is a great advantage for the English student; because it enables him, without breach in the continuity of his studies, to use Mascart and Joubert as a commentary upon Maxwell, who is often by no means easy reading. The French work is also supplementary to Maxwell, for writers avoid as much as possible the purely theoretical side of electrical science, and treat electrical phenomena, more especially in their second volume, as subjects of observation, and, above all, of measurement. This volume, which is now before us, is, in fact, an epitome of all the wisdom in exact electrical measurement which has been gained during a period of extraordinary activity in that field. This period began with the researches of Gauss and Weber; and may perhaps be said to have culminated in the great series of determinations of the absolute units which were made about the time of the Congress of Electricians at Paris in 1884. The prominent part taken by MM. Mascart and Joubert in this work has well fitted them to record with precision the details of the leading methods by which it was accomplished, and it would be hard to refer the student of electrical science to an authority on electrical measurement at once so clear and precise in detail, and, with a few small and evidently accidental exceptions, so manifestly candid and fair, as the second volume of the treatise of Mascart and Joubert.

The space at our disposal in the pages of NATURE allows us to give but a brief summary of the contents of this volume. Part I. deals with the auxiliaries of electrical measurement, such as the measurement of angles, of the periods and amplitudes of oscillations, of couples, and of such properties of circular currents as are important in the construction of galvanometers and other electrical instruments. As an example of the care with which the subject is treated, we may refer to the discussion, in §§ 659, 660, of the power of a telescope, and of the relation that ought to subsist between the dimensions of a graduated circle and of the telescope with which it is associated. The conclusion of this discussion is marred in the English version by inadequate translation. Thus, for example, "un cercle de ce diamètre [80 cm.] devra donc être associé à une lunette de 16 centimètres d'ouverture," does not mean "a circle of this diameter is therefore comparable with a telescope of 16 cm. aperture." The meaning is, that, to get the full use of the circle, a telescope having an objective lens of 80 cm. aperture is required; and that a more powerful one is unnecessary.

Part II., which is the kernel of the volume, describes the various electrical measurements as they are carried out in practice. There are chapters on electrometry, and on measurement of current, resistance, electromotive force, capacity, constants of coils, absolute resistance, and the fundamental velocity, *v*. The methods are described in great variety and with great detail. They are illustrated by giving not only the old classical results, but also by means of the most recent examples. Nothing is attempted like the exhaustive catalogue of results, good, bad, and indifferent, which makes Wiedemann such an invaluable book of reference. Experimental results are given simply as part of the exposition of the methods by which they are obtained. It is probably for this reason that the authors make no mention of the valuable experiments on dieletric strength recently made by their fellow-countryman, Baille.

Part III. is devoted to magnetic measurements, and is excellent so far as it goes. It is by no means so exhaustive as the purely electrical part; and, probably for that very reason, will be found to be lighter reading for the tyro in electricity and magnetism; to such we commend more especially the parts relating to the determination of so-called magnetic poles and to the magnetism of feebly magnetic and diamagnetic bodies, subjects which are very frequently imperfectly understood or inadequately expounded in current text-books.

Part IV., which is called a complement, deals with industrial applications, and contains a table of numerical constants. The table of constants gives full references to the sources of such information as it contains, and will be found most useful. The part that deals with industrial applications is to our thinking the least satisfactory part of the book; not because there is any want of clearness or soundness in it, but because it is too short and too scantily illustrated by references to practical cases to give the student any real idea of the problems that surround the electrical engineer.

In describing the various methods of electrical measurement the authors are, on the whole, very sparing of criticism. They seem to assume that they are addressing an audience fitted to draw their own conclusions from the facts put before them. Occasionally the weak points of the methods used by various experimenters are pointed out, but the authors never indulge in that species of criticism which consists in treating a fellow-labourer and all his productions with indiscriminate scorn because the critic has discovered some microscopic oversight, or believes that he has wrung one more decimal place from reluctant Nature.

There are one or two little points which might be amended in a future edition. For example, the elegant method of discussing resisted motion by means of the equiangular spiral, given in § 682, should be attributed to its author, Prof. Tait. The use of the fish-back galvanometer-needle (*i.e.* a needle made up of a number of separate parallel needles) was not an invention of M. Deprez, at least not an original invention; for the writer used, more than twelve years ago, a galvanometer fitted with a needle of this sort, which had been constructed for the B.A. Committee of 1867. Who the inventor was, is doubtful; but probably he took his idea from the laminated magnets constructed by Jamin and others. Perhaps the most serious historical oversight is made in § 1274, where, in speaking of the graphical characteristic of a dynamo, language is used which would lead the reader to infer that this important method in the theory and practice of electrical engineering was introduced by M. Marcel Deprez, the fact being that it was first introduced, fully explained, and actually used by Dr. Hopkinson in 1879.[1] What M. Deprez did, was, we believe, simply to give a name to Hopkinson's curve, and to further develop its applications. It would be easy to correct, in footnotes or otherwise, these and a few similar small blots on a work which is, in most respects, remarkably fair and cosmopolitan in its history.

Regarding the work of the translator, we can, on the whole, speak very favourably. There are, however, passages here and there which are so inadequately translated that they suggest the idea of an inferior assistant not always sufficiently overlooked. Compare, for example, the following piece of the original with the accompanying translation:—

"Si la loi était générale, on en conclurait, pour le cas de deux plateaux parallèles, que la production de l'étincelle correspond toujours à une même valeur de la densité électrique et, par suite, de la force électrique et de la pression électrostatique, ou, dans les idées de Maxwell, à un même état ou une même énergie spécifique du milieu interposé."

"If the law was general, we should conclude, for the case of two parallel plates, that the production of electricity almost represents the same value of the electrical density, and therefore of the electrical force and the electrostatic pressure, or, as in Maxwell's views, to the same condition or the same specific energy of the interposed medium."

It will be seen that the English passage is not a translation of the French, is not English, and means nothing. We mention this, by far the worst, case of loose translation that we have noticed, to draw the attention of the English editor to the need there is for revision. Such corrections as are absolutely necessary might be given on a fly-leaf; and, in order to help, we mention a few things that we have noticed. Some are misprints, some wrong, some merely doubtful.

P. 39, "compass of horizontal intensity"?

P. 41, "observations" (oscillations?)

P. 48, "collate" (collect?)

P. 50, "bodies of easy construction"?

P. 237, "combine the experiment"?

P. 293, "but they are not sufficiently so, &c."?

P. 557, "*residues* of the Leyden jar"?

P. 577, "induced *charges*" (décharges induites)?

P. 578, "to *make the constant* of the ballastic galvanometer" (faire la tare: why use tare? Tare is English).

P. 878, "regulation of a galvanometer" (tarage d'un galvanomètre)?

Notwithstanding minor shortcomings, this English translation of the work of MM. Mascart and Joubert will be of great use to English readers; and we hope that it will not be thought that, by calling attention to inaccuracies here and there, we mean to depreciate the labour of the editor, or to undervalue the debt which the English scientific public owes him for rendering more accessible one of the most important electrical treatises of the day.

G. C.

[1] See his papers in the Proceedings of the Institution of Mechanical Engineers, April 1879 and April 1880.

SYNOPTICAL FLORA OF NORTH AMERICA.

Synoptical Flora of North America: the Gamopetalæ. A Second Edition of Vol. I. Part 2, and Vol. II. Part 1, collected. By Asa Gray, LL.D. Large 8vo. 480 + 494 pp. (Washington: Published by the Smithsonian Institution, 1888.)

THE first feeling which the sight of this book reawakens in the mind is one of deep regret that Prof. Asa Gray did not live to carry out the plans he had entertained so long for an elaboration of a complete flora of Temperate North America upon one uniform plan. A work of this scope was planned by Dr. Torrey and himself when he was quite a young man, and the first part appeared as long ago as 1838. It was soon found by the authors that it was impossible to identify satisfactorily the plants which had been named by their predecessors without studying the European Herbaria; and in order to do this Dr. Gray spent a year in Europe in 1838–39. Another instalment, which extended to the end of Polypetalæ, was published in 1840, and the remainder of the first volume, extending to the end of Compositæ, in 1842. Then Dr. Gray accepted the post of Fisher Professor of Natural History in the University of Harvard, and what with teaching and herbarium work, and the preparation of the successive five editions of his "Flora of the Northern United States," and the elaboration of the new collections that poured in as fresh territories were explored and settled, his time was fully occupied for thirty-five years. In 1878 he returned to the more comprehensive work, and in that year published the first part of the second volume, which includes the remaining orders of Gamopetalæ, from Goodeniaceæ to Plantaginaceæ. In 1884 he issued a revised edition of the part devoted to the Compositæ and small allied orders. The work we have now before us is a reprint of the whole of the Gamopetalæ, with two supplements, embodying additions and corrections up to the end of 1885. Although the title-page bears the date of 1888, it was really issued, as the secondary title-page indicates, in January 1886, and we have had it in use at Kew for a couple of years. The present volume, therefore, covers the central third, brought up to date, of the complete undertaking as planned; and at the beginning the Polypetalous Dicotyledons are still left as they stood in 1840, except for the most useful bibliographical index, brought up to date, which Dr. Sereno Watson issued in 1878; and the Incompletæ and Monocotyledons, to which Dr. Watson has happily devoted special attention during many years, have still to be dealt with.

The flora of Temperate North America contains about the same number of species as that of the whole of Europe, but of course the orders are to a certain extent different, and others enter in the two floras in very different proportions. In the present work there are described 3521 species of Gamopetalous Dicotyledons, of which all but 162 are indigenous. They fall under 562 genera, of which 520 are native. The American Compositæ alone, 1636 species, far more than outnumber the whole Phanerogamic flora of Britain. Next to Compositæ come Scrophulariaceæ, represented by 367 species and 38 genera. Of Hydrophyllaceæ, an order nearly restricted to North America, there are 129 species and 14 genera; of Polemoniaceæ, another nearly endemic order, there are 133 species. The more tropical character of the North American, as compared with the European, flora, is shown by the presence of 44 non-stellate Rubiaceæ, 9 Sapotaceæ, 97 Asclepiadaceæ, 6 Bignoniaceæ, and 41 Acanthaceæ. To get such a large number of plants worked up by such a model systematist as Dr. Gray is an enormous boon to all species botanists. A great many of the species are here described for the first time; and a still larger proportion have only been previously noticed in scattered unclassified papers. A large number of the best-known North American plants cultivated in our gardens belong to Gamopetalæ; and to have such genera as Aster, Solidago, Helianthus, Pentstemon, Phacelia, and Gilia, put in order and brought up to date will be a great saving of time and trouble, and make the book essential, not only to botanists, but to all owners of gardens who wish to understand the characters, affinities, and geographical distribution of the plants they grow.

In arranging their material the authors of the four great recent descriptive local floras have followed four different plans. In Bentham's "Flora Australiensis" there is, under each genus, an initial analytical key, in which each species is distinguished, and afterwards a single detailed description of each species and its varieties. The 8500 species of the Australian flora, described after this plan, fill seven volumes of from 500 to 800 pages each. In Boissier's "Flora Orientalis" the initial key only goes down to the sections, and there is a less detailed single description given of each species. The number of species is about 10,000, and the whole work runs on to five large volumes of about 1000 pages each. In Sir J. D. Hooker's "Flora Indica" there is no initial key, but sub-genera and groups are briefly characterized, and under each species is given both a compact diagnosis and brief description. Under this plan the 10,000 Dicotyledons of India fill five octavo volumes of 700 to 800 pages each. Dr. Gray gives no initial key, more detailed characters of sub-genera and groups, and under each species a single short description. Under this plan the 3500 Gamopetalæ fill a book of 970 larger pages. It is an omission, we think, that Dr. Gray has not numbered his species, for, in referring from the book to the herbarium and back again, such numbers are a very useful guide. Mr. Bentham, Sir J. D. Hooker, and Dr. Gray all three adopt the same comprehensive idea of what constitutes a species, and use substantially the same orders and genera, and the same plan of nomenclature; and it is a very great convenience in herbarium work that these three great floras have been treated upon one uniform system.

Our best sympathies are with the American botanists in the great loss they have sustained. In securing such a competent assistant as Dr. Sereno Watson, Dr. Gray was very fortunate, and we trust that the material for the two other volumes is in such an advanced state of preparation that they may be published under his editorship before long. We European botanists have great reason to thank the managers of the Smithsonian Institution for their liberality in granting funds for the book. What a boon it would be if we could have a general flora of Europe planned upon the same lines; but with all our great Universities and Herbaria this does not at present seem at all likely.

J. G. BAKER.

HYDRODYNAMICS.

Treatise on Hydrodynamics. Vol. I. By A. B. Basset. (Cambridge: Deighton, Bell, and Co. London: George Bell and Sons. 1888.)

THIS book deserves to be most warmly received by all who are interested in this branch of mathematics, in which remarkably rapid progress has been made of late years. For some time past a constant and familiar acquaintance with the Proceedings of learned Societies has been necessary to enable students to keep abreast with the subject; and the author has performed real service in incorporating in his work many important results and memoirs.

This volume, which is to be followed by a second, contains the general equations of motion, with the auxiliary discussions of vortex and irrotational motion, and also the theory of motion of solids in a fluid, in which both the hydrodynamical and the dynamical effects of the motion are very fully discussed. The chapter on the equations of motion is noticeable for the introduction of Clebsch's transformation, proving the permanency of vortex lines and vortex sheets, and for the application of the principles of least action and energy. Students are apt to lose sight of general dynamical principles in the not inconsiderable difficulties of pure analysis that attend this subject; and it is well they should be aided to bear in mind that their symbols are after all intended to represent physical phenomena, while it adds considerably to the interest of the subject to exhibit its analogies with kindred physical principles. But it is to be regretted that in this chapter the author has not removed the obscurity which arises from the fact that the equations of motion can be obtained in the same form by either a Lagrangian or an Eulerian method. The device of endowing each particle of fluid with co-ordinate axes, all its own, marks the first method; the observation of fluxes at a certain point of space is the distinguishing feature of the second. To identify the results thus obtained (as *e.g.* in the equations of motion in spherical co-ordinates) is justifiable, but will certainly lead to much misapprehension at the outset.

A chapter on images and doublets is useful as collecting together what must otherwise be introduced in a random manner. The discussion of motion in two dimensions is as complete as the limited number of cases that are soluble will allow; several new cases, not previously found in the text-books, illustrate the increasing difficulty of the analysis. In dealing with discontinuous motions the author follows Kirchhoff, beyond whose work on this question it seems impossible to advance.

The second half of the book, treating of the motion of solids in a fluid, is singularly interesting, and contains the last contributions to the dynamical theory, which are due to the author himself. These are marked by great generality of treatment and power of analysis, but we fear the complexity of the results will prevent their being generally appreciated.

In dealing with the velocity-potential due to the motion of an ellipsoid, it would appear that the most direct and general method of obtaining the result in every case is to form Laplace's equation in ellipsoidal co-ordinates; instead of this the author has recourse to formulæ in the theory of attraction, which need modification to suit each special case.

A chapter on the motion of two spheres indicates the attention given to this problem of late years, and may also serve as a warning of its hopelessness. The anticipations of its yielding an explanation of magnetic phenomena, to which the first experiments by Bjerknes gave rise, have been dissipated by the exhaustive mathematical treatment it has received.

The excellence of this work leads us to look forward with great interest to the publication of the second volume, which will deal with fresher and more suggestive portions of the subject; and the two volumes together will prove of very great use to every student. The words on the title-page, "with numerous examples," strike us as below the dignity of a subject like hydrodynamics. The book will certainly be appreciated for its own merits even more than for its examination usefulness, to which aim too many books conform.

OUR BOOK SHELF.

Sierra Leone; or, the White Man's Grave. By G. A. Lethbridge Banbury. (London: Swan Sonnenschein, Lowrey, and Co., 1888.)

THE author of this book explains that he does not offer it as "one of travel over unknown ground" or "as one of dangerous adventures and hardships." His aim simply is to bring before his readers a description of an Englishman's life in "the most interesting but deadly colony of Sierra Leone." He has done his work well, and the book will be cordially welcomed by all who have any special reason for wishing to obtain clear and accurate information about this part of the West African coast. The volume consists chiefly of letters written while Mr. Banbury was at Sierra Leone, and has therefore a freshness and vividness which it would have been hard for him to match in a more elaborate and formal work. The most valuable chapters are those in which he sets forth the impressions produced upon him by the natives, in whose ideas and customs, as here depicted, there is an odd mixture of Christianity and the lowest forms of paganism. Mr. Banbury has a strong belief in the power of education to improve the character of the native population, and he urges that more strenuous efforts should be made for the establishment of proper schools. It is tolerably certain that if permanent good cannot be done to the colony by this means there is no other way in which real progress can be secured, for, as Mr. Banbury points out, the unhealthiness of the climate prevents any large increase of the number of European settlers.

Nature's Fairy-Land: Rambles by Woodland, Meadow, Stream, and Shore. By H. W. S. Worsley-Benison. (London: Elliot Stock, 1888.)

THIS book consists of a series of papers selected from a considerable number which have appeared in various periodicals. The author has a clear, pleasant style, and his vivid descriptions and explanations are well adapted to awaken in the minds of young readers a genuine interest in various aspects of scientific truth. The volume opens with an attractive paper on "The Journeyings of the Rain Drops," and this is followed by papers entitled "From Root to Flower," "Out Among the Gorse," and "Companions of the Corn." These three papers serve as an introduction to other chapters on plant-life. There are also interesting essays on such subjects as shells and shell-builders, spiders, and the nests of fishes.

Lessons in Elementary Mechanics. By W. H. Grieve, P.S.A. (London: Longmans, Green and Co., 1888.)

THE second stage of mechanics is alone dealt with here, and throughout, the author has rendered the various forces which produce motion, together with the laws which regulate those forces, in a clear and simple style; the illustrations are numerous, and are specially adapted to an elementary course. The work is suited to the requirements of the second stage of the revised code, and the arrangement of the chapters is the same as that in the Syllabus of Instruction adopted by the London School Board. The examples at the end of each chapter are instructive and well chosen, and the book concludes with a series of examination papers and results to the numerical questions.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Photography of Lightning.

So much interest is being taken at the present time in obtaining photographs of lightning flashes, that perhaps some one would be willing to take the necessary trouble, and use a moving camera. A camera revolving or vibrating at any ordinary pace would probably give each single flash unaltered, but it might analyse multiple and complex flashes into their constituents.

The eye is so easily deceived as to what is really happening in these sudden effects that very erroneous views may easily be formed, and indeed are in some quarters now prevalent.

Whether it is better to make the camera revolve as a whole, or only the sensitive plate, or whether a revolving mirror should be used with a stationary camera, are questions for experience to decide.

One good method, if not too troublesome in practice, would be to arrange a double camera, with component axes parallel, so as to photograph the same flash in both halves, but with the sensitive plate of one fixed, of the other rapidly revolving. Appearances really due to succession in time could be then easily distinguished, and might be capable of interpretation.

July 10. OLIVER J. LODGE.

Micromillimetre.

I AM glad that the Council and Fellows of the Royal Microscopical Society have seen their way to the adoption of the word *micron*, but the letter in which Mr. Crisp announces this decision to you is not, I think, calculated to give a correct impression of the circumstances under which it was taken.

Firstly, I need hardly say that I did not take exception to the word *micromillimetre*, but to its use as equivalent to the thousandth of a millimetre.

In the next place, I wrote to the Secretaries of the Royal Microscopical Society on behalf of the Council of the Physical Society, of which no mention is made by Mr. Crisp.

Thirdly, the proposal of the Council of the Physical Society was that the word *micron* should be adopted.

Lastly, I am myself quite in favour of this course, and in fact moved its adoption by the Physical Society.

The word *micromētre* must in accordance with the rules of the B.A. Committee be a possible alternative just as a *cubic decimetre* is alternative to a *litre*, and I think the disadvantage of the multiplication of special names not based on a uniform system is nearly as great as that of the possible occasional confusion between micromētre and micrometer. This is however a little matter as compared with the use of *micromillimetre* in two different senses, and the official sanction of *micron* by the French authorities (of which I was not aware when I originally wrote to you) is quite sufficient to turn the scale in its favour.

As some of your readers may not have seen the previous correspondence, will you allow me in conclusion to state that it is now generally agreed,

(1) That the thousandth of a millimetre shall be called a *micron* and denoted by μ.

(2) That the millionth of a millimetre shall be called the *micromillimetre* and denoted by $\mu.\mu$.

ARTHUR W. RÜCKER.

Distribution of Animals and Plants by Ocean Currents.

I beg to forward you herewith some extracts from a letter just received from Port Elizabeth, South Africa, which, I think, cannot fail to interest your readers in connection with Darwin's theory of the distribution of animals and plants in some cases by ocean currents. My correspondent writes :—

"About the beginning of the year 1887 the attention of the public of Port Elizabeth was aroused by finding a quantity of pumice-stone washed up upon the shores of the bay, showing volcanic action. Some of the pieces were covered with barnacles of a few months' growth, and others appeared as though a mass of vitrified matter had been poured upon them. At the same time, shipmasters stated that they had seen large masses floating upon the sea as they approached the east coast of Africa. Strange fish also made their appearance in our waters, and, among the number, two large specimens of the ox-ray species were found washed up upon the rocks. But more remarkable was the discovery of four venomous sea-snakes about 18 inches long, the bodies marked black above and yellow below, answering the description of the *Pelamis bicolor* usually to be found about the coasts of Sumatra, Java, and the adjacent isles, and which must have followed the floating debris. One of these snakes was still alive when found, although it did not long survive, and one of the others was in a sufficient state of preservation to be sent to the Museum. What will prove more interesting still, is the discovery of a large seed resembling a cocoa-nut, which was picked up about the same time, of which Mr. Russell Hallack, of Port Elizabeth, gives the following description :—

"'About the latter end of 1886 a large husky fruit was picked up. It resembled a square cocoa-nut of 4 inches cube, not quite so deep as broad and long. Inside this husk, which was more cork-like than fibrous, was a solitary nut, about $1\frac{3}{4}$ inch round, melon-shaped, with fluted outside, covered with a coating resembling potato-peel. This nut had been bitten by the boy who found it, but whether the taste was not to his liking, or for some other reason, he was persuaded to give the remains to the gardener of the north-end park, who planted it. In due time the shoot came up like a potato-plant with small leaves. The plant is now about 4 feet high, and the small leaves have developed into grand foliage 20 inches long by 7 or 8 broad. It is supposed to be the *Barringtonia speciosa*, a native of the East Indies. A smaller variety, the *B. racemosa*, is said to exist in Natal and the east coast of Africa, but is easily distinguished from this by the smallness of its fruit. The *B. speciosa* belongs to the myrtle tribe, but differs from the ordinary type in having this large, one-sided, corky husked fruit; it is one of the handsomest of its tribe, and in the Moluccas attains the height of 40 or 50 feet, with a circumference of 10 to 14 feet. It is generally found near the sea.'"

The suggestion is that this nut, as well as the snakes, the strange fish, and the pumice-stone, are all relics of the great Krakatão eruption in 1883, and that they had drifted about till the beginning of 1887, till thrown upon the coast of South Africa. If this be really the case, the tenacity of life in the snakes and the nut is truly remarkable, and, as my correspondent adds: "Surely some of this debris must have been deposited on the island shores visited by these currents, and if we could only become acquainted with the date of their appearance upon each, some idea might be formed as to the course taken by these plants, &c., in their journey to Southern Africa."

I find, by a reference to the back numbers of Nature, that the pumice has been traced to the east coast of Africa, leaving portions on various islands *en route*, and that some of it was timed to reach the west coast of America at Panama in 1886; but nowhere do I find any notice, except that given above, of animal or vegetable debris accompanying the masses of pumice. Perhaps the publication of these interesting facts may call forth similar observations from some of the Pacific Islands.

A. W. Buckland.

Watches and the Weather.

My neighbours, Messrs. Jacob and Ross, watchmakers, often tell me their experiences in the breaking of mainsprings.

Unreflecting people fancy they have broken the spring by over-winding, or in other words have drawn asunder a piece of steel by the force of finger and thumb.

The springs of course break through a subtle molecular change produced in the steel by atmospheric causes: they usually fly asunder a few hours after being wound, at 3 or 4 o'clock in the morning. Many watches and clocks come to the workshops for new springs after a frost, but not until a thaw has set in; still more come after thunderstorms.

This morning a clock spring was taken out of its box, which had overstrained itself at one moment into seventeen pieces, there was a complete fracture in each coil along a radial line from the centre. Some time back one was found with three such radial lines of fracture.

Of course this subject is not new, but it gains by recorded experiences.

W. B. Croft.

The College, Winchester, July 9.

Preserving the Colour of Flowers.

I should be greatly obliged if some of your readers would inform me how to preserve the colour of those flowers prone to fade during and after pressing.

In a local paper I saw an extract from the *Pharmaceutical Journal*, in which salicylic acid was recommended. I have tried it both as powder and in solution in spirit; in either case it had a great tendency—except in the case of yellow flowers—to change the colour to either a bright scarlet or to a light brown.

A. W.

[There is no difficulty in preserving the colour of yellow flowers if they are properly dried by the ordinary method, *i.e.* in absorbent paper, changed at the end of the first day, and once or twice afterwards. It is very difficult to prevent such plants as Pedicularis, Bartsia, and Melampyrum turning black. See an account of a plan recently tried in Germany by Schönland, in *Annals of Botany*, vol. i. p. 178, 1887.—J. G. Baker.]

THE LIFE STATISTICS OF AN INDIAN PROVINCE.

SOME years ago, in this journal (vol. xxix. p. 338), I published a short article on the intimate relations which subsist between meteorological conditions and the statistics of death and crime in India. In this it was incidentally mentioned that, imperfect as they were, the vital statistics of the North-West Provinces and Oudh were at that time more to be depended on than those of any other province in India, thanks to the unremitting attention paid to the subject of registration by the late Sanitary Commissioner, Dr. Planck; and though they have not sensibly improved since 1884, but perhaps rather fallen off in accuracy, the birth and death registers of these provinces are still undoubtedly better than any others in India embracing an equal population.

As ten complete years have now elapsed since the amalgamation of the two provinces, which together contain a larger population than any European country except Russia, and as similar statistics are not at present obtainable from any other Oriental country but India, it may be of interest to compare some of the conditions of life revealed by them with those obtaining in the more favoured countries of the West. That India has a high death-rate, owing to the unhealthiness of the prevailing climatic conditions and imperfect sanitation, as well as to the low vitality of the mass of the people consequent upon superabundant population and insufficient food, is universally understood; but there is no proper appreciation of the marvellous recuperative power of a population among whom prudential restraints on increase are unknown, and where almost every woman has been married in childhood, and commences to bear children at the age of fourteen or fifteen years. It may be said with almost absolute truth that there are not only no old maids in India, but no unmarried women above the age of puberty, except the unfortunate class of Hindu widows of the higher castes, who are not permitted to marry again; but though this class appeals in many ways to our sympathies, it is of very slight importance from the point of view of the increase of population, the widows of child-bearing age amounting to only 9 per cent. of the total number of females of the same age—a proportion which compares very favourably with that of widows and spinsters in England. This wonderful power of rapid recovery after decimation by famine or pestilence will be fully exhibited in the tables given below.

The registration of deaths was in regular operation for several years, both in the North-West Provinces and in Oudh, before the two were united under one administration in 1877. That of births was first introduced generally in 1879, though it had been tentatively commenced in municipalities and cantonments some time previously. We have therefore now (February 1888) ten complete years' death statistics for the united provinces, of a fairly uniform degree of accuracy, and nine years' registers of births, decidedly improving in completeness and accuracy for the first five or six years. The births for the first year of the ten—1878—may also be approximately arrived at by a proportionate computation from those registered that year in municipalities. The total number of births of each sex, registered in each of the ten years, was as follows:—

Year.	Males.	Females.	No. of Males to 100 Females.
1878	667,975*	545,285*	122.50
1879	669,921	555,911	120·51
1880	747,953	642,826	116·34
1881	948,191	831,282	114·06
1882	875,616	780,543	112·18
1883	950,932	850,469	111·81
1884	1,015,699	915,262	110·98
1885	957,672	861,609	111·15
1886	874,099	785,433	111·28
1887	902,844	805,891	112·03
Total	8,610,902	7,574,511	113·68

An inspection of the last column shows that these numbers require to be corrected, not only by an allowance for general incompleteness of the records, but by a special addition to counteract the tendency to omit females. During the first seven years this tendency diminished as registration improved, and the numbers of the two sexes approximated more and more to equality; but even with the most intelligent and careful recording agency, the true ratio between the sexes at birth will never be attained in the records until the opinion of the mass of the people on the relative values of male and female life has undergone a complete alteration. The ratios for the first seven or eight years in the table give a curve apparently asymptotic to a certain line, the ordinate of which would stand for the ratio attainable by the greatest care in registration under the present conditions. Representing the above ratios for the first eight years by the formula, $a + \frac{b}{t}$, where t is counted in years from 1877, we find the ordinate of the asymptote, a, to be 108·57. In the provinces there are, however, two districts in which the numbers born of the two sexes invariably approach much more nearly to equality. One is Garhwál, a Himalayan district inhabited by an unsophisticated people who claim to be *Rájpúts*, but are probably of aboriginal descent, and who have never come under Muhammadan influence in any way, or acquired the custom of paying a heavy dowry with the bride, which is the cause of female infanticide among many of the higher castes. The other is Lalitpur, in the extreme south, where the inhabitants are chiefly *Chamárs* and other low castes, who have never concealed their women or practised infanticide, and amongst many of whom the bridegroom's family pay for the bride. The statistics for these two districts give a series of ratios represented by a curve whose asymptote has an ordinate of 100·00, or which points ultimately to exact equality between the sexes. In like manner, if we select for each year that district in which the recorded birth-rate was highest, and where, therefore, the registration was presumably most complete, we get a curve pointing to an ultimate ratio of 102·78 males to 100 females. If we take the mean of all three results, that for Garhwál and Lalitpur being probably below the true average for the whole population, we get 103·78 males to 100 females. This comes very near the ratio for England, which, I believe, is between 103 and 104, and is almost identical with that deduced from the distribution of the population according to age and sex at the last two censuses of the North-West Provinces—namely, 103·75. It may therefore be adopted as a close approximation to the truth, and it shows that, in regard to the relative numbers of the sexes, human nature is much the same in the East and West, notwithstanding the deceptive appearance presented by unanalyzed statistics, as well as by public gatherings in countries where respectable women seldom venture out of doors.

* Estimated from those registered in municipalities.

The numbers of females in the above table must therefore be all recast so as to give 103·78 males for every 100 females.

This special inaccuracy in the birth tables being corrected, there remains the general inaccuracy due to incompleteness of the register, which is common to both births and deaths, and has been estimated by Dr. Planck, after careful and extended personal inquiry, at 20 per cent. of the total, or one-fourth of the numbers recorded. When both causes of error are allowed for, the total number of births in each year will be as in the second column of the next table. The third column gives the recorded deaths, increased by 25 per cent. to make them represent approximately the true mortality, and the last shows the increase or decrease of population each year, due to these causes. The figures in this column represent very fairly the total gain or loss of population, for the number of emigrants is only three or four thousand annually, and this loss is partly balanced by a return migration, the numbers of which are not known.

Year.	Births.	Deaths.	Increase.
1878	1,639,544	1,902,175	−262,631
1879	1,644,320	2,393,124	−748,804
1880	1,835,850	1,601,544	+234,306
1881	2,327,335	1,753,091	+574,244
1882	2,149,219	1,856,409	+292,810
1883	2,334,062	1,520,371	+813,691
1884	2,493,036	1,944,177	+548,859
1885	2,350,606	1,763,299	+587,307
1886	2,145,476	1,834,516	+310,960
1887	2,216,030	1,977,174	+238,856
Total	21,135,478	18,545,880	+2,589,598

During the year of scarcity, 1878, and that of pestilence, 1879—for the great epidemic of unprecedentedly fatal malarial fever that year surely deserves the name of pestilence—the net loss of population was over a million; but in the next three years this was fully recovered, and in the succeeding years large numbers were added to the population, especially in the healthy year, 1883. Thus the net gain for the ten years, notwithstanding famine and pestilence, was over two millions and a half, an increase almost unprecedented since the first census in 1853, and doubtless the result of an unusually long succession of abundant harvests. Since 1885, however, the increase has grown less and less rapid; and as another scarcity is now nearly due, if any trust may be placed in the average period of the recurrence of droughts in the past, it seems likely that in the next two or three years the increase may be temporarily stopped.

With these figures, and the fixed point given by the census of 1881, it is possible to find the probable number living at the commencement of each year from 1878 to 1888, and also the mean birth- and death-rate for each year of the ten. The census was taken on the night of February 17, 1881, and the total of the people numbered was 44,107,869. In the Census Report it is, however, shown that over a million females between the ages of 5 and 20 must have escaped enumeration; and when allowance is made for them, the probable accurate total comes out 45,232,391. During January and the first seventeen days of February the increase was 118,532; so at the beginning of 1881 the population stood at 45,113,859. From this starting-point the following figures have been worked out:—

Year.	Number living at Commencement.	Year.	Number living at Commencement.
1878	45,890,988	1884	46,794,604
1879	45,628,357	1885	47,343,463
1880	44,879,553	1886	47,930,770
1881	45,113,859	1887	48,241,730
1882	45,688,103	1888	48,480,586
1883	45,980,913		

The mean number living during the ten years was 46,478,714.

The total area of the united provinces is given in the Census Report as 106,104 square miles. The population is thus at the present time about 457 to the square mile, including in the average the Himalayan province of Kumaon, over 12,000 square miles in area, where the average density is less than 90 to the square mile. There is practically no export trade, except in agricultural produce; hence the whole population is supported directly or indirectly by the agriculture of the province; and there is probably no purely agricultural country in the world, except perhaps some parts of China, where so dense a population is maintained.

The birth- and death-rates and rate of increase or decrease each year, calculated on the usual basis of 1000 living, are given in the next table.

Year.	Birth-Rate.	Death-Rate.	Rate of Increase.
1878	35·83	41·57	− 5·74
1879	36·33	52·88	−16·55
1880	40·54	35·37	+ 5·17
1881	51·26	38·61	+12·65
1882	46·89	40·50	+ 6·39
1883	50·32	32·78	+17·54
1884	52·97	41·31	+11·66
1885	49·35	37·02	+12·33
1886	44·72	38·24	+ 6·48
1887	45·82	40·88	+ 4·94
Mean	45·40	39·91	5·49

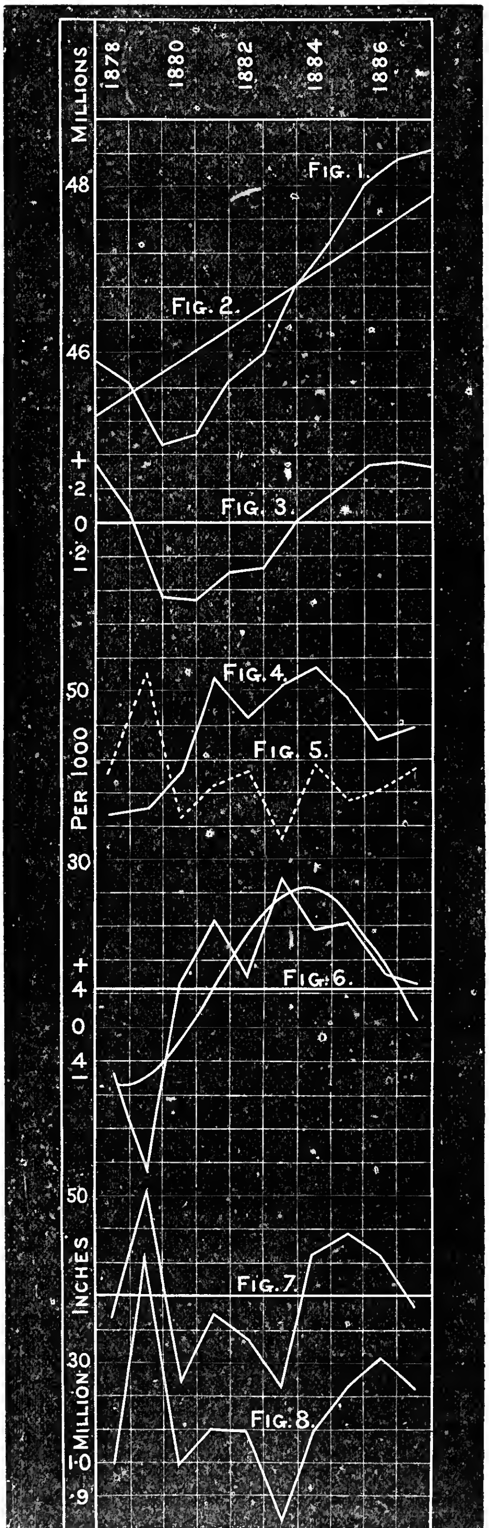
The birth-rate, even in the worst years of the ten, was as high as in England, while in the best years it was about 50 per cent. higher. The death-rate averaged nearly forty per mille, and therefore, notwithstanding the high birth-rate, the population increased only at the rate of 5·5 per thousand per annum.

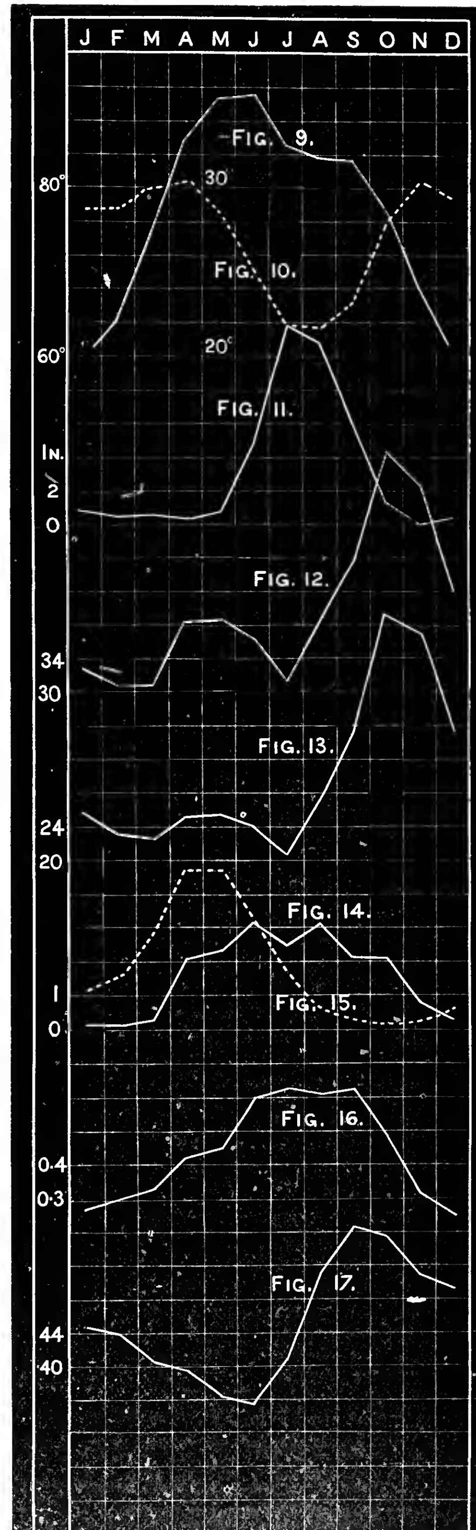
A glance at the annexed diagram will render these data more intelligible. Fig. 1 exhibits the movement of the total population from year to year; and the nearly straight line, marked Fig. 2, shows what this movement would have been had it proceeded uniformly at the rate of 5·49 per mille per annum. The curve has been computed by the formula $P = P_0 r_n$, where n is counted from the beginning of 1883, and consequently the ordinate for 1883, P_0, is the geometrical mean of all the ordinates of Fig. 1. The differences of the ordinates of the first two curves are charted in Fig. 3, which therefore exhibits the extent to which the actual population exceeds or falls short of that given by a uniform movement. This is apparently a periodic function of the time; and if so, the period does not differ much from ten years, since the last ordinate is only slightly greater than the first. Figs. 4 and 5 represent the birth- and death-rates respectively. At first sight these appear to have no relation to each other, as concomitant and opposed variations are nearly equal in numbers. The years of lowest death-rate, 1880 and 1883, were, however, followed by the years of highest birth-rate, showing that healthy conditions conduce to fecundity as well as diminished mortality. An exception to this rule is, however, found in 1879, which, following a healthy year, should have had a high birth-rate, but was marked instead by an exceptionally low one. The year 1878, though a dry and very healthy year, was one in which the vitality of the people reached a low ebb, owing to long-continued scarcity, approaching in some places to famine; for, though few or none actually died of starvation, millions were for many months at starvation-point.

The annual rate of increase per thousand, shown in Fig. 6, is probably the best possible measure of the general well-being of the people, combining as it does the effects of abundance or scarcity of food, which influence the birth-rate, with those of health and disease, on which the death-rate depends. Curiously enough, this index of general prosperity or the reverse is much less liable to sudden fluctuations than the birth- or death-rate alone, and yet, like the numbers represented by Fig. 3, it is apparently subject to a periodic oscillation about a mean value. The length of the period is probably something over ten years, since the last year gives a considerably greater result than the first, though it exhibits a downward tendency. It is therefore possible that the rate of increase of a primitive people, living a natural life untrammelled by too much civilization, and multiplying up to the limit of the means of subsistence, may be subject—like the prices of grain, investigated by Mr. E. Chambers and the late Mr. Stanley Jevons, and like many other terrestrial phenomena—to a periodic variation determined by that of the energy received from the sun. Assuming that there is a variation with a period of eleven years, the rates of increase charted in Fig. 6 lead to the formula, $r = 4·576 + 11·725 \sin\left(\frac{2\pi n}{11} + 262\frac{1}{2}°\right)$. This formula gives the smoothly flowing curve of Fig. 6, which coincides, as fairly as may be, with the curve of actual variations. For the minimum epoch the formula gives 1878·73, and for the maximum 1884·23,—dates which fall suggestively near those of the corresponding phases of solar disturbance.

Into this interesting speculation it is impossible at present to enter further, beyond remarking, as was said at the beginning, that the increase of the population during the last ten years was probably above the average, and too rapid to be maintained. The hypothesis that it is subject to a variation in the eleven-year period leads to the result that the mean for a long term of years is only 4·576 per thousand, instead of 5·49. Now, in the Report on the Census of 1881, the Census Officer, Mr. Edmund White, calculated that the population, as reported, increased only 2·33 per thousand per annum between 1853 and 1881; but it is pointed out that this result was vitiated by an over-estimation in 1853, when the individual members of the population were not counted by name, but only the total number of each family was entered in the census forms. In the sixteen years from 1865 to 1881 the rate of increase was 4·48, and as these years included a fair proportion of good and bad, the rate of movement is probably near the truth. It differs only by a small fraction from the mean rate given by the above formula, according to which the population might be expected to double itself in 152 years, notwithstanding the already great pressure on the soil. In the same Census Report, from the distribution of the population according to age, a mean death-rate of 39·5 per mille is arrived at. This agrees sufficiently closely with the rate here found to warrant the conclusion that the corrections applied to the numbers actually registered cannot be far wrong.

Fig. 7 shows the variations of the average rainfall of the province, for which the general mean of the ten years


Millions
1878
1880
1882
1884
1886
48
Fig. 1.
Fig. 2.
46
+ 2
0
1·2
Fig. 3.
50
Fig. 4.
Fig. 5.
Per 1000
30
+ 4
0
1·4
Fig. 6.
50
Inches
Fig. 7.
30
Million
Fig. 8.
1·0
·9


J F M A M J J A S O N D
Fig. 9.
80°
30°
Fig. 10.
60°
20°
Fig. 11.
In.
2
0
Fig. 12.
34
30
Fig. 13.
24
20
Fig. 14.
1
0
Fig. 15.
Fig. 16.
0·4
0·3
Fig. 17.
44
40

was almost exactly 38 inches. Neither the birth-rate nor the rate of increase has any distinct relation to the rainfall, but there are very evident indications of such a relation as regards the rate of mortality. The first seven years witnessed great fluctuations in the rainfall, and these were almost exactly parallel in the death-rates, the wettest years being those of greatest mortality. During the last three years, on the other hand, the death-rate increased slightly as rainfall diminished, and *vice versâ*. Amongst the principal causes of death, cholera and small-pox vary enormously in their prevalence from year to year, these diseases being of an epidemic nature; but the variations do not seem to be related in any way to the rainfall. On the contrary, those from endemic malarial fevers, represented by Fig. 8, follow the rainfall variations very closely, and they are the chief factors in the general mortality.

In my former article, the death-rates from various causes were compared with the prevailing meteorological conditions, not year by year, but month by month. If a similar method be adopted with the statistics of the ten years now available, the conclusions arrived at in the former paper are fully borne out as regards all their more important points, only needing, in one or two instances, slight modifications in detail.

The next table gives the total mortality for each month, computed as a rate per mille per annum, and also the rates for certain specified causes of death, which can in most cases be recognized by the recording agency. In computing these, the registered numbers have all been increased by an allowance for omissions similar to that given above.

Month.	Total Mortality.	Fevers.	Cholera.	Small-pox.	Suicide.	Wounds or Accidents.
January ...	32·71	25·64	0·08	1·16	0·032	0·236
February ...	30·85	22·95	0·08	1·65	0·036	0·255
March ...	30·94	22·26	0·31	2·71	0·062	0·276
April ...	38·26	25·46	2·04	4·79	0·087	0·325
May ...	38·40	25·55	2·32	4·75	0·080	0·366
June ...	36·29	24·12	3·11	3·20	0·083	0·508
July ...	31·41	20·88	2·59	1·71	0·078	0·543
August ...	38·67	26·94	3·08	0·65	0·077	0·535
September ...	45·27	34·39	2·19	0·26	0·084	0·532
October ...	58·60	48·77	2·06	0·12	0·076	0·404
November ...	54·67	46·07	0·79	0·18	0·050	0·276
December ...	42·53	36·12	0·36	0·51	0·034	0·228
Year ...	39·90	29·98	1·58	1·81	0·066	0·374

This table shows how utterly insignificant as causes of death are cholera and small-pox, the two most dreaded diseases, by the side of fevers, which account for three-fourths of the total mortality.

The monthly mean values of the three chief climatic factors for the last ten years may be compared with the preceding figures. They are:—

	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Year.
	°	°	°	°	°	°	°	°	°	°	°	°	°
Mean temperature ...	59·4	64·5	75·0	85·7	90·7	91·2	85·0	83·7	83·2	77·8	67·9	60·3 ...	77·0
Range of temperature ...	27·5	27·4	30·0	30·7	27·1	21·3	13·9	13·1	16·0	25·3	30·7	28·9 ...	24·3
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
Rainfall	0·87	0·53	0·56	0·22	0·84	4·60	11·91	10·63	6·07	1·38	0·06	0·42 ...	38·09

These are computed on the average of the whole province, exclusive of the higher hill stations.

The relations of the rates of mortality to these climatic causes will be best seen from the curves marked Fig. 9 to Fig. 16. The general death-rate, and that of fever mortality, which follows it closely, have two maxima in October and April or May, and two minima in July and February or March. The secondary maximum in May is a very important feature in the fever curve (Fig. 13), but rises noticeably above the preceding minimum in Fig. 12, which represents the general mortality, owing to the influence of small-pox and, to a small extent, of cholera. The fatal prevalence of fever seems to be altogether uninfluenced by the temperature (represented by Fig. 9), and its variations are almost exactly opposed in phase to those of the rainfall, shown in Fig. 11; but the maxima and minima nearly coincide in time with those of the daily range of temperature (Fig. 10). These statistics therefore confirm the general experience that people are most subject to fever when the nights are chilly and the days hot. If we neglect the secondary maximum in the hot season, Figs. 11 and 13, representing rainfall and fever mortality respectively, will be observed to be almost identical in form, except that the latter is displaced three spaces to the right. This means that malarial fevers are directly dependent on rainfall, in their annual variation as in the variations from year to year; but it takes about three months in Northern India for the malarial conditions brought about by the rainfall (which probably depend on the growth and decay of vegetation) to attain full development. Dr. Meldrum has shown, in several of his annual reports, that in Mauritius the highest fever mortality follows the maximum rainfall at an interval of about two months, and in Northern India a parallel rule seems to hold, except that the interval is slightly longer.

The meteorological conditions predisposing to cholera are evidently heat and moisture, the disease being more prevalent than usual during the whole hot season from April to October, and dying out in the winter. The cholera curve (Fig. 14), which is drawn on a scale four times more open than that for the total mortality, has two maxima in June and August, and a secondary minimum in July. In most years the first maximum falls in April or May, but it has been thrown forward to June in the average for the last ten years by the excessive mortality of last June, when over 50,000 deaths from cholera were registered.

Regarding small-pox (Fig. 15), shown on the same enlarged scale as cholera, all the remarks in my former article hold good. This disease is at a maximum in April and May, and it diminishes rapidly during the rains, until it almost dies out in October and November. The conditions most favourable to its spread seem to be a high wind, and very dry, or perhaps rather very dusty, air; and the number of fatal cases may be almost exactly represented as a direct function of the wind velocity and the dryness of the air. This result is completely in accordance with all that is known about the cause and mode of propagation of the disease.

Fig. 16 gives the annual variation of the deaths by violence, including under this head both suicides and wounds. The curve is a very smoothly flowing one, with a distinct annual minimum at the coldest time of the year, a steady rise through the dry hot season, and relatively high ordinates throughout the rainy season. The scale of the curve is ten times more enlarged than that of the cholera curve, or forty times larger than the scale of total mortality or fever; but though this magnification renders the annual variation visible, it does not reveal any irregularity except a slight increase in September, when, owing to a long "break," or the premature cessation of the rains, the weather sometimes becomes very much hotter than in July or August. This September maximum is more distinct in the suicide ratios than in those of deaths from wounds. It appears, therefore, that these fatalities from crime, instead of disease, are distinctly subject to climatic causes, and the explanation given in my former article, which attributes them to irritability of temper consequent on long-continued heat and moisture, is the best I can put forward.

When the birth statistics are analyzed with reference to the annual period, results equally striking and curious are brought out. The numbers registered, when tabulated month by month, corrected for the causes of error mentioned at the commencement of this article, and thrown into the form of average rates per thousand per annum, give the following table, in which also the monthly ratios, which are for nine years only, have been slightly altered so as to make the annual mean equal to that already found for ten years:—

Month.	Males.	Females.	Total.	Number of Males to 100 Females.
January	22·67	21·92	44·59	103·42
February	22·31	21·53	43·84	103·67
March	20·72	19·95	40·67	103·86
April	20·17	19·30	39·47	104·51
May	18·46	17·64	36·11	104·65
June	18·12	17·30	35·43	104·74
July	20·80	19·70	40·50	105·59
August	25·81	24·72	50·53	104·41
September	28·85	27·86	56·71	103·55
October	28·30	27·41	55·71	103·25
November	25·89	25·15	51·04	102·94
December	25·36	24·88	50·24	101·52
Year	23·12	22·28	45·40	103·77

From the existence of the *Holi* festival among the Hindus, and of similar spring festivals, accompanied with lascivious songs and dances, among many barbarous tribes, as well as from the traces of such festivals still surviving in Europe, and the hints given by classical writers regarding the nature of certain annual religious mysteries performed by the early Greeks and Romans, anthropologists have thought that possibly, during prehistoric times, the human species, like the lower animals in a state of nature, had an annual pairing-time. If any traces of such a condition still survive, we may with some confidence look for them in India, where a large number of the poorer classes are chronically on the verge of starvation, and the different seasons are sufficiently marked in character to affect people differently both in body and in mind. The birth-rates in the above table, represented by Fig. 17 in the diagram, exhibit a most distinct annual variation, smoother and more uniform in character than any of the mortality curves, and with a range equal to nearly 50 per cent. of the mean value. The minimum falls in June and the maximum in September,—dates which point to a maximum of conceptions in December, and a minimum in September. The latter month is near the end of the long and depressing hot season, when malarial influences are rapidly increasing to a maximum, the food-supply of the year is nearly exhausted, and there is the greatest tendency to suicide. The births, as well as the deaths, therefore, show that at the end of the rains the vitality and energy of the people have reached low-water mark.

In December, on the other hand, not only is the salubrity of the country greatly increased, as shown by the rapid diminution of nearly every cause of death, but food is again cheap and abundant. The crops of millet, on which the poorer classes live, are sown in July and reaped in November. During December and the latter half of November they are threshed out, and then is the season for paying the village functionaries and labourers their share of the produce. Consequently food is more abundant at this time of the year than at any other, and as a result of these conditions we find a large number of births the following September and October.

It thus appears that among the poorest of the population there is probably still a more or less distinct annual reproductive season, but instead of being determined by the returning warmth of spring, as must have been the case in prehistoric Europe, it follows the annual return of healthy conditions with abundant food-supply. That the *Holi* festival occurs in spring, instead of in December, is perhaps to be accounted for as a survival from a time when the ancestors of the Hindus lived in a colder climate.

In the last column of the table are given the monthly values of the ratio of males to females at birth. This appears to be subject to a small but distinct annual variation, with a maximum in July, and a minimum in December; but whether this is a remote and obscure physiological effect of the annual march of the seasons, or only a chance arithmetical result, I cannot say.

Allahabad, February 8. S. A. HILL.

ON THE ORBITS OF AEROLITES.[1]

MY studies have led me to the following three propositions:

1. The meteorites which we have in our cabinets and which were seen to fall were originally (as a class, and with a very small number of exceptions), moving about the sun in orbits that had inclinations less than 90°; that is, their motions were direct, not retrograde.

2. The reason why we have only this class of stones in our collections is not one wholly or even mainly dependent on the habits of men; nor on the times when men are out of doors; nor on the places where men live; nor on any other principle of selection acting at or after the arrival of the stones at the ground. Either the stones which are moving in the solar system across the earth's orbit move in general in direct orbits; or else for some reason the stones which move in retrograde orbits do not in general come through the air to the ground in solid form.

3. The perihelion distances of nearly all the orbits in which these stones moved were not less than 0·5 nor more than 1·0, the earth's radius vector being unity.

The first and thirds propositions are limited strictly by their terms to the meteorites from stone-falls actually witnessed, and also represented by specimens in some one or more of existing collections. The investigations that have led to them have been limited to the same stone-falls. This is not because any line of separation is suspected to exist astronomically between the stone-furnishing and detonating meteors, or even between them and the shooting stars, but because, for manifest reasons, any facts established about these stones have a greater value than similar facts about meteors from which no stones have been secured.

About 265 observed falls are represented by specimens in existing collections. The history of these falls I have searched out with no little pains, so far as the material for such history could be found in books accessible to me. Every direct statement and every indirect indication which I have obtained about the paths of these meteors through the air have been carefully considered, and their meaning and value duly estimated. The determination of the path of a stone-furnishing meteor through the air is greatly aided by the fact that we know at once one point of the trajectory, viz.: the point where the stone strikes the ground. To this fact may usually be added another, viz.: that some of the observations are by persons near the place of fall, and hence their statements of direction, so far as we may trust them, have peculiar significance. In individual cases it will be found that not much reliance can be placed upon the asserted direction of the meteor's motion. But when the results are all

[1] "Upon the relation which the former Orbits of those Meteorites that are in our collections, and that were seen to fall, had to the Earth's Orbit" by H. A. Newton. (From the *American Journal of Science*, July 1888.)

collated there is such a general agreement in support of the first and third propositions set forth above that I am very confident that they are true.

The orbit of a meteoroid about the sun is wholly given when we know these three things, the time when it enters the air, the direction of its motion, and the velocity. The velocity cannot be easily measured directly. But the connection between meteors and comets will be assumed as fully proven. The velocity of the meteoroids (neglecting the increase due to the earth's attraction), ought then to be that of the comets, at the same distance from the sun. The greatest cometary velocity at the distance unity is $\sqrt{2}$, the earth's velocity being unity. The smallest velocity for any known comet is that of Encke's comet, which at the earth's mean distance from the sun is 1·244. It seems safe, therefore, to assume that the meteorites we are considering had velocities relative to the sun not greater than 1·414, nor less than 1·244.

The direction of a meteor's motion through the air is to be determined solely by the evidence of observers of the stone-fall. This evidence needs to be carefully collated, especially when statements apparently conflict. A judicial temper of mind must be preserved in estimating the meaning of the statements, lest the evidence be twisted to the support of some preconceived notion. Knowing the danger, I have tried to keep my own mind free from bias.

We need not know the *exact* day, but we must know the time of day of the stone-fall, else the direction through the air cannot be used. This throws out about one-fifth of the total number of falls named above,—there being no statement of the time of day of the fall attainable. There are left 210 different cases available for use. For 94 of these there is no reliable statement of the direction of the motion of the meteor. We know only the day and the hour. Even this, however, is of some value, since we know that the meteor must have been moving downward at the place of fall; that is, from some point of the heavens then above its horizon. For 116 stone-falls the direction of the motion of the meteor is more or less definitely indicated by the statements of observers, or by the statements of those who have inquired into and reported the facts of the falls.

We may then divide the observed stone falls into three groups which will be separately considered: (*a*), 116 falls for which we have statements as to the direction of the path through the air; (*b*), 94 falls of which we know the time of day; (*c*), 50 or more falls of which the history is too scanty to give the time of day.

There is frequent occasion to speak of two points on the celestial sphere for which the English language has no good names. These are the point from which a body is moving, and the point to which a body is moving. These two points are opposed to each other, as north is to south, east to west, zenith to nadir. The words *quit* and *goal* will be used to denote these two points. The *earth's quit* is that point of the ecliptic from which the earth is moving, the *earth's goal* that point to which the earth is moving; the one being about 90° ahead of the sun in the ecliptic, the other 90° behind it. A *meteor's quit* is that point of the heavens from which the meteor is moving; its *goal* that point of the heavens to which it is moving. The motion may be that relative to the earth, in which case the point of the celestial sphere from which it is moving is the meteor's *relative quit*. Thus the relative quit of a meteor when it is entering the air must be above the horizon of the place of entrance, inasmuch as the meteor must be moving downward. If a meteoroid's motion be corrected for the earth's motion the direction of its absolute motion about the sun is obtained, and then the two points of the celestial sphere from which and to which the meteoroid is moving are its *absolute quit* and its *absolute goal*.

The observations have been treated graphically. They do not demand nor do they admit of greater accuracy in methods of discussion than can be used in graphic processes, and these processes have many advantages over numerical computations. A stereographic projection of two hemispheres was prepared and printed, upon which there were three sets of coordinate lines from three sets of poles. The three sets of points were the angles of triquadrantal triangles. Thus the lines were drawn to represent at intervals of 10° the distances and directions from the poles P, P, S, E, and G, Q, (Fig. 1). In the engraved figure these coordinate limes are omitted. The common diameter of the two hemispheres E S E was made to represent the ecliptic, and the sun was placed at the centre or at the edge of one of the hemispheres. The points P would then be the poles of the ecliptic, and if S be the place of the sun the earth's quit will be Q, and the earth's goal G.

To treat any single meteor a large celestial globe was first set for the time and place of the fall. Upon the globe the celestial latitude and longitude of the zenith and of the west-point were then measured. The day of the year gave the sun's longitude. The zenith and west-point could then be marked upon the chart, after which it was easy to draw the circles representing the meridian and the prime vertical. The stereo-graphic projection was peculiarly advantageous in this work as all circles are represented by circles, and angles are conserved in the projection. The effort was then made to mark upon the chart the meteor's relative quit as accurately as the observations permit, or rather to describe an area within which the quit was probably or certainly located

Some of the 116 meteorite quits have been heretofore fairly well determined by other persons, or they can be so determined. This is the case with the meteors of Agram, Weston, Orgeuil, Pultusk, Iowa, Rochester, Estherville, Krahenberg, Khairpur, Vendome, etc. For other cases we are able by comparing the various statements of observers to locate approximately the relative quit. But for a considerable number of the falls we have to be content with the simple statement that the stones came from the north, or from the northeast, or from the south-southeast, or from some other similarly defined direction. When this has been the case I have taken a point 20° above the horizon in the direction indicated, and considering this as the centre of an area of considerable size within which the quit was probably located, have treated the point itself as the meteor's quit.

These observations of direction in some cases will be in error, or will be perverted in reporting, as every one who has tried to reconcile numerous accounts of a meteor has unpleasantly learned. But when the statements have come from persons who saw the stones come down, they are usually of much more value than similar reports about ordinary meteors. In any case when the reports are single they must be taken for what they are worth. I have plotted them as given.

In several notable instances where there are full accounts I have not been able to accept the conclusions heretofore arrived at as to the direction of the meteor's path. . Thus, Dr. Bowditch made the path of the Weston meteor to be from north to south and parallel to the horizon. I make it to have moved from a point N. 40° W., 35° high. The Cold-Bokkeveld meteor was described by Sir Thomas Maclear as moving from the west-north-west. It apparently moved in the opposite direction; that is, from the east-south-east. The l'Aigle meteor was described by M. Biot as moving from the south-south-east, whereas it is well nigh certain that it came from the north-west. In like manner the Stannern meteorite was assumed by von Schreibers to have come from the north-north-west, whereas there are reasons of great weight for believing that it came from the opposite direction. I may add that these and other like changes are not made under any pressure or bias to prove my propositions. In fact three of the four changes just named make the evidence for my conclusions weaker instead of stronger.

In the treatment of the observations several quantities have been neglected as not large enough to be comparable with the probable errors of the observations themselves. Thus the effect of the earth's attraction in changing the direction of motion, or what has been called the *zenithal attraction* of the quit, has been allowed for only in a general way. So the earth's quit and goal are treated as being exactly 90° from the sun; or, in other words, the earth's orbit has been treated as a circle. In like manner the motion of the place of fall due to the earth's rotation on its axis has not been taken account of.

Having located upon the chart the meteor's relative quit we have next to construct its absolute quit. This evidently lies on the great circle joining the relative quit to Q (Fig. 1), which, when the sun is at S is represented on the chart by a straight line through Q, together with its corresponding line through G. When the absolute velocity of the meteroid in its motion about the sun is given, the place on this circle of the absolute quit can be determined by combining by the parallelogram of velocities the motions of the earth and of the meteoroid. The following table is an abstract of a larger one used in this reduction, and is constructed for the limiting velocities 1·414 and 1·244:—

Table showing the Distances from the Earth's Quit to the Absolute Quit of a Meteoroid for Different Distances from the Earth's Quit to the Relative Quit of the Meteoroid.

Distance from Q to relative quit.	Distance from Q to absolute quit. $v = 1{\cdot}414$.	$v = 1{\cdot}244$
30°	9°·3	6°·3
60	22·1	15·8
90	45·0	36·5
120	82·1	75·8
150	129·3	126·3
180	180·0	180·0

In the following constructions the maximum velocity of the meteoroid has been used. When the meteoroid's

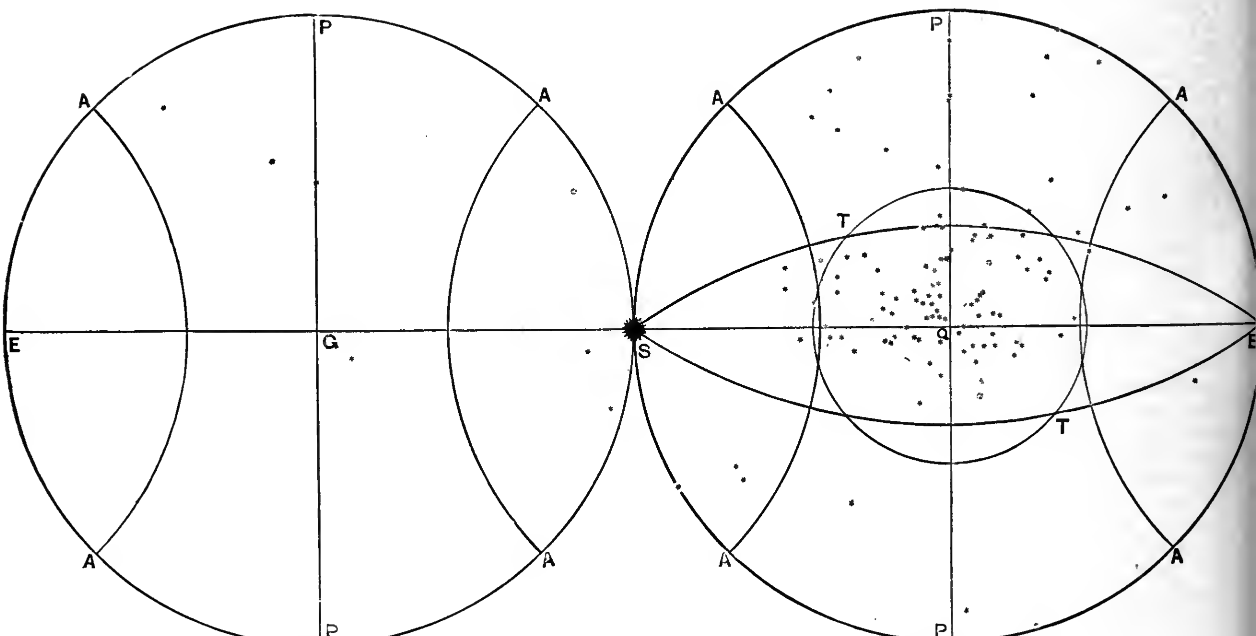


FIG. 1.—Showing the distribution of 116 meteorite quits relatively to the sun's place and to the earth's quit.

relative quit is known as a point the absolute quit is at once constructed. If, however, we have an area within which the relative quit is probably located we may mark off with equal facility points on the boundaries of the area within which the absolute quit is probably located. If the former area is a circle the latter will be an oval. The centre of the circle does not correspond exactly to the centre of the oval, but by applying a correction to the table the centre of the oval absolute quit area can be directly constructed from the centre of the circular relative-quit area.

In Fig. 1 I have given in a single diagram constructed on a stereographic projection, the results for 116 stone-falls. The best determinations which the accounts admit of for the meteor's direction were first made out. Then the centre of the probable quit area in each case was assumed to be the actual quit. When only the quarter of the heavens from which the stones came is stated the centre of probable area was taken 20° above the horizon. Interpreted thus, the stars in Fig. 1 represent the places of the 116 absolute quits relatively to the place of the sun, S, and to that of the earth's quit and goal, Q and G.

Let us denote any one of these quits (or stars), by the letter q. The elements of the orbit in which the corresponding stone was formerly moving can be easily obtained from the projection. The earth's longitude on the day of fall is the longitude of the node. The angle qSQ is the inclination of the orbit to the ecliptic, and its amount is at once read off on the projection. The orbit has been assumed to have been a parabola. Hence, twice the complement of qS was the angular distance of the stone from its perihelion. If $q\text{S} > 90°$, the perihelion had not been reached; if $q\text{S} < 90$, the perihelion, had been passed. The perihelion distance was $\sin^2$QS. If, however, it be assumed that the orbit was a long ellipse of given major axis, the place of the absolute quit, q, moves somewhat nearer to Q along the line qQ, the angle in the plane of the orbit from perihelion was a little more than twice the complement of qS, and the perihelion distance somewhat less than $\sin^2 q$S. But all these quantities are easily computed in terms of the assumed major axis. With a semi-major axis as large as 5 the change in Fig. 1 would not be so considerable as to modify any conclusions we can deduce from the grouping of the stars.

The most noticeable fact revealed by the figure is the clustering of the stars about the point Q. All but seven of the 116 meteor quits are in the Q hemisphere; that is, had orbits whose inclinations were less than 90°. One hundred and nine followed the earth, seven met it. Again the two lines STE are drawn to represent circles inclined 35° to the ecliptic. More than two-thirds of the meteor quits lie between these two lines; hence, over two-thirds of the orbits were inclined less than 35° to the ecliptic, the motion being direct.

It should be said that this clustering of the points near Q is somewhat exaggerated in the figure by the nature of the stereographic projection. The scale of distances near Q differs from that near the circumference. But this does not affect the distribution between the hemispheres.

It has been assumed that certain centres of quit areas were themselves the quits. Can the condensation of the quits near Q have been caused in any way by this assumption? Or, is it possible that general errors of observation, or inaccuracy of reporting, could have been the cause? To answer this question let us suppose that there had existed a law that led to condensation of the relative quits in any manner whatever. The effect of the errors of observing or reporting, and also the effect of the assumption above stated, would be toward scattering these relative quits over the heavens more equably, and thus masking the law. Then when the relative quits thus unduly scattered are reduced to absolute quits there might be as a result a tendency towards condensation near Q. If, however, we draw the circle TT, enclosing those absolute quits whose relative quits are in the hemisphere next Q, the general tendency of the errors in question would be towards equalizing the number of absolute quits within to those without the circle TT. Now, the number of stars is nearly twice as great within

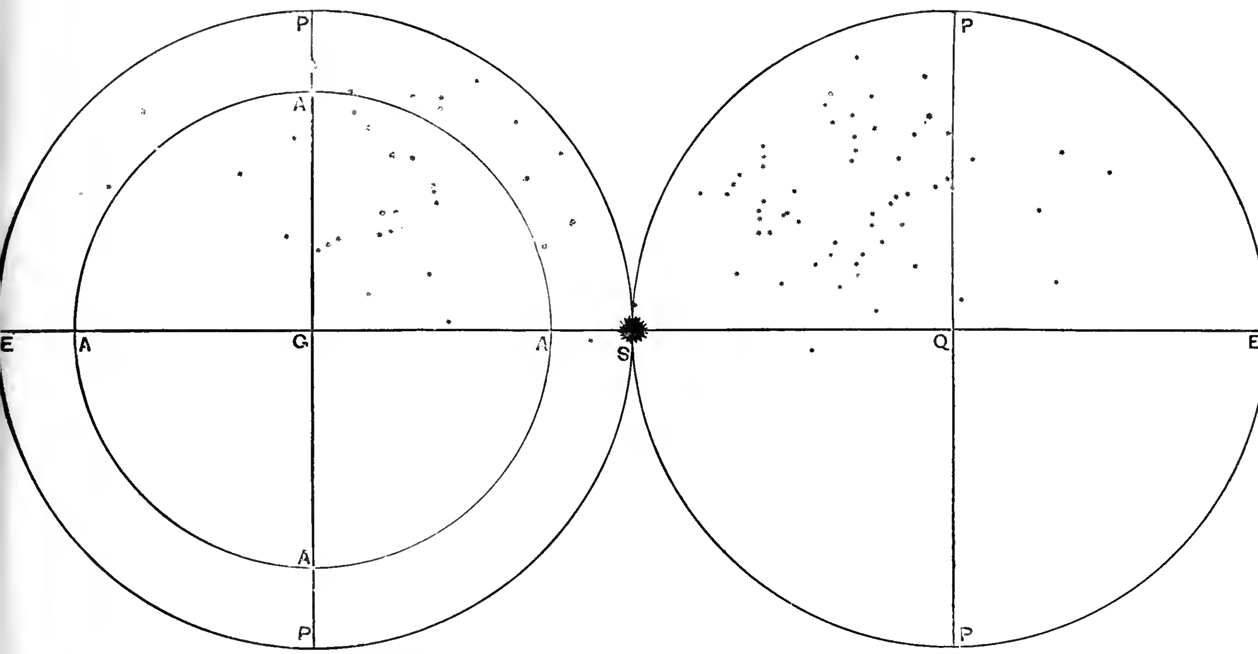


FIG. 2.—Showing relatively to the sun's place, the zeniths for the time and place of 94 stone-falls.

as without the circle. The condensation about Q, shown in Fig. 1, exists therefore in spite of, and not in consequence of, these errors. With a good deal of confidence do I conclude that these 116 meteors were, as a class and with probably a very few exceptions, before coming into the air following the earth in its orbit about the sun.

Another fact of great interest is also shown by the grouping of the points in Fig. 1. In general these stones did not go in their orbits very near to the sun. Assuming that the orbits were parabolas we have for all the stones whose perihelion distances were less than one-half, $\sin^2 qS < \frac{1}{2}$. If there be drawn circles, AA, AA, 45° from S and from E, then will all the stones whose absolute quits were in the central zone, A P P A A A which is bounded by the circles A A, have perihelion distances greater than one-half and less than unity. Of these there are 103 out of a total 116. If the same orbits are assumed to have had semi-major axes equal to 5, then the circles A A would have to be drawn a fraction of one degree farther from S and from E to serve as the limiting curve to orbits whose perihelion distances exceed one-half.

It appears from Fig. 1 that these 116 stones were, with a few exceptions, following the earth in their orbit about the sun. This could happen from either one or more of three possible causes:

Firstly, that nearly all the stones in the solar system are moving in direct orbits, very few in retrograde orbits;—

Or, secondly, that stones moving in retrograde orbits for some reason, as for example their great relative velocity, may not have been able to pass through the air and to reach the ground in solid form;—

Or, thirdly, that stones moving in such retrograde orbits, and coming through the air, may be falling while men sleep, or for some like reason may fail to be found. In other words, the effective cause may work above the air, in the air, or below the air.

Let us assume, as an hypothesis, that neither of the first two are the true causes. In that case we should have the stones moving in every direction as they cross the earth's orbit. There should be about as many orbits having retrograde motions as direct motions. Hence the absolute quits of all stones coming into and hence, by hypothesis, coming through the air, should be symmetrically distributed in their longitudes relative to the sun. At least there should be as many absolute quits in the G-hemisphere as in the Q hemisphere (Fig. 1). Take account now of the earth's motion and locate the relative quits. All these stones whose absolute quits lie outside of the circle T T will have their relative quits in the G-hemisphere. Upon the hypothesis of parabolic orbits and of an equable distribution of the absolute quits over the celestial sphere the number of relative quits in the G-hemisphere should be to those in the Q-hemisphere as $1 + \cos \frac{\pi}{4} : 1 - \cos \frac{\pi}{4}$, or as 17:3. The relative quits should then be *very much* more numerous in the G-hemisphere than in the Q-hemisphere.

Furthermore, suppose that the heavens visible at a given time and place, are divided by a vertical circle into two halves; and suppose that this vertical circle is at right angles to the plane containing the zenith and the earth's quit and goal. That half of the visible heavens that lies towards the earth's goal may be called the goal-half, the other half may be called the quit-half of the visible heavens. In any given period there should evidently be, under the several hypotheses stated, many more stones coming into the air and reaching the ground directed from the goal-half than there should be directed from the quit-half of the visible heavens. Still further, since this proposition applies to any epoch whatever, we may apply it to 116 periods covering the times of the 116 stone-falls, that is, to the 116 stone-falls themselves Many more of these should (under the hypotheses stated) have come from the goal-half than from the quit-half of the visible heavens.

If, then, the relative quit of each of these 116 stones is supposed to be carried around in azimuth 180°, the altitude being unchanged, the 116 distances from each new place of the quit to the earth's quit for the epoch of the fall should, in the average, be decidedly less than the corresponding 116 distances from the actual relative quits to the earth's quit. This should hold true (under the hypotheses stated) no matter what causes below the air may have occasioned the selection of the 116 epochs. The fact that more persons are abroad in the evening hours from 6h. to 10h. p.m., than in the corresponding morning hours, 2h. to 6h. a.m., may well cause that more stones should be secured in the evening than in the morning hours. In the evening hours the earth's quit is above the horizon; in the morning hours the earth's goal. It might easily be that we should for this reason get more stones of direct than of retrograde motions. But the above criterion is entirely independent of any such principle of selection of the epochs. A change of the azimuth of the quits through 180° should cause a larger number of them (under the hypotheses stated) to approach the earth's quit than to recede from it.

I have marked off upon the working sheets the position 180° in azimuth from each of 115 relative quits, the altitude being unchanged, and measured the several distances from the earth's quit. (One fall, Nedagolla, was unavailable). The following is the result. In 44 cases the meteor's quit by the change approaches the earth's quit: in 70 cases it approaches the earth's goal; in one it remains unchanged. That is, instead of a very large majority of the quits moving towards the earth's quit we have nearly two-thirds of them moving the other way. In the reversed position, moreover, we should have had 38 absolute quits in the G-hemisphere instead of 7. These numbers show very decidedly that the hypotheses made above are not true. The principle of selection is not *entirely* below the air, and the numbers testify so markedly against that hypothesis that I feel warranted in adding that the cause is *mainly* either above the air, or in the air.

Between the first and second causes named the materials used for the present discussion do not furnish a *positive* critical test. But if, as I believe, the Stannern stone came from the south, we have at least one instance of stones coming into the air with a velocity of nearly, or quite, 45 miles a second and reaching the ground in solid form. About twenty-five of the quits in Fig 1 imply velocities of not less than 25 miles a second on entering the air. Large velocities do not seem to be entirely fatal to the integrity of the meteorites. I believe that the first cause was the dominant one rather than the second, yet for a crucial test of the two causes, if one can be found, we must look to a class of facts other than those we have been considering.

We are now in position to consider the other ninety-four stone-falls. In Fig. 2, the construction of which is similar to that of Fig. 1, the stars mark the zenith points for each time and place of the ninety-four falls. A grouping is at once noticeable. They are nearly all in the northern hemisphere, since the observing peoples live there. Those stars in the hemisphere of which S is the pole, that is between the two lines P P and P P, are evidently daylight stone falls, since S is above the horizon for each case. These constitute about seven-eighths of the whole number. The reason for this predominance is manifest. In the night men see the fireball or the train, whereas in the day the first intimation of the stone fall is usually the hearing of the detonation two or three minutes after the fireball has disappeared. Hence, daylight stone-falls are those whose directions are less likely to be observed, and these ninety-four falls are the ones of which the directions are unknown.

It will also be seen that there are nearly twice as many in the Q-hemisphere as in the G-hemisphere; that is, there are nearly twice as many that fell when the earth's quit was above the horizon as there were when the earth's goal was above the horizon. In general, the former were afternoon stone-falls, the latter forenoon stone-falls. Now the habits of the urban population have not much to do with these daylight meteors, for the fireballs were not seen. The accounts come from the country, where the stones in general have fallen, and about as m ny people are there abroad in the forenoon as in the afternoon. If stones came to the ground as often from retrograde as from direct orbits we ought apparently to have had very many more zeniths in the G-hemisphere than in the Q-hemisphere. The contrary being the fact of experience we may reasonably say that the ninety-four stone-falls, about which we know comparatively little, seem decidedly to follow the same laws as the 116 falls about which we know so much more.

This conclusion is greatly strengthened if we take account of the effect of the earth's attraction in carrying the meteor's quit toward the zenith. Any stone must be moving downward when it enters the air. But the earth's attraction must change the direction of its motion during the approach to the earth. Hence the region of the heavens from which a stone can approach the earth is not bounded by the actual horizon, but by a curve which may be treated as a depressed horizon. This depression of the horizon is far greater toward the quit than toward the the goal side of the horizon. The maximum depression for a stone moving in a parabolic orbit is about 17°. It hence follows that when the zenith is more than 73° and less than 90° from G, both the points G and Q are above the depressed horizon, and therefore that the 14 falls whose zeniths are between these limits, that is, are between the circles A A and P E P S, Fig. 2, should be left out of the count. The corresponding region on the Q-hemisphere is less than one degree in breadth, and contains one zenith point. We have left only 20 falls when the earth's goal alone was above the depressed horizon to be compared with 59 falls when the earth's quit alone was above the depressed horizon.

Of the 50 observed falls constituting the third group, of which the hour of fall is not stated, very few particulars other than the fact of fall are known. Although we are left without the power of saying that they indicate the same law as the other 210 falls, we find at the same time no reason to suspect the contrary. It is not unreasonable to assume that the well-observed stone-falls are good representatives of the whole group, and to affirm the three propositions with which I set out as true, in general, not only for the 210 stone-falls of the first two groups, but for the whole 260 stone-falls which are represented by stones in our cabinets, and in which the stones were seen or known to fall.

It also seems a natural and proper corollary to these propositions (unless it shall appear that stones meeting the earth are destroyed in the air), that the larger meteorites moving in our solar system are allied much more closely with the group of comets of short period than with the comets whose orbits are nearly parabolic. All the known comets of shorter periods than 33 years move about the sun in direct orbits that have moderate inclinations to the Ecliptic. On the contrary, of the nearly parabolic cometic orbits that are known only a small proportion of the whole number have small inclinations with direct motion.

It also follows that in future reductions of these stone-fall observations it will be better to assume that the velocity of the stone in its orbit was not that velocity which corresponds to a parabolic orbit, but that which corresponds to the mean orbit of the comets of short period. The largeness of the perihelion distances has an evident bearing also upon the idea that these stones form the fuel of the sun.

The presentation of the argument here made has been incomplete in that the details of the investigation of individual stone-falls have been entirely omitted. Some of the determinations of the paths are, I think, as complete as I can hope to make them. But others must be regarded as provisional, since I hope to secure respecting them additional data. I hope at some future time to give a complete discussion of all these observed stone-falls. In the past I have been greatly indebted to friends for aid in collecting accounts of the falls, and I heartily thank them therefore. I shall be very grateful also in the future for unpublished observations of the stone-falls, as well as for observations that have been so published as not to be likely to have attracted attention. I bespeak the kindly aid of any who have made or have collected such observations.

NOTES.

At the time of the Paris Exhibition in 1889, several scientific congresses will assemble in the French capital—congresses of zoology, anthropology, physiology, electricity, dermatology, hygiene. The *Revue Scientifique* expresses a hope that the great congress of electricity in 1881 may be taken as a model for all these assemblies; that attempts will be made, as far as possible, to establish uniformity in scientific nomenclature; and that men of science in other countries will not allow themselves to be deterred by international jealousies from being adequately represented at meetings whose proceedings will relate to matters of universal interest.

At the next meeting of the British Association there will be a discussion in Section D on the vexed question of the formation of coral reefs. The discussion will be opened by Dr. Sydney J. Hickson.

On Tuesday evening Mr. W. H. Smith, speaking of the measures with which it would be impossible to deal during the present Session, announced that the Government had decided to drop the Technical Instruction Bill. He deeply regretted that this was necessary, "but perhaps," he added, "there may not be much loss of time, as the Royal Commission on Elementary Education will report shortly on the whole question, and it will be interesting and convenient to the House to have that report before it before attempting to legislate on the subject."

A Conference of the Executive Committee of the National Association for the Promotion of Technical Education and representatives of branches and co-operating associations was held last Saturday afternoon at the Society of Arts. Afterwards the first annual meeting of the Association was held. Lord Hartington presided, and delivered an able and interesting speech, showing how the establishment of a proper system of technical instruction has been rendered absolutely necessary by the conditions of modern industrial development.

The anniversary meeting of the Sanitary Institute of Great Britain will be held to-day at 3 p.m. The chair will be taken by Mr. Edwin Chadwick, C.B., who will present the medals and certificates awarded to the exhibitors at the exhibition held at Bolton. Dr. B. W. Richardson, F.R.S., will deliver an address, entitled, "The Storage of Life as a Sanitary Study."

On Thursday, the 5th inst., Prof. Stokes distributed the prizes to the students at the Medical School, St. Thomas's Hospital. In addressing the students he said that he need not remind them that diligence was the great road to success, and urged that it was a duty to work for our fellow-creatures as well as ourselves. He thought that the two noblest professions were those, one of which assisted in the rectification of man's character and the other in alleviating the results of disease. In the exercise of the medical profession our best feelings were, he thought, called forth. The best foundation was a general liberal education, and although those branches of science which bear directly on medicine might be separated from their practical application, they were in themselves most interesting, and, when studied for their own sakes, were excellent mental training. He was glad to hear from Dr. Ord that St. Thomas's students were successful in athletics, as the cups exhibited testified. In the necessarily sedentary life of a medical student exercise and relaxation should not be neglected, and students did well to study the use of their muscles in athletic pursuits. Sir John Simon, on behalf of the Governors of the Hospital, thanked Prof. Stokes for distributing the prizes, and referred to the high position attained by Prof. Stokes, who, as President of the Royal Society, and representative in Parliament of the University of Sir Isaac Newton, might be said to have gained the best possible prize, but hinted that the happiness of life consisted in its endeavours rather than in its prizes. He concluded by alluding to the retirement of Dr. Ord, whose services as Dean of the Medical School during the past twelve years had been, he felt sure, much appreciated by the Governors of the Hospital, by the medical and surgical staff, and by the students.

The French Minister of Public Instruction has authorized the following scientific missions:—M. Georges Martin is entrusted with a mission to Sweden and Norway, to study the different educational questions; M. Henry Meyners d'Estrey is sent to explore the mountainous districts of Scandinavia, and to study certain questions connected with ethnography and anthropology; M. Gaston Angelvy, civil engineer, goes to explore the tract of country between Lake Nyassa and the coast of the Indian Ocean, and to visit more particularly the basin of the river Royaurva.

The Musée Guimet in Paris, which contains specimens of a great number of objects used in religious ceremonies, was nominally opened some days ago. It will not, however, be opened to the public for several months.

THE meeting which will shortly be held in Paris for the study of tuberculosis, under Prof. Chauveau's presidency, promises to be very interesting and successful.

THE International Congress of "Americanists" will hold its seventh session in Berlin from October 2 to 5 next. The organizing committee has just issued the programme. The first day will be devoted to questions relating to the discovery of the New World, to the history of America before the time of Columbus, and to American geology; the second to archæology; the third to anthropology and ethnography; the fourth to philology and palæography.

IT is proposed that an exhibition, to be called the "Three Americas Permanant Exhibition," shall be established at Washington in 1892 as a memorial of the discovery of America by Columbus. Both Houses of Congress have expressed approval of the scheme. While the subject was being considered by the House Committee on Commerce, Major J. W. Powell, director of the U.S. Geological Survey, pointed out, in an interesting address to the Committee, the benefits that archæologists would be likely to derive from such an exhibition, and the importance of securing without delay the necessary materials.

WE have received the volume containing a report of the Proceedings of the thirty-sixth meeting of the American Association for the Advancement of Science, held at New York in August, 1887. Among the more interesting contents of the volume is the address of Dr. Daniel G. Brinton, vice-President of the Section for anthropology. In this address Dr. Brinton presents a comprehensive review of the data for the study of the prehistoric chronology of America. Speaking of physical characteristics, he says that although the anatomy and physiology of the various American tribes present great diversity they also display a really remarkable fixedness of type. No observer well acquainted with this type could err, he thinks, in taking it for another. "Darwin says that the Fuegians so closely resemble the Botocudos [of Brazil] that they seemed members of the same tribe. I have seen Arawacks from Guiana who in the north-west would have passed for Sioux." According to Prof. J. Kollmann, the results of whose researches on this subject are accepted by Dr. Brinton, the essential physical identity of the American race is as extended in time as in space. Prof. Kollmann has analyzed the cranioscopic formulas of the most ancient American skulls, those from the alleged tertiary deposits of the Pampas, that obtained from Rock Bluff, Illinois, the celebrated Calaveras skull from California, and one from Pontemelo in Buenos Ayres of geologic antiquity. The conclusion at which he arrives is that the earliest Americans—those who were contemporaries of the fossil horse and other long since extinct quadrupeds—possessed the same racial character as the natives of the present day, with similar skulls and a like physiognomy.

ON Monday the atmosphere in the Channel became so rarefied that objects could be seen with extraordinary distinctness at a distance of between 30 and 40 miles from Dover and Folkestone. The *Times* says that the lighthouse at Cape Grisnez, Calais, and the dome of the Cathedral, and Napoleon's Column at Boulogne could be distinctly seen with the naked eye, and every prominent object could be picked out along the French coast. The distance from Dover to Boulogne as the crow flies is 28 miles, and the column is about 2 miles further inland.

THE following telegram from Valparaiso was lately received at Buenos Ayres:—"A rather severe earthquake shock was experienced in Santiago on Sunday, May 13, at 11.30 a.m., and considerable alarm prevailed in consequence of May 13 being the anniversary of the great earthquake in 1647, which laid a large portion of the city in ruins, and which was the origin of the procession of the Señor de Mayo. A severe but short vertical shock occurred here on Tuesday, the 15th, at 8.5 p.m. A strong earthquake shock was felt at Yumbel on the 10th, at 9.15 p.m. A smart earthquake shock, preceded by a long subterranean noise, was experienced in Santiago on Wednesday, the 16th, at 4.55 a.m. The shock was also felt here, but slightly." At Buenos Ayres several earthquake shocks were experienced on the night of Monday, June 4. According to the *Buenos Ayres Standard*, a slight shock was felt at 12.18. Three seconds afterwards a very strong shock occurred, and the oscillation was slow and pronounced. The walls of houses and all movable articles were shaken, and a third shock, which seemed to be nothing more than the subsidence of the second, occurred two seconds afterwards. No serious accident followed the occurrence. Several families, however, were so startled that they rushed out of their houses and sought refuge in the open square. The shocks were felt with more or less intensity all over the province of Buenos Ayres and in Montevideo. As felt in Montevideo the shock passed from south-south-west to north-north-east.

AT the meeting of the French Meteorological Society on June 5, M. Angot communicated a paper on the climate of St. Martin-de-Hinx (Landes) based on observations made since 1864, in which he has determined the diurnal variations of each element. He also announced that as soon as funds were obtained he intended to publish *in extenso* several long series of observations. At several of the places mentioned, including Paris, Marseilles, &c., the observations date from far into the last century. M. L. Teisserenc de Bort communicated a note relative to two earthquakes which occurred at 8 p.m. on the 4th, and at 5 p.m. on May 14 last, in the department of Puy-de-Dôme. M. Moureaux remarked that the magnetograms at Parc-St.-Maur showed no special disturbances at those times. M. Renou paid a tribute to the memory of M. Hervé-Mangon, to whose exertions the separation of the meteorological from the astronomical service was due. This memoir will be printed in the Bulletin of the Society.

A NEW base and its series of salts, belonging to the remarkable group known as "platinum bases," have been obtained by Dr. Heinrich Alexander, of Königsberg. The base itself has the composition $Pt(OH)_2 . 4NH_3O$, and may be considered as the hydroxylamine-platinum compound corresponding to the free base of the well-known green salt of Magnus, $Pt(OH)_2 . 4NH_3$. The chloride of the series was prepared some little time ago by Lossen, but can be most readily obtained, according to Alexander, by mixing a 10 per cent. solution of potassium platinous chloride with hydrochloride of hydroxylamine and an alkaline carbonate. On standing, the deep red liquid becomes decolourized, and the reaction is completed when a yellowish precipitate commences to settle; on the addition of more alkali the new base is immediately and quantitatively precipitated. The precipitate is then dissolved in the calculated quantity of cold dilute hydrochloric acid, and on passing a gentle stream of hydrochloric acid gas through the solution, or on the addition of absolute alcohol, fine colourless needles of the chloride $PtCl_2 . 4NH_3O$ are deposited. These needles are very soluble in water, but, like many other chlorides, are insoluble in concentrated hydrochloric acid. The free base is at once precipitated from this salt on the addition of stronger bases, such as potash and soda, or even ammonia. It is perfectly stable in the air and is extremely insoluble in water and alcohol; it behaves exactly like a true metallic hydroxide, dissolving in acids with formation of the corresponding salts. The sulphate $PtSO_4 . 4NH_3O$, which is best obtained by treating the base with the calculated quantity of sulphuric acid upon a water bath, crystallizes well in short, heavy prisms, difficultly soluble in cold but better in hot water, the crystals deposited from which contain a molecule of water of crystallization. When heated above 100° C. it violently decomposes with detonation. In a similar manner the phosphate and oxalate of the series were obtained pure and analyzed. The former separates out in microscopic crystals while the latter is deposited in beautiful stellar aggregates of long needles. During the course of the work, two interesting isomeric salts were obtained. When the base is treated with excess of warm hydrochloric acid and the solution allowed to cool, yellow needles of a chloride of the composition $PtCl_2 . 2NH_3O$ fall out. If however potassium platinous chloride be added to dilute solutions of the first chloride, $PtCl_2 . 4NH_3O$, beautiful violet needles of an isomeric salt, $PtCl_2 . 4NH_3O + PtCl_2$, separate out. The two substances are quite distinct, though possessing the same empirical formula, reminding one of the remarkable isomerism so frequently met with among the compounds of carbon.

UNDER the heading of "Psychology" the *American Naturalist* for May has a curious paragraph on "The Monkey as a Scientific Investigator." In the interesting little "Zoo" connected with the National Museum at Washington, there is a fine male grivet monkey (*Cercopithecus erythræa*), who shares a large cage with four opossums. To human beings he shows himself anything but amiable, but "he takes kindly to his strange companions, and they have been the best friends from the first." The attention of the attendant was lately drawn to the cage by the excitement of a crowd in front of it, and on going to ascertain the cause he was surprised to see the monkey seated in the middle of the cage, with one of the opossums lying quietly on her back on his lap, and her head under his arm. "The monkey had just discovered the marsupial pouch of the opossum, and was diligently investigating it. Had he not been a close observer it certainly would have remained unseen, for it was so tightly closed as to be perfectly invisible in its normal condition. The monkey carefully lifted the outer wall of the pouch, and peered into the cavity. Then he reached in with his hand, felt about for a moment, and to the astonishment of everybody took out a tiny young opossum, about 2 inches long, hairless, blind, and very helpless, but alive and kicking. Jock held it up to the light, where he could get a good view of it, scrutinized it with the air of a *savant*, and presently returned it to the pouch, very carefully. After replacing it he looked into the pouch again, and presently drew out another for examination, which he looked at with solemn interest, smelt it, and then carefully put it back. It was thus it became known to the attendants that the old female opossum had the young ones, which had previously been looked for in vain."

SOME time ago an English resident at Canton, Mr. Pitman, bought a curious monstrosity—a sow with six legs. The front part of the body is simple, that is, the animal has one head, one thorax, and two front legs. Behind, all the organs are double. M. Bézaure, the French Consul at Canton, persuaded Mr. Pitman to let him have this strange creature for the Paris Museum of Natural History, where it may now be seen. It is white, with great black spots, and appears to be in perfect health. An account of it, by M. Charles Brongniart, of the Museum of Natural History, appears in the current number of *La Nature*. The separation of the two trunks seems to begin after the dorsal vertebræ; but the animal is so fat that this cannot be precisely determined.

MANY women who are anxious to obtain a University training cannot afford to pay the fees required for residence at one of the colleges or halls in connection with the old Universities. For their benefit Aberdare Hall, Cardiff, was founded; and we are glad to learn that the institution has made steady progress since it was opened in 1885. This year the number of students has doubled. At University College, Cardiff, the students at Aberdare Hall are taught on the same footing as the men students. They generally work for London University degrees, but when they wish to prepare for other examinations the necessary help is gladly given.

THE Irish Exhibition in London has published a useful "Handy-book of Reference for Irishwomen." It is edited by Miss Helen Blackburn, and Mrs. Power Lalor contributes a preface. The volume presents full and accurate information as to women's work in Ireland, and as to the schools and classes in which they may obtain scientific and technical training.

THE annual report of the Geological and Natural History Survey of Canada for 1886 (vol. ii. new series) has been issued. It embodies the results of some of the work of preceding years, and not all of the work of the year for which it is dated. The volume consists of thirteen parts, separately paged and lettered, and relating to various portions of the Dominion from Nova Scotia to British Columbia, and northward to the Arctic Ocean. The parts were issued separately with accompanying maps and illustrations in pamphlet form, as they were received from the printers.

THE new number of the *Mineralogical Magazine* contains, besides abstracts and a full index to vol. vii., the following papers:—On the development of a lamellar structure in quartz-crystals by mechanical means, by Prof. John W. Judd, F.R.S.; on the polysynthetic structure of some porphyritic quartz-crystals in a quartz-felsite, by Major-General C. A. McMahon; on kaolinite, by Alan Dick; note on the occurrence of celestite containing nearly 14 per cent. of free sulphur, by H. J. Johnston-Lavis; notes on hornblende as a rock-forming mineral, by Alfred Harker.

M. VAYNIÈRE has brought out the second of the four parts of his Atlas of invertebrate animals.

IN a circular issued by Mr. Edward S. Holden, director of the Lick Observatory, it is stated that the Observatory buildings will be open to visitors during office hours every day in the year. An hour or so, he points out, can be profitably occupied in viewing the various instruments, and the rest of the stay can be well spent in walks to the various reservoirs, from which magnificent views of the surrounding country can be had. With regard to the admission of visitors at night, Mr. Holden says that, for the present, visitors will be received at the Observatory to look through the great telescope every Saturday night between the hours of 7 and 10, and at these times only. Whenever the work of the Observatory will allow, other telescopes will also be put at the disposition of visitors on Saturdays between the same hours. Mr. Holden hopes that, by setting apart these times for visitors (which allow freer access to the Lick Observatory than is allowed to any other Observatory in the world) all interested may be able to arrange their visits in conformity to them; and that the remaining hours of the week will be kept entirely uninterrupted, in order that the astronomers may do the work upon which the reputation of the Observatory entirely depends.

FROM a report signed by Mr. Edward S. Holden we learn that the trustees of the Lick Observatory, acting on his advice, have provided a photographic attachment to the 36-inch telescope, which will enable this to be used as a gigantic camera for photography. It cannot be used to make maps according to the scheme of the Paris Congress, since that scheme requires a focal length of 13 feet, while that at the Lick Observatory will be 47. But a vast deal of work may be done in connection with applications to astronomy other than the construction of the chart. In the photography of the moon, of the planets, of nebulæ, and comets the Lick telescope will have some important advantages. "But," says Mr. Holden, "it is in the photography of stars—of double and binary stars, of all the fainter stars, of all star clusters — that the Lick photographic telescope will find its chief application and demonstrate its immense superiority. One of the first works to be done is to photograph the vicinity of all the brighter stars, for the discovery of fainter companions, and for the permanent record of their surroundings. A certain number of stars will be selected and photographed at regular intervals throughout the year. Measures made upon these plates will give the data by which the distances of these stars from the earth can be determined. Similar measures upon photographs of star clusters may serve to give us a clue to the laws which govern the internal structure of these wonderful objects. A continuous series of photographs of the brighter parts of one of the brighter comets will certainly throw a flood of much needed light upon the process of their development."

THE additions to the Zoological Society's Gardens during the past week include a White-thighed Colobus (*Colobus vellerosus* ♂), a Campbell's Monkey (*Cercopithecus campbelli* ♀), a White-Collared Mangabey (*Cercocebus collarii*), a Bosman's Potto (*Perodicticus potto*), a Marabou Stork (*Leptoptilus crumeniferus*), a Black Sternothere (*Sternothærus niger*) from West Africa, presented by Mr. H. H. Johnston, F.Z.S. ; two Black-Bellied Sand Grouse (*Pterocles arenarius*) from North Africa, presented by Sir Kirby Green, K.C.M.G. ; an Eyed Lizard (*Licerta ocellata*), European, presented by Mr. J. Hopson ; a Patas Monkey (*Cercopithecus patas* ♀), two West African Love Birds (*Agapornis pullaria*) from West Africa, a Cormorant (*Phalacrocorax carbo*), British, three Scarlet Ibises (*Eudocimus ruber*) from South America, five Common Chameleons (*Chamæleon vulgaris*) from North Africa, deposited ; a Chipping Squirrel (*Tamias striatus*) from North America, five Lesser Pintailed Sand Grouse (*Ptero les exustus* 1 ♂, 3 ♀) from Abyssinia, two Modest Grass Finches (*Amadina modesta*) from Australia, purchased ; a Moor Monkey (*Semnopithecus maurus* ♂) from Java, received in exchange ; a Spotted Tinamon (*Nothura maculosa*), two Cambayan Turtle Doves (*Turtur senegalensis*), three Chiloe Widgeon (*Mareca chiloensis*), three Slender Ducks (*Anas gibberifrons*), two Australian Wild Ducks (*Anas superciliosa*), three Mandarin Ducks (*Æx galericulata*), eleven Chilian Pintails (*Dafila spinicauda*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE MARKINGS ON MARS.—M. Perrotin, in a more recent communication to the Paris Academy of Sciences, states that the district of *Libya*, the disappearance of which he had recorded a week or two earlier (NATURE, vol. xxxviii. p. 185), has undergone a further change, the "sea" which had so recently covered it having retreated again for the most part, so that the present appearance of the district is intermediate between that which it recently presented and that under which it was seen in 1886. Of the canals M. Perrotin has noticed four, three of which are double, which, starting from the "seas" of the southern hemisphere near the equator, and following a nearly meridional course, extend right up to the north polar ice cap, being traceable across the "seas" which immediately surround the latter. No other observer as yet seems to have traced these canals for such a distance, and across "seas" as well as continents. This observation renders their true character more puzzling than ever, and seems effectually to dispose both of M. Fizeau's just published theory, which explains them by the analogy of the rifts in terrestrial glaciers, Mars being assumed to be in a glacial condition, and of that of Mr. Proctor, who ascribes them to the varying appearances of the Martial rivers when clearly seen or partly veiled by local mists. More detailed observations of these strange markings are needed, and it is to be much desired that as many as possible of actual drawings made at the telescope should be published. It is possible that the comparison of sketches made with different observers and with different apertures, would throw much light on the subject ; if, for instance, the appearances were partly optical and due to some effect of diffraction, it would soon become apparent.

COMET 1888*a*, SAWERTHAL.—The remarkable change in brightness which this object displayed about May 20 (NATURE, vol. xxxviii. p. 114) seems to have been well observed, and there is a general agreement that the increase in brightness amounted to 2½ or 3 magnitudes. At Dorpat Herr Blumbach estimated the comet as 9–10 on May 19, and as 7–8 on May 22. Dr. Franz, at Konigsberg, considered the increase as amounting to 3½ magnitudes, estimating the brightness as 5·8 on May 21, whilst Dr. Kammermann, at Geneva, on May 25, reckoned the comet as between the 5th and 6th mags., and the increase as having been between 2 and 3. Father Fenyi, of the Kalocsa Observatory, finds the change of magnitude about the same, but estimates the absolute brightness differently ; the recorded magnitudes being: May 20, 9·3 ; May 21, 7·8 ; May 22, 6·8 ; and May 23, 6·8. Father Fenyi also supplies (*Astr. Nach.*, No. 2844) a series of sketches of the comet, showing the changes of shape which have accompanied the changes of brightness, and especially the development about May 28 of a sort of wing on either side of the head. These wings appear, however, to have been seen earlier at other observatories, thus Herr Kortazzi, at Nicolaiew, observed them on May 24, and Herr Wutschichowski gives a beautiful drawing of them under date May 25 (*Astr. Nach.*, No. 2845). The comet does not appear to have been satisfactorily observed with the spectroscope during this period of unusual brilliancy. The outburst was soon over, and the comet speedily returned to its former faintness.

The following ephemeris (*Astr. Nach.*, No. 2838) is in continuation of that given in NATURE, vol. xxxviii. p. 186.

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
July 13	1 7 18	50 32·8 N.	0·3352	0·3306	0·029
15	1 7 42	50 55·4			
17	1 7 56	51 17·2	0·3459	0·3331	0·028
19	1 8 2	51 38·4			
21	1 7 57	51 58·8	0·3563	0·3353	0·026
23	1 7 43	52 18·5			
25	1 7 19	52 37·4	0·3664	0·3372	0·025
27	1 6 45	52 55·4			
29	1 6 0	53 12·6	0·3762	0·3389	0·023
31	1 5 6	53 28·9			
Aug. 2	1 4 1	53 44·2 N.	0·3857	0·3405	0·022

The brightness on February 18 is taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JULY* 15–21.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on July 15

Sun rises, 4h. 3m. ; souths, 12h. 5m. 44·2s. ; sets, 20h. 8m. : right asc. on meridian, 7h. 40·8m. ; decl. 21° 26′ N. Sidereal Time at Sunset, 15h. 44m.

Moon (at First Quarter July 16, 12h.) rises, 11h. 7m. ; souths, 17h. 21m. ; sets, 23h. 22m. : right asc. on meridian, 12h. 56·6m. ; decl. 0° 35′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury	3 44	11 24	19 4	6 58·6	18 0 N.
Venus	4 4	12 11	20 18	7 45·9	22 9 N.
Mars	12 56	18 2	23 8	13 38·3	11 15 S.
Jupiter	15 38	20 2	0 26*	15 38·0	18 36 S.
Saturn	5 19	13 6	20 53	8 41·8	18 56 N.
Uranus	11 34	17 14	22 54	12 50·3	4 42 S.
Neptune	0 39	8 25	16 11	3 59·5	18 53 N.

* Indicates that the setting is that of the following morning.

Comet Sawerthal.

July.	h.	Right Ascension. h. m.	Declination. ° ′
15	0	1 7·5	50 45 N.
19	0	1 8·0	51 28

Occultations of Stars by the Moon (visible at Greenwich).

July.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
17	ξ′ Libræ	6	19 22	19 40	145 176
18	θ Libræ	4½	21 2	21 23	8 339
19	49 Libræ	5½	0 20	near approach	202 —
19	B.A.C. 5700	6½	22 26	23 14	142 232

July.	h.	
16 ...	13 ...	Mars in conjunction with and 6° 40′ south of the Moon.
18 ...	17 ...	Jupiter in conjunction with and 4° 5′ south of the Moon.
20 ...	0 ...	Mercury stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m	
U Cephei	0 52·4 ...	81 16 N. ...	July 15,	21 31	*m*
			,, 20,	21 11	*m*
W Virginis	13 20·3 ...	2 48 S. ...	,, 16,	22 0	*m*
δ Libræ	14 55·0 ...	8 4 S. ...	,, 20,	0 44	*m*
U Coronæ	15 13·6 ...	32 3 N. ...	,, 15,	21 43	*m*
W Herculis	16 31·3 ...	37 34 N. ...	,, 20,		*m*
U Ophiuchi	17 10·9 ...	1 20 N. ...	,, 19,	2 4	*m*
			,, 19,	22 12	*m*
W Sagittarii ...	17 57·9 ...	29 35 S. ...	,, 16,	21 0	*m*
Z Sagittarii	18 14·8 ...	18 55 S. ...	,, 16,	0 0	*m*
T Serpentis	18 23·4 ...	6 14 N. ...	,, 20,		*M*
β Lyræ	18 46·0 ...	33 14 N. ...	,, 15,	1 0	*M*
R Lyræ	18 51·9 ...	43 48 N. ...	,, 18,		*m*
R Cygni	19 33·8 ...	49 57 N. ...	,, 19,		*M*
S Aquilæ	20 6·5 ...	15 17 N. ...	,, 21,		*m*
S Delphini	20 37·9 ...	16 41 N. ...	,, 18,		*m*
X Cygni	20 39·0 ...	35 11 N. ...	,, 21,	0 0	*m*

M signifies maximum ; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
The *Perseids*	25° ...	50° N. ...	Swift, streaks.
Near γ Draconis	269 ...	51 N. ...	Swift.
,, α Lacertæ	336 ...	49 N. ...	Swift, short.

GEOGRAPHICAL NOTES.

THE Geographical Society of Paris have decided to avail themselves of the Universal Exhibition at Paris, next year, by convening an International Congress of the Geographical Sciences, to meet in the month of August. There will be two classes of members, subscribing respectively 40 and 20 francs, and each member will be entitled to receive a copy of the publications of the Congress and have a vote in the questions discussed at the meetings. Each Society represented at the Congress will be invited to submit a report on the voyages, explorations, and publications which have most contributed, in the country to which it belongs, to the progress of geography during the past hundred years ; the combined reports will afterwards be published with the names of their authors.

DR. H. MEYER has made some important corrections in the preliminary account of his ascent of Kilimanjaro. After verifying and correcting his barometrical observations, he admits that the previously accepted height of 18,700 feet is more accurate than that given by himself, 19,850 feet. He then refers to the dense mist which prevented him from seeing beyond a wall of inaccessible ice, 130 feet high, which his first account indicated as being the terminal point of the peak. It results from these observations that Dr. Meyer did not reach to within 820 feet of the summit of Kilimanjaro, which therefore still remains unconquered.

M. JULES BORELLI, the French traveller, who accompanied M. Rimbaud last year in his interesting journey from Antotto to Harar, is engaged in exploring the country to the south-west of Shoa. The Paris Geographical Society has received some of the results accruing from his journey from Antotto to Jiren, which is situated in 7°42′ N. latitude, and 34°35′ E. longitude. Among these results is the discovery of the sources of the River Hawash, which lie at the foot of Mount Ilfata at the extremity of the Meca range, and not near Mount Dandi, as hitherto supposed. On the summit of the latter peak the traveller found a double lake resembling in shape the figure 8, which is of considerable extent and depth ; an affluent of the Gudar, and thus of the Abbay, issues from this lake. He also discovered a deep lake at the bottom of the immense crater mountain known as Mount Harro ; the surroundings of this sheet of water are described by the traveller as of incomparable beauty. From this lake, which is named by the natives Wancit, a stream issues and joins the Walga, the source of the latter river being in the summit of Mount Harro. Dr. Traversi, the Italian explorer, made in June, 1887, an excursion into the mountainous region of Urbanagh, lying to the east of the district now being explored by M. Borelli. The chief result of this journey of Dr. Traversi is to throw light on the problem of the hydrographical systems of the Somali and Galla countries. From the summit of Mount Gafat he was able to confirm his previous observations made near the Suai Lake, with reference to the three lakes above-mentioned and their interconnection.

ON CERTAIN INEQUALITIES RELATING TO PRIME NUMBERS.

I SHALL begin with a method of proving that the number of prime numbers is infinite which is not new, but which it is worth while to recall as an introduction to a similar method, by series, which will subsequently be employed in order to prove that the number of primes of the form $4n + 3$, as also of the form $6n + 5$, is infinite.

It is obvious that the reciprocal of the product

$$\left(1 - \frac{1}{p_1}\right)\left(1 - \frac{1}{p_2}\right)\left(1 - \frac{1}{p_3}\right) \cdot \cdot \cdot \cdot \left(1 - \frac{1}{p_{N.p}}\right)$$

(where p_i means the ith in the natural succession of primes, and $p_{N.p}$ means the highest prime number not exceeding N)[1] will be equal to

$$\frac{1}{1} + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \frac{1}{5} + \frac{1}{6} + \cdot \cdot \cdot \cdot + \frac{1}{N} + R,$$

and therefore greater than log N (R consisting exclusively of positive terms).

Hence

$$\left(1 + \frac{1}{p_1}\right)\left(1 + \frac{1}{p_2}\right) \cdot \cdot \cdot \cdot \left(1 + \frac{1}{p_{N.p}}\right) > M \log N,$$

where

$$M = \left(1 - \frac{1}{p_1^2}\right)\left(1 - \frac{1}{p_2^2}\right) \cdot \cdot \cdot \cdot \left(1 - \frac{1}{p_{N.p}^2}\right),$$

and is therefore greater than $\frac{2}{\pi}$.

Hence the number of terms in the product must increase indefinitely with N.

By taking the logarithms of both sides we obtain the inequality

$$S_1 - \tfrac{1}{2}S_2 + \tfrac{1}{3}S_3 - \tfrac{1}{4}S_4 + \cdot \cdot \cdot \cdot > \log \log N,$$

where in general S_i means the sum of inverse ith powers of all the primes not exceeding N ; and accordingly is finite, except when $i = 1$, for any value of N. We have therefore

$$S_1 > \log \log N + \text{Const.}$$

The actual value of S_1 is observed to differ only by a limited quantity from the second logarithm of N, but I am not aware whether this has ever been strictly proved.

Legendre has found that for large values of N

$$(1 - \tfrac{1}{3})(1 - \tfrac{1}{5}) \cdot \cdot \cdot \cdot \left(1 - \frac{1}{p_{N.p}}\right) = \frac{1{\cdot}104}{\log N}.$$

Consequently

$$\left(1 - \frac{1}{p_1}\right)\left(1 - \frac{1}{p_2}\right) \cdot \cdot \cdot \cdot \left(1 - \frac{1}{p_{N.p}}\right) = \frac{{\cdot}552}{\log N}.$$

This would show that the value of our R bears a finite ratio to log N ; calling it θ log N we obtain, according to Legendre's formula,

$$\frac{1}{1 + \theta} = {\cdot}552, \text{ which gives } \theta = {\cdot}811,$$

so that the nebulous matter, so to say, in the expansion of the reciprocal of the product of the differences between unity and the reciprocals of all the primes not exceeding a given number, stands in the relation of about 4 to 5 to the condensed portion consisting of the reciprocals of the natural numbers.

I will now proceed to establish similar inequalities relating to prime numbers of the respective forms $4n + 3$ and $6n + 5$.

Beginning with the case $4n + 3$, I shall use q_j to signify the jth in the natural succession of primes of the form $4n + 3$, and $q_{N.q}$ to signify the highest q not exceeding N, N.q itself signifying the number of q's not exceeding N.

[1] N.p itself of course denotes in the above notation the number of primes (p) not exceeding N.

Let us first, without any reference to convergence, consider the product obtained by the usual mode of multiplication of the infinite series

$$S = 1 - \tfrac{1}{3} + \tfrac{1}{5} - \tfrac{1}{7} + \tfrac{1}{9} - \ldots \textit{ ad inf.}$$

by the product

$$\frac{1}{1-\frac{1}{2}} \cdot \frac{1+\frac{1}{q_1}}{1-\frac{1}{q_1}} \cdot \frac{1+\frac{1}{q_2}}{1-\frac{1}{q_2}} \cdot \frac{1+\frac{1}{q_3}}{1-\frac{1}{q_3}} \ldots \textit{ ad inf.}$$

It is clear that the effect of the multiplication of S by the numerator of the above product will be to deprive the series S of all its negative terms. Then the effect of dividing by the denominator of the product, with the exception of the factor $1 - \frac{1}{2}$, will be to restore all the obliterated terms, but with the sign + instead of −. Lastly, the effect of multiplying by the reciprocal of $(1 - \frac{1}{2})$ will be to supply the even numbers that were wanting in the denominators of the terms of S, and we shall thus get the indefinite series

$$1 + \tfrac{1}{2} + \tfrac{1}{3} + \tfrac{1}{4} + \ldots \textit{ ad inf.}$$

Call now

$$Q_N = \frac{1}{1-\frac{1}{2}} \cdot \frac{1+\frac{1}{q_1}}{1-\frac{1}{q_1}} \cdot \frac{1+\frac{1}{q_2}}{1-\frac{1}{q_2}} \ldots \frac{1+\frac{1}{q_{N.q}}}{1-\frac{1}{q_{N.q}}}.$$

Q_N, which is finite when N is finite, may be expanded into an infinite aggregate of positive terms, found by multiplying together the series

$$1 + \tfrac{1}{2} + \tfrac{1}{4} + \tfrac{1}{8} + \ldots$$
$$1 + \frac{2}{q_1} + \frac{2}{q_1^2} + \frac{2}{q_1^3} + \ldots$$
$$1 + \frac{2}{q_2} + \frac{2}{q_2^2} + \frac{2}{q_2^3} + \ldots$$
$$\ldots\ldots\ldots\ldots\ldots\ldots$$
$$\ldots\ldots\ldots\ldots\ldots\ldots$$
$$1 + \frac{2}{q_{N.q}} + \frac{2}{q_{N.q}^2} + \frac{2}{q_{N.q}^3} + \ldots$$

Let

$$S_N = 1 - \tfrac{1}{3} + \tfrac{1}{5} - \tfrac{1}{7} + \tfrac{1}{9} - \tfrac{1}{11} + \ldots \pm \frac{1}{N},$$

then from what has been said it is obvious that we may write

$$Q_N S_N = 1 + \tfrac{1}{2} + \tfrac{1}{3} + \tfrac{1}{4} + \ldots + \frac{1}{N} + V - R,$$

where V and R may be constructed according to the following rule: Let the denominator of any term in the aggregate Q_N be called t, and let θ be the smallest odd number which, multiplied by t, makes $t\theta$ greater than N; then if θ is of the form $4n + 1$ it will contribute to V a portion represented by the product of the term by some portion of the series S_N of the form

$$\frac{1}{\theta} - \frac{1}{\theta+2} + \frac{1}{\theta+4} - \ldots$$

and if θ is of the form $4n + 3$ it will contribute to − R a portion equal to the term multiplied by a series of the form

$$-\frac{1}{\theta} + \frac{1}{\theta+2} - \frac{1}{\theta+4} + \ldots$$

Hence R is made up of the sum of products of portions of the aggregate Q_N multiplied respectively by the series

$$\tfrac{1}{3} - \tfrac{1}{5} + \tfrac{1}{7} - \tfrac{1}{9} + \tfrac{1}{11} - \tfrac{1}{13} + \ldots$$
$$\tfrac{1}{7} - \tfrac{1}{9} + \tfrac{1}{11} - \tfrac{1}{13} + \ldots$$
$$\tfrac{1}{11} - \tfrac{1}{13} + \ldots$$

of which the greatest is obviously the first, whose value is $1 - S_N$.

Consequently R must be less than the total aggregate Q_N multiplied by $1 - S_N$.

Therefore

$$Q_N S_N + Q_N(1 - S_N) > 1 + \tfrac{1}{2} + \tfrac{1}{3} + \tfrac{1}{4} + \ldots + \frac{1}{N} > \log N,$$

i.e.

$$Q_N > \log N,$$

from which it follows that when N increases indefinitely the number of factors in Q_N also increases indefinitely, and there must therefore be an infinite number of primes of the form $4n + 3$.

Denoting by M_N the quantity

$$\left(1 - \frac{1}{q_1^2}\right)\left(1 - \frac{1}{q_2^2}\right) \ldots \left(1 - \frac{1}{q_{N.q}^2}\right)$$

we obtain the inequality

$$\left(1 + \frac{1}{q_1}\right)^2\left(1 + \frac{1}{q_2}\right)^2 \ldots \left(1 + \frac{1}{q_{N.q}}\right)^2 > \tfrac{1}{2}M_N \log N,$$

and taking the logarithms of both sides

$$\Sigma_1 - \tfrac{1}{2}\Sigma_2 + \tfrac{1}{3}\Sigma_3 - \ldots > \tfrac{1}{2}\log\log N + \tfrac{1}{2}\log M_N - \tfrac{1}{2}\log 2,$$

where in general Σ_i denotes the sum of the ith powers of the reciprocals of all prime numbers of the form $4n + 3$ not surpassing N.

Hence it follows that $\Sigma_1 > \frac{1}{2}\log\log N$.

If we could determine the ultimate ratio of the sum of those terms of Q_N whose denominators are greater than N to the total aggregate, and should find that μ, the limiting value of this ratio, is not unity, then the method employed to find an inferior limit would enable us also to find a superior limit to Q_N; for we should have $V < \mu Q_N$ added to the sum of portions of what remains of the aggregate when μQ_N is taken from it multiplied respectively by the several series

$$\tfrac{1}{5} - \tfrac{1}{7} + \tfrac{1}{9} - \tfrac{1}{11} + \tfrac{1}{13} - \tfrac{1}{15} + \ldots \textit{ ad inf.}$$
$$\tfrac{1}{9} - \tfrac{1}{11} + \tfrac{1}{13} - \tfrac{1}{15} + \ldots \textit{ ad inf.}$$
$$\tfrac{1}{13} - \tfrac{1}{15} + \ldots \textit{ ad inf.}$$

the total value of the sum of which products would evidently be less than

$$(1 - \mu)(S - 1 + \tfrac{1}{3})Q_N.$$

Hence the total value of V would be less than

$$\mu Q_N S + (1 - \mu)Q_N(S - \tfrac{2}{3}),$$

i.e. less than

$$Q_N S - \tfrac{2}{3}(1 - \mu)Q_N,$$

and consequently we should have

$$\tfrac{2}{3}(1 - \mu)Q_N < \log N,$$

i.e.

$$Q_N < \frac{3}{2(1 - \mu)} \log N.$$

From which we may draw the important conclusion that if μ is less than 1, *i.e.* if when N is infinite the portion of the aggregate $S_N Q_N$ comprising the terms whose denominators exceed N does not become infinitely greater than the remaining portion, the sum of the reciprocals of all the prime numbers of the form $4n + 3$ not exceeding N would differ by a limited quantity from half the second logarithm of N.

A precisely similar treatment may be applied to prime numbers of the form $6n + 5$. We begin with making

$$S_N = 1 - \tfrac{1}{5} + \tfrac{1}{7} - \tfrac{1}{11} + \tfrac{1}{13} - \tfrac{1}{17} + \tfrac{1}{19} - \ldots$$

We write

$$Q_N = \frac{1}{1-\frac{1}{2}} \cdot \frac{1}{1-\frac{1}{3}} \cdot \frac{1+\frac{1}{r_1}}{1-\frac{1}{r_1}} \cdot \frac{1+\frac{1}{r_2}}{1-\frac{1}{r_2}} \ldots \frac{1+\frac{1}{r_{N.r}}}{1-\frac{1}{r_{N.r}}}$$

We make

$$Q_N S_N = 1 + \tfrac{1}{2} + \tfrac{1}{3} + \tfrac{1}{4} + \ldots + \frac{1}{N} + V - R.$$

We prove as before that

$$R < (1 - S)Q_N,$$

and thus obtain

$$Q_N > \log N,$$

and then putting

$$M_N = \left(1 - \frac{1}{r_1^2}\right)\left(1 - \frac{1}{r_2^2}\right) \ldots \left(1 - \frac{1}{r_{N.r}^2}\right),$$

and finally noticing that

$$\frac{1}{1-\frac{1}{2}} \cdot \frac{1}{1-\frac{1}{3}} = 3,$$

we obtain

$$\left(1 + \frac{1}{r_1}\right)^2\left(1 + \frac{1}{r_2}\right)^2 \ldots \left(1 + \frac{1}{r_{N.r}}\right)^2 > \tfrac{1}{3}M_N \log N.$$

Taking the logarithms of both sides of the equation, we find

$$\Theta_1 - \tfrac{1}{2}\Theta_2 + \tfrac{1}{3}\Theta_3 - \ldots > \tfrac{1}{2}\log\log N + \tfrac{1}{2}\log M_N - \tfrac{1}{2}\log 3,$$

where Θ_i means the sum of ith powers of the reciprocals of all the prime numbers, not exceeding N, of the form $6n + 5$.

Either from this equation or from the one from which it is derived it at once follows that the number of primes of the form $6n + 5$ is greater than any assignable limit.

Parallel to what has been shown in the preceding case, if it could be ascertained that the sum of the terms of the aggregate Q_N whose denominations do not exceed N bears a ratio which becomes indefinitely small to the total aggregate, it would follow by strict demonstration that the sum of the reciprocals of the primes of the form $6n + 5$ inferior to N would always differ by a limited quantity from the half of the second logarithm of N.

It is perhaps worthy of remark that the infinitude of primes of the forms $4n + 3$ and $6n + 5$ may be regarded as a simple rider to Euclid's proof (Book IX., Prop. 20) of the infinitude of the number of primes in general.

The point of this is somewhat blunted in the way in which it is presented in our ordinary text-books on arithmetic and algebra. What Euclid gives is something more than this:[1] his statement is, "There are more prime numbers than any proposed multitude (πλῆθος) of prime numbers"; which he establishes by giving a formula for finding at least one more than any proposed number. He does not say, as our text-book writers do, "if possible let A, B, ... C be *all* the prime numbers," &c., but simply that if A, B, ... C are *any* proposed prime numbers, one or more additional ones may be found by adding unity to their product which will either itself be a prime number, or contain at least one additional prime; which is all that can correctly be said, inasmuch as the augmented product may be the power of a prime.

Thus from one prime number arbitrarily chosen, a progression may be instituted in which one new prime number at least is gained at each step, and so an indefinite number may be found by Euclid's formula: *e.g.* 17 gives birth to 2 and 3; 2, 3, 17 to 103; 2, 3, 17, 103 to 7, 19, 79; and so on.

We may vary Euclid's mode of generation and avoid the transcendental process of decomposing a number into its prime factors by using the more general formula, $a, b, \ldots c + 1$, where $a, b, \ldots c$, are any numbers relatively prime to each other; for this formula will obviously be a prime number or contain one or more distinct factors relatively prime to $a, b, \ldots c$.

The effect of this process will be to generate a continued series of numbers all of which remain prime to each other: if we form the progression

$$a,\ a+1,\ a^2+a+1,\ a(a+1)(a^2+a+1)+1,\ \ldots$$

and call these successive numbers

$$u_1,\ u_2,\ u_3,\ u_4,\ \ldots$$

we shall obviously have

$$u_{x+1} = u_x^2 - u_x + 1.$$

It follows at once from Euclid's point of view that no primes contained in any term up to u_x can appear in u_{x+1}, so that all the terms must be relatively prime to each other. The same consequence follows *a posteriori* from the scale of relation above given; for, as I had occasion to observe in the *Comptes rendus* for April 1888, if dealing only with rational integer polynomials,

$$\phi(x) = (x - a)f(x) + a,$$

then, whatever value, c, we give to x, no two forms $\phi^i(c)$, $\phi^j(c)$ can have any common measure not contained in a: in this case $\phi(x) = (x-1)x + 1$; so that $\phi^i(c)$ and $\phi^j(c)$ must be relative primes for all values of i and j.[2]

It is worthy of remark that all the primes, other than 3, implicitly obtained by this process will be of the form $6i + 1$.

Euclid's own process, or the modified and less transcendental one, may be applied in like manner to obtain a continual succession of primes of the form $4n + 3$ and $6n + 5$.

[1] Whereas the English elementary book writers content themselves with showing that to suppose the number of primes finite involves an absurdity, Euclid shows how from any given prime or primes to generate an infinite succession of primes.

[2] Another theorem of a similar kind is that, whatever integer polynomial $\phi(x)$ may be, if i, j have for their greatest common measure k, then $\phi^k[\phi(0)]$ will be the greatest common measure of $\phi^i[\phi(0)]$, $\phi^j[\phi(0)]$.

As regards the former, we may use the formula

$$2 \cdot a \cdot b \ldots c + 1$$

(where $a, b, \ldots c$ are any "proposed" primes of the form $4n + 3$), which will necessarily be of the form $4n + 3$, and must therefore contain *one* factor at least of that form.

As regards the latter, we may employ the formula

$$3 \cdot a \cdot b \ldots c + 2$$

(where $a, b, \ldots c$ are each of the form $6n + 5$), which will necessarily itself be, and therefore contain *one* factor at least, of that form.

The scale of relation in the first of these cases will be, as before,

$$u_{x+1} = u_x^2 - u_x + 1;$$

so that each term in the progression, abstracting 3, will be of the form $4i + 3$ and $6j + 1$ conjointly, and consequently of the form $12n + 7$; as *e.g.*,

$$3,\ 7,\ 43,\ 1807,\ \ldots$$

In the latter case the scale of relation is

$$u_{x+1} = u_x^2 - 2u_x + 2,$$

which is of the form $(u_x - 2)u_x + 2$. It is obvious that in each progression at each step one new prime will be generated, and thus the number of ascertained primes of the given form go on indefinitely increasing, as also might be deduced *a posteriori* by aid of the general formula above referred to from the scale of relation applicable to each. Each term in the second case (the term 3, if it appears, excepted) will be simultaneously of the form $6i - 1$ and $4j + 1$, and consequently of the form $12n + 5$, as in the example 5, 17, 257, 65537, ...

The same simple considerations *cease* to apply to the genesis of primes of the forms $4n + 1$, $6n + 1$. We may indeed apply to them the formulæ

$$(2 \cdot a \cdot b \ldots c)^2 + 1 \text{ and } 3(a \cdot b \ldots c)^2 + 1$$

respectively, but then we have to draw upon the theory of quadratic forms in order to learn that their divisors are of the form $4n + 1$ and $6n + 1$ respectively.

Of course the difference in their favour is that in their case *all* the divisors locked up in the successive terms of the two progressions respectively are of the prescribed form; whereas in the other two progressions, whose theory admits of so much simpler treatment, we can only be assured of the presence of *one* such factor in each of the several terms.

Euler has given the values of two infinite products, without any evidence of their truth except such as according to the lax method of dealing with series without regard to the laws of convergence prevalent in his day, and still held in honour in Cambridge down to the times of Peacock, De Morgan, and Herschel inclusive (and this long after Abel had justly denounced the use of divergent series as a crime against reason), was erroneously supposed to amount to a proof, from which the same consequences may be derived as shown in the foregoing pages, and something more besides.[1] These two theorems are—

$$(1)\quad \frac{3}{3+1} \cdot \frac{5}{5-1} \cdot \frac{7}{7+1} \cdot \frac{11}{11+1} \cdot \frac{13}{13-1} \cdot \ldots = \frac{\pi}{4}$$

(where, corresponding to the primes 3, 7, 11, ... of the form $4n + 3$, the factors of the product on the left are

$$\frac{3}{3+1},\ \frac{7}{7+1},\ \frac{11}{11+1},\ \ldots$$

all of them with the sign + in the denominator; while the fractions corresponding to primes of the form $4n + 1$ have the − sign in their denominators).

$$(2)\quad \frac{5}{5+1} \cdot \frac{7}{7-1} \cdot \frac{11}{11+1} \cdot \frac{13}{13-1} \cdot \frac{17}{17+1} \cdot \ldots = \frac{\pi}{2}\sqrt{3}$$

where, as in the previous product, the sign in the denominator of each fraction depends on the form of the prime to which it corresponds (being + for primes of the form $6n - 1$, and − for primes of the form $6n + 1$).

[1] It follows from the first of these theorems that with the understanding that no denominator is to exceed n (an indefinitely great number), $(1 + \frac{1}{3})(1 + \frac{1}{7})(1 + \frac{1}{11})(1 + \frac{1}{19}) \ldots$ bears a finite ratio to $(1 + \frac{1}{5})(1 + \frac{1}{13})(1 + \frac{1}{17}) \ldots$ so that as their *product* is known to be infinite, each of these two partial products must be separately infinite; in like manner from Euler's second theorem a similar conclusion may be inferred in regard to each of the two products $(1 + \frac{1}{5})(1 + \frac{1}{11})(1 + \frac{1}{17})(1 + \frac{1}{23})(1 + \frac{1}{29})(1 + \frac{1}{41}) \ldots$ and $(1 + \frac{1}{7})(1 + \frac{1}{13})(1 + \frac{1}{19})(1 + \frac{1}{31}) \ldots$

Dr. J. P. Gram (*Mémoires de l'Académie Royale de Copenhague*, 6me. série, vol. ii. p. 191) refers to a paper by Mertens ("Ein Beitrag zur analytischen Zahlentheorie," *Borchardt's Journal*, Bd. 78), as one in which the truth of the first of the two theorems is demonstrated—"fuldstoendigt Bevis af Mertens" are Gram's words.[1]

Assuming this to be the case, we shall easily find when N is indefinitely great, so that S_N becomes $\frac{\pi}{4}$,

$$Q_N S_N = \frac{1}{(1-\frac{1}{2})(1-\frac{1}{3})\ldots\ldots\left(1-\frac{1}{N}\right)},$$

which, according to Legendre's empirical law (Legendre, "Théorie des Nombres," 3rd edition, vol. ii. p. 67, art. 397), is equal to $\frac{2\log N}{K}$, where $K = 1{\cdot}104$; and as we have written $Q_N S_N = \log N + (V - R)$, we may deduce, upon the above assumptions,

$$V - R = \left(\frac{2}{K} - 1\right)\log N = 0{\cdot}811\ldots\ldots\log N.$$

R, we know, is demonstrably less than $\left(1 - \frac{\pi}{4}\right)\log N$, consequently V must be less than $(0{\cdot}812 + 0{\cdot}215)\log N$, *i.e.* less than $1{\cdot}027 \log N$, and *a fortiori* the portion of the omnipositive aggregate Q_N, which consists of terms whose denominators exceed N, when N is indefinitely great, cannot be less than $\frac{4}{\pi}\left(1 - \frac{\pi}{4}\right)\log N$, *i.e.* $0{\cdot}273 \log N$.

Before concluding, let me add a word on Legendre's empirical formula for the value of

$$(1-\tfrac{1}{3})(1-\tfrac{1}{5})\ldots\ldots\left(1-\frac{1}{p_{N.p}}\right),$$

referred to in the early part of this article.

If N is any odd number, the condition of its being a prime number is that when divided by any odd prime less than its own square root, it shall not leave a remainder zero. Now if N (an unknown odd number) is divided by p, its remainder is equally likely to be 0, 1, 2, 3, ... or $(p - 1)$. Hence the chance that it is not divisible by p is $\left(1 - \frac{1}{p}\right)$, and, if we were at liberty to regard the like thing happening or not for any two values of p within the stated limit as independent events, the expectation of N being a prime number would be represented by

$$(1-\tfrac{1}{3})(1-\tfrac{1}{5})(1-\tfrac{1}{7})(1-\tfrac{1}{11})\ldots\ldots\left(1-\frac{1}{p_{N^{\frac{1}{2}}.p}}\right),$$

which, according to the formula referred to, for infinitely large values of N is equal to $\frac{1{\cdot}104}{\log N^{\frac{1}{2}}}$. It is rather more convenient to regard N as entirely unknown instead of being given as odd, on which supposition the chance of its being a prime would be $\frac{1{\cdot}104}{2\log N^{\frac{1}{2}}}$ or $\frac{1{\cdot}104}{\log N}$.

Hence for very large values of N the sum of the logarithms of all the primes inferior to N might be expected to be something like $(1{\cdot}104)N$. This does not contravene Tchebycheff's formula (Serret, "Cours d'Algèbre Supérieure," 4me ed., vol. ii. p. 233), which gives for the limits of this sum AN and BN, where $A = 0{\cdot}921292$, and $B = \frac{6A}{5} = 1{\cdot}10555$; but does contravene the narrower limits given by my advance upon Tchebycheff's method (see *Am. Math. Journal*, vol. iv. Part 3), according to which for A, B, we may write A_1, B_1, where

$$A_1 = 0{\cdot}921423,\ B_1 = 1{\cdot}076577.$$[1]

That the method of probabilities *may* sometimes be successfully applied to questions concerning prime numbers I have shown reason for believing in the two tables published by me in the *Philosophical Magazine* for 1883.[2]

New College, June 10. J. J. SYLVESTER.

[1] It always seems to me absurd to speak of a complete proof, or of a theorem being rigorously demonstrated. An incomplete proof is no proof, and a mathematical truth not rigorously demonstrated is not demonstrated at all. I do not mean to deny that there are mathematical truths, morally certain, which defy and will probably to the end of time continue to defy proof, as, *e.g.*, that every indecomposable integer polynomial function must represent an infinitude of primes. I have sometimes thought that the profound mystery which envelops our conceptions relative to prime numbers depends upon the limitation of our faculties in regard to time, which like space may be in its essence poly-dimensional, and that this and such sort of truths would become self-evident to a being whose mode of perception is according to *superficially* as distinguished from our own limitation to *linearly* extended time.

[1] Viz. $A_1 = \frac{51072}{50999}A$, and $B_1 = \frac{59595}{50999}A$, the values of which are incorrectly stated in the memoir. Strange to say, Dr. Gram, in his prize essay, previously quoted, on the number of prime numbers under a given limit, has omitted all reference to this paper in his bibliographical summary of the subject, which is only to be accounted for by its having escaped his notice; a narrowing of the asymptotic limits assigned to the sum of the logarithms of the prime numbers series being the most notable fact in the history of the subject since the publication of Tchebycheff's memoir. Subjectively, this paper has a peculiar claim upon the regard of its author, for it was his meditation upon the two simultaneous difference-equations which occur in it that formed the starting-point, or incunabulum, of that new and boundless world of thought to which he has given the name of Universal Algebra. But, apart from this, that the superior limit given by Tchebycheff as $1{\cdot}1055$ should be brought down by a more stringent solution of his own inequalities to only $1{\cdot}076577$—in other words, that the excess above the probable mean value (unity) should be reduced to little more than ⅔rds of its original amount—is in itself a surprising fact. Perhaps the numerous (or innumerable) misprints and arithmetical miscalculations which disfigure the paper may help to account for the singular neglect which it has experienced. It will be noticed that the mean of the limits of Tchebycheff is $1{\cdot}01342$, the mean of the new limits being $0{\cdot}99900$. The excess in the one case above and the defect in the other below the probable true mean are respectively $0{\cdot}01342$ and $0{\cdot}00100$.

[2] A principle precisely similar to that employed above if applied to determining the number of reduced proper fractions whose denominators do not exceed a given number n, leads to a correct result. The expectation of two numbers being prime to each other will be the product of the expectations of their not being each divisible by any the same prime number. But the probability of one of them being divisible by i is $\frac{1}{i}$, and therefore of two of them being not each divisible by i is $\frac{1}{i^2}$. Hence the probability of their having no common factor is

$$(1-\tfrac{1}{4})(1-\tfrac{1}{9})(1-\tfrac{1}{25})(1-\tfrac{1}{49})\ldots\ldots\ ad\ inf.,\ i.e.\ \text{is}\ \frac{6}{\pi^2}.$$

If, then, we take two sets of numbers, each limited to n, the probable number of relatively prime combinations of each of one set with each of the other should be $\frac{6n^2}{\pi^2}$, and the number of reduced proper fractions whose denominators do not exceed n should be the half of this or $\frac{3n^2}{\pi^2}$. I believe M. Césaro has claimed the prior publication of this mode of reasoning, to which he is heartily welcome. The number of these fractions is the same thing as the sum of the *totients* of all numbers not exceeding n. In the *Philosophical Magazine* for 1883 (vol. xv. p. 251), a table of these sums of totients has been published by me for all values of n not exceeding 500, and in the same year (vol. xvi p. 231) the table was extended to values of n not exceeding 1000. In every case without any exception the estimated value of this totient sum is found to be intermediate between

$$\frac{3n^2}{\pi^2} \text{ and } \frac{3(n+1)^2}{\pi^2}.$$

Calling the totient sum to n, $T(n)$, I stated the exact equation

$$T(n) + T\left(\frac{n}{2}\right) + T\left(\frac{n}{3}\right) + T\left(\frac{n}{4}\right) + \ldots\ldots = \frac{n^2+n}{2},$$

from which it is capable of proof, without making any assumption as to the form of Tn, that its asymptotic value is $\frac{3n^2}{\pi^2}$. The functional equation itself is merely an integration (so to say) of the well-known theorem that any number is equal to the sum of the totients of its several divisors. The introduction to these tables will be found very suggestive, and besides contains an interesting bibliography of the subject of Farey series (*suites de Farey*), comprising, among other writers upon it, the names of Cauchy, Glaisher, and Sir G. Airy, the last-named as author of a paper on toothed wheels, published, I believe, in the "Selected Papers" of the Institute of Mechanical Engineers. The last word on the subject, as far as I am aware, forms one of the *interludes*, or rather the *postscript*, to my "Constructive Theory of Partitions," published in the *American Journal of Mathematics*.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 3.—"Electro-Chemical Effects on Magnetizing Iron." Part II. By Thomas Andrews, F.R.S.E. Communicated by Prof. G. G. Stokes, P.R.S.

The present paper contains the results of a further study of the electro-chemical effects observed between a magnetized and an unmagnetized bar when in circuit in certain electrolytes, recorded in Part I. of this research. The method of experimentation was generally similar to that pursued and described in Part I., though it was necessary to introduce numerous modifications of detail and also new modes of experimentation. The bars experimented on were of specially prepared wrought-iron and cast-steel; all the rods were finely polished, and the general physical properties of the metals are given in Table B. Steel bars were employed in some of the experiments, because after magnetization by the coil their subsequent influence as permanent magnets could be observed. The reagents employed as electrolytes consisted of various solutions of bromine, ferric chloride, and chlorine water, ferrous sulphate, ferric chloride, cupric chloride, cupric sulphate, cupric nitrate, cupric acetate, cupric bromide, nickel chloride, hydrochloric, acid, nitric acid, and potassium chlorate. A pair of bars in each experiment were immersed as elements in the solution in the special apparatus employed, in circuit also with a delicate galvanometer, and after normal galvanic equilibrium had been obtained the bar within the coil was magnetized for various periods and the magneto-chemical effect observed. It was found to vary with the nature of the metal and solution employed, and also with the extent of the magnetization of the metals. The average results of many repeated experiments are given in numerous detailed tables, and it was generally found that a magnetized bar became electro-positive to an unmagnetized one. In Parts I. and II. a total of near 600 iron and steel bars have been experimented upon. Experiments were also made showing that local currents were developed in a magnetized bar between the more highly and less magnetized parts thereof, when the rod was immersed in suitable solutions acting chemically upon it.

Interesting experiments have also been made in connection with the influence of magnetization on the action of strong nitric acid on iron and steel. In course of the research the results of an extensive quantitative study of magneto-chemical phenomena have been recorded, the effect in connection with a considerable variety of typical reagents having been carefully observed; with some reagents the effect was found to be comparatively small, in other instances it was somewhat considerable. The general conclusion was that under the conditions recorded a magnetized bar was electro-positive to an unmagnetized one, when the two were immersed in suitable solutions, and that the extent of the result was in some degree dependent both on the nature and strength of the solution, and also on the extent of the magnetization of the metal.

June 7.—"Note on the Volumetric Determination of Uric Acid." By A. M. Gossage, B.A. Oxon.

It seemed improbable that the method recently proposed by Dr. Haycraft for the volumetric determination of uric acid in urine could be accurate, since both Salkowski and Maly had previously shown that the precipitate of silver urate obtained from urine contains variable quantities of other urates. To test the method, I examined samples of various urines both by his method and by that of Salkowski, which is universally acknowledged to be the most trustworthy. The mean percentages of uric acid found were as follow:—

Experiment	I.	II.	III.	IV.	V.
Haycraft's method	0·108	0·076	0·082	0·072	0·108
Salkowski's method	0·084	0·035	0·051	0·035	0·084

The results obtained by Haycraft's method were always considerably higher than those obtained by Salkowski's. The reason of this is that Dr. Haycraft has assumed that the silver precipitate from urine consists of a urate containing only 1 atom of silver in the molecule, whereas the proportion of silver in silver urate corresponds more nearly to 2 atoms in the molecule. Assuming, then, that there are 2 atoms of silver in all the molecules of the urate, and dividing the results obtained by Haycraft's method by two, we see that the results so obtained are usually lower than those obtained by Salkowski's method, and that the proportion between the results by the two methods varies, as would be expected from Salkowski's researches.

EDINBURGH.

Royal Society, June 4.—Dr. John Murray, Vice-President, in the chair.—Dr. G. Sims Woodhead exhibited a series of photographs of large sections of the lung.—A paper by the Astronomer-Royal for Scotland on Scottish meteorology for the last thirty-two years was read.—Dr. E. Sang read a paper on John Leslie's computation of the ratio of the diameter to the circumference of a circle.—A paper by Lord Maclaren on the figure of aplanatic lenses was read.—Prof. Tait submitted some quaternion notes.

June 18.—The Hon. Lord Maclaren, Vice-President, in the chair.—The Secretary exhibited M. Amagat's photographs of the crystallization of chloride of carbon under pressure alone.—A paper by Prof. W. Carmichael McIntosh and Mr. E. E. Prince, St. Andrews' Marine Laboratory, was communicated.—A paper by Prof. Anglin on certain theorems mainly connected with alternants, was read.—Prof. Haycraft and Dr. R. T. Williamson gave a demonstration of a method, which can be used chemically, for estimating quantitatively the alkalinity of the blood.—A preliminary notice of a paper by Dr. G. N. Stewart on electrolytic decomposition of proteid substances was submitted.—Papers by Dr. A. B. Griffiths, on the Malpighian tubules of *Libellula depressa*, and on a fungoid disease in the roots of *Cucumis sativa*, were communicated.

PARIS.

Academy of Sciences, July 2.—M. Janssen, President, in the chair.—Reply to Mr. Douglas Archibald's strictures on the subject of storms, by M. H. Faye. The storm laws, as established by the observations of Capper, Piddington, Reid, and Redfield, are declared to be one of the greatest discoveries of the century, and their truth is here vindicated against the recent attacks of Prof. Loomis, Dr. Meldrum, and especially Mr. E. Douglas Archibald, in NATURE for June 14 (p. 149). Archibald's diagram of the Manilla cyclone of October 20, 1882, is here reproduced, and it is contended that these highly characteristic phenomena can be explained only by admitting a *descending* motion in the central part of the cyclone. But on the opposite supposition it is precisely here that the *ascending* current should be strongest, for this central region corresponds exactly to the minimum of barometric pressure. The error in this theory of his opponents is attributed to a confusion between two quite distinct kinds of depressions, a confusion which has for fifty years impeded the progress of meteorological science and increased the perils of navigation.—On the cultivation of Bœmaria in Provence, by M. Naudin. The author reports that the white species (*B. nivea*), lately introduced from China, thrives well in the Antibes district, where the green variety (*B. utilis*) has long been acclimatized. The foliage makes excellent fodder for cattle.—Automatic control of the velocity in machinery of variable action, by M. H. Léauté. An apparatus, the result of many years' study, is here described, by means of which the action of engines may easily be regulated, even when required to work at varying rates of speed.—On a compass enabling the observer to find the meridian on land or water despite the disturbing influence of iron, by M. Bisson. An ingenious apparatus is described by means of which the compass may be prevented from deviating more than one-tenth of a millimetre, even in the neighbourhood of iron. It has been tested with satisfactory results on board several French ironclads, and works equally well by land or sea.—On the snows, ice, and waters of Mars, by M. Flammarion. In reply to some recent remarks on the meteorological condition of this planet, it is pointed out that the varying state of the polar ice-caps has long been carefully observed by Maedler, Schiaparelli, and others, the inference being that Mars is not in a state of glaciation. On the contrary its temperature is equal to, if not higher, than that of the earth, and its polar snows melt periodically to a far greater extent than on our planet.—On the graphic representation of numerical divisors, by M. Saint-Loup. By adopting a rectangular distribution of the numerals, the author arrives at some practical results on the general grouping of the prime numbers.—On the determination of the constants and of the dynamic coefficient of elasticity for steel, by M. E. Mercadier. By the method already indicated (*Comptes rendus*, July and August, 1887), the author here determines the relation $\frac{\lambda}{\mu}$ of the constants for steel. In a future paper will be given the results of the experiments undertaken to determine the coefficients of electricity.—On the mechanism of electrolysis by the process of alternative currents, by MM. J. Chappuis and G. Maneuvrier. The recognized impossibility of electrolysing the sulphate of copper by alternative currents is explained by the theory that the copper deposited on each electrode by one of the currents is immediately dissipated by the inverse current. This explanation is here justified by the authors' experiments, which render *visible* the decomposition of the sulphate of copper, as they had previously done for acidulated water. From this experimental study they hope to deduce the general principles for the practical application of alternative currents in the process of electrolysis.—Application of Carnot's principle to endothermic reactions, by M. Pellat. By distinguishing between the temperature of the bodies giving rise to the endothermic reaction and that of the source supplying in the form of heat the energy needed for the reaction, the author is led by the application of Carnot's principle to a law analogous to that of Potier, but of a more general character.—On the hydrochlorate of cupric chloride, by M. Paul Sabatier. The author admits the priority of M. Engel's researches on the properties and preparation of this substance, but points out that this chemist gives it a very different composition from that which he has himself obtained, and which is represented by the formula CuCl, HCl, 5HO.—On the artificial reproduction of the micas and of scapolite, by M. Doelter. A process is described, by means of which the author has artificially reproduced the chief minerals of the mica group, as well as of natural scapolite. He has already effected the synthesis of biotite, phlogopite, muscovite, and lepidolite (zinnwaldite variety).—Fresh physiological researches on the organic substance which has the property of hydrogenating sulphur, by M. J. de Rey-Pailhade. During his further study of this substance, to which he has given the name of philothion, the author has determined several new facts, amongst others that when the yeast is treated by reagents, the death of the organism always precedes the destruction of this organic substance. Philothion is generated by the physiological development of the yeast, and combines with sulphur according to an equation of which sulphuretted hydrogen is a factor. Acting as a diastase, it adds a fresh proof to M. Berthelot's theory of fermentation. Lastly, it is the first known instance of a substance extracted from a living organism which has the property of hydrogenizing sulphur.—Prof. Langley has been elected by a large majority to succeed the late M. Roche as Corresponding Member of the Academy on the Section of Astronomy.

BERLIN.

Physiological Society, June 22.—Prof. du Bois Reymond, President, in the chair.—Dr. H. Virchow spoke on the blood-vessels of the eye in Carnivora as worked at by Bellarminoff under his direction. The communication was illustrated by drawings and the exhibition of preparations. The points of most general interest which stand out from among the mass of details in this research are that the blood-vessels of the eye have a tendency to form rings from which a large number of fine branches pass posteriorly; further that the arrangement is often very different in different classes of animals, thus, for instance, the course of the arteries in the eye of a dog as compared with that of a rabbit is such that the dog's eye must be turned through an angle of 180° in order to make the course of its arteries correspond with that of the rabbit's eye.—Dr. Heymans communicated the results of his researches on the nerve-endings in the unstriated muscle-fibres of the medicinal leech. In the alimentary canal of the Hirudinea the muscle-fibres are placed both longitudinally and circularly; they consist of a contractile sheath and a protoplasmic axis containing the nucleus, and either have pointed ends or else divide into two or more branches, each of which then ends in a point. The muscle-fibres are separated from each other by large interstitial spaces filled with connective tissue, in which the nerve-plexus lies and sends fine nerve-branches into the muscle-fibres. The nerves end partly as extremely fine filaments and partly as round, flattened end-plates, and in no case does the nerve-ending penetrate the contractile sheath of the fibre so as to come into connection with the protoplasmic axis. In the vascular system of the leech the muscular layers are principally disposed in a circular fashion, but frequently the speaker noticed that at some point or another a circular fibre divided itself into two branches, and that the latter were then bent through a right angle so as now to pass in a longitudinal course in the wall of the blood-vessel. The nerve-endings in the fibres of the vascular system are the same as in those of the alimentary canal. Similarly, the muscle-fibres in the vascular system do not lie in close apposition to each other, but are separated by interstitial spaces; each fibre also contains only one nucleus.—Dr. van der Gehnchten, of Holland, gave a short abstract of his observations on the minute structure of striated muscles in Vertebrata and Arthropoda. He described the appearance of the muscles in the fresh conditions, after the coagulation of the myosin and after the solution of the amorphous proteid, and illustrated his statements by drawings. According to these researches the muscle-fibre of the Vertebrata consists of a network of doubly-refractive filaments, whose meshes are filled with the semi-fluid plasmatic substance. In Arthropoda the structure differs according as the muscle is taken from the wings or the legs; when taken from the latter the structure is extremely similar to that in the Vertebrata. In the discussion which followed, Dr. Benda pointed out that being engaged for years in studying the structure of striated muscle he had often obtained preparations similar in appearance to those of Dr. van der Gehnchten, but his interpretation of these appearances was very different. He pointed out, moreover, that he had often observed transitional forms between the muscles of the leg and wing in Arthropoda and those of Vertebrata. Without entering into any details, Dr. Benda gave it as his opinion that the network in a striated muscle-fibre must not be regarded as contractile, but as a connective-tissue interstitial substance, in whose interspaces the really contractile muscle fibrillæ lie.

IN the report of the meeting of the Physical Society in NATURE of June 21, p. 192, for "Dr. Lummer" (line 37 from the bottom) read "Prof. von Helmholtz."

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Geologische Studien ueber Niederlandische West Indien, 2te. Lief. Holländisch Guyana, K. Martin (Brill, Leyden).—Lectures on Geography: Lieut.-General R. Strachey (Macmillan).—British Dogs, No. 21: H. Dalziel (Gill).—Speaking Parrots, Part 3: Dr. K. Russ (Gill).—India in 1887: Prof. R. Wallace (Oliver and Boyd).—Annual Report of the Aëronautical Society of Great Britain for the years 1885–86 (Hamilton).—Beiblätter zu den Annalen der Physik und Chemie, 1886, No. 6 (Leipzig).—Geological Magazine, July (Trübner).—Journal of Anatomy and Physiology, July (Williams and Norgate).—Jahrbuch der Meteorologischen Beobachtungen der Wetterwarte der Magdeburgischen Zeitung, Jahrg. v., 1886 (Magdeburg).—Zeitschrift für Wissenschaftliche Zoologie, xlvi. Band, 4 Heft. (Leipzig).—Mind, July (Williams and Norgate).—Notes from the Leyden Museum, vol. x. No. 3 (Leyden).—Journal of the Chemical Society, July (Gurney and Jackson).

CONTENTS.

THURSDAY, JULY 19, 1888.

THE CHOICE OF A CHEMIST TO THE NAVY.

SIR HENRY ROSCOE'S watchful regard of the true interests of science was evidenced by his recent question in the House to the First Lord of the Admiralty, whether, in consequence of the resignation of Dr. Debus of the Professorship of Chemistry in the Royal Naval School at Greenwich, it was proposed to reduce the status of this post to a lectureship; and if so, whether he would take into consideration the inexpediency of this step being taken, in view of the importance to naval officers of a knowledge of this science, and of the necessity that in the Government Naval School the post in question should be filled by a gentleman of the highest possible scientific position and attainments.

Lord G. Hamilton is reported to have replied that the resignation of Dr. Debus, Professor of Chemistry at the Royal Naval College, had only just been received by the Admiralty, and therefore it would be premature to make any statement as to the manner in which it may be thought desirable to fill the vacancy so caused. The policy of the Admiralty was always to inquire into the circumstances of any appointment of this kind that may fall vacant, with the view of adjusting the salary to the requirements of the day.

It must be obvious that this statement savours most strongly of officialism, and that it affords no information whatever with regard to the views and intentions of the Admiralty. We have already clearly indicated what are the requirements of the day, and Sir Henry Roscoe has given emphasis to our views; but it is more than probable that unless attention be again directed to the importance of the issues involved in the appointment of a chemist to the Navy the course of action indicated in our previous article as likely to be followed will inevitably be adopted.

We therefore without hesitation again urge that in a case of this kind only one course can be adopted with safety by the Admiralty, if the interests of the nation are to be considered—that course is to engage the services of the best man available. No candidate's claims should be considered unless it can be shown that he is a trained chemist, and has been actively engaged in the pursuit of new knowledge; and unless it appear probable that he is enthusiastic and single-minded enough to continue to interest himself in research work and to lead his senior pupils to engage in research. We are fully aware that in imposing this standard we are demanding higher qualifications than many may consider necessary; that some may even think that nothing more is required at Greenwich than one who will teach young midshipmen the elements of chemistry and simple analysis fairly well; but to this we demur most emphatically, believing it to be incontestable that the science of chemistry may minister directly and indirectly in so many ways to the wants of our Navy that it is essential to give it the highest possible footing in the course of study at a naval college.

In the recently published life of W. E. Forster, a fragment of conversation at a dinner party preserved by Mrs. Forster is recorded which will be aptly quoted here. "Mr. —— said that —— was always going about asking people what was the ideal towards which they were working, and there was a laugh at the notion. But my husband did not join in it, saying that, for his part, if he was not constantly thinking of the ideal which he was working up to, he should not be able to get on at all." We venture to think that the infertility of British chemists and the inferior position which chemistry holds in this country, especially at our two great universities, as well as our failure to excel in those industries in which chemistry plays an important part, are due to the absence of an ideal among our chemists generally in any way approaching to that which has long obtained in Germany, where no higher grade appointment can be bestowed except on a man who is master of his subject, and not a past-master even but an active worker; and it is the absence of any such ideal which in cases like the present renders it possible for the authorities to entertain the suggestion of reducing the status of the post at Greenwich.

We believe that a master chemist is required at Greenwich for a variety of reasons. Firstly, as a matter of national honour; secondly, because, as we have already said, the subject must be taught technically, *i.e.* with direct reference to the knowledge and requirements of the students; thirdly, because the students are not only young beginners, but are of all ages, including many men of ripe experience, and it is scarcely necessary to remark that no one who is not a thorough chemist can possibly gain the sympathies of this latter class; and, lastly, because no one who is not himself actively engaged in research will remain *au courant* with the progress of knowledge, and will be able to select and incorporate into his teaching important new facts, thus avoiding the otherwise inevitable tendency to teach in a stereotyped and bookish fashion from year to year.

The proper man being found, he should be told at the outset that it is expected that when engaged in investigation he will devote his attention primarily to problems of importance in the Navy; a short intercourse with men versed in naval affairs and requirements would soon furnish an active-minded chemist with more than sufficient subject-matter meriting attentive study. It is more than probable that if a good example were set, and a spirit of enthusiasm kindled among the students, officers who had been led to take a real interest in chemistry would be willing, in the intervals of enforced inactivity when they were not on service, to devote themselves to research; and if but moderate encouragement were given to such men, we can conceive that Greenwich at no distant date might become an important school of naval research.

Unfortunately it is only too obvious that the public are slow to heed the repeated warnings of experts that our competitors in commerce are outrunning us largely because of their readiness to avail themselves of the aid which science can afford to industry. The evidence that foreign Governments are more anxious than is ours to make every possible use of science in the service of the Army and Navy is also growing daily; but we are confident that in the present instance the danger of the retrograde action which appears to have been contemplated having been pointed out, the naval authorities will not allow themselves to be guided by shortsighted advisers, and will no longer countenance any change which does not enhance their opportunities of receiving aid from so all-important a branch of science as chemistry.

NEW WORKS ON LEPIDOPTERA.

South African Butterflies: a Monograph of the Extra-Tropical Species. By Rowland Trimen, F.R.S., &c., assisted by James Henry Bowker, F.Z.S. Vols. I. and II. Royal 8vo. (London: Trübner and Co., 1887.)

Descriptions of New Indian Lepidopterous Insects from the Collection of the late Mr. W. S. Atkinson, M.A. Part III. Heterocera (continued). By Frederick Moore, F.L.S., &c. 4to. (Calcutta: Published by the Asiatic Society of Bengal, 1888.)

MORE than twenty-one years have elapsed since Mr. Trimen finished the publication of his "Rhopalocera Africæ Australis." During the whole of this time he has kept the subject of South African butterflies steadily in view, and the number of additional species discovered in South Africa is so large that he has chosen a new title for his book rather than call it a second edition of the old one.

Between 1866 and the present time the number of species of butterflies known to inhabit South Africa has swollen from 222 to 380, and instead of a small octavo volume we have now before us two out of three royal octavo volumes of goodly dimensions. This progress in the study of a favourite group of insects in South Africa probably represents a similar progress in the knowledge of the butterflies of the world, for nearly everywhere it has been increased by rapid strides.

Mr. Trimen has had the advantage of living in the country the butterflies of which he describes, and he has been in close correspondence with numerous enthusiastic helpers, foremost amongst whom is Colonel J. H. Bowker, whose name appears on the title-page as Mr. Trimen's coadjutor.

The earlier chapters of the work are devoted to general subjects relating to insects and leading up to the special subject in view. In all this portion Mr. Trimen has exercised admirable judgment, giving the leading points in concise but clear language. The classification adopted is that of Mr. H. W. Bates, which has now stood the test of many years' practical working, hardly any important alteration having been made in its main features since it was published. Still, much remains to be done before some of the great families, such as the Lycænidæ and Hesperidæ, and sub-families, such as Satyrinæ and Nymphalinæ, can be reduced to order.

As is well known, the front pair of legs in the imago provides one of the most important characters for determining the families of butterflies. Their examination affords a most interesting study. Owing to improved methods of preparing these limbs, whereby their scaly clothing is either destroyed or rendered invisible, they can be conveniently arranged for microscopic examination. The full extent to which they are atrophied is thus clearly revealed. The front legs of the males in the members of some families have their tarsal joints either more or less fused together or reduced to a single atrophied joint; but the variation in the extent to which this takes place is great. It sometimes also happens that when a number of individuals are examined, one will be found in which rudimentary spurs appear, and even unsymmetrically and attached to one tarsus only and not the other of the same insect. In some cases recently examined, males in the Erycinidæ have been found with the front legs furnished with the full complement of joints and with claws. The like occurs in the Lycænidæ both in America and in South Africa. Such cases, however, are exceptional, and though they break down to some extent the universal application of these characters to the discrimination of families, discrepancies are only to be expected, and the wonder is there are so few of them. Mr. Trimen appears to have studied this part of his subject with care, but a closer examination than is usually made will repay the labour of arranging the preparations. Though the variation in the relative lengths of the femur, tibia, and tarsus have been compared, the coxa has seldom been taken into consideration. Yet it, too, furnishes useful points for distinguishing forms, and in the case of the Erycinidæ the prolongation of this joint in the male front leg beyond its junction with the trochanter is diagnostic of the family. Mr. Trimen has not made any use in his classification of the varied structures presented by the secondary sexual characters of the terminal segments of the body; but there can be little doubt that, as improved methods of preparation are discovered, these characters will be found very useful in determining the relationship of species if not of genera.

The limits of the fauna treated of, as the title of the book states, extend from the Tropic of Capricorn southwards to the Cape of Good Hope. This district forms a sub-region of the great African or Ethiopian region. Its distinguishing characteristics are mainly negative, only six out of the sixty-nine genera not being found elsewhere, though 195 out of 380 of the species are peculiar. Whether the northern limit of this section of the African fauna really lies along the tropic remains to be seen, as our knowledge of the butterfly fauna north of this line is very meagre: of the interior we know nothing, and of the coasts not much. Regarding the internal distribution of the species, it would appear that the western and central portions, as well as that in the neighbourhood of the Cape, are poor in species. During a residence of over twenty-five years, Mr. Trimen has succeeded in capturing only forty-seven species within a radius of twelve miles from Cape Town. In the eastern districts the fauna is richer: Natal produces 206 species, and in the neighbourhood of Delagoa Bay many additional species occur. Each species is very fully described in this work, and many useful notes are added whereby the allied forms may be discriminated. Their history and range are also given with great precision. The larvæ and pupæ of many species are described, and this feature is a very acceptable addition, as most works on exotic Lepidoptera are silent on the subject.

The portion of the introduction that will be read with the greatest interest is that which relates to protection, resemblances, mimicry, &c. (pp. 32-40). A concise summary of the best work on this subject is given; and the instances furnished by the African butterfly fauna are described more in detail. Some years ago Mr. Trimen brought forward some very interesting cases of mimetic resemblances in butterflies, the most important being that in which *Papilio merope* is involved. He was able to prove that, wherever it is found, the females of this species take the pattern of a *Danais*, and though the males hardly vary over a very wide area, the female varies with the *Danais* in each district except in

Madagascar and Abyssinia, where females and males are alike.

The plates, on which a selection of the less known species are depicted, are chromolithographs, and are rather uneven in quality, as is usually the case in drawings of butterflies by this process. Some of the figures are admirable, while others, such as the Lycænidæ, are not at all satisfactory. Notwithstanding this defect, we can safely say that Mr. Trimen's "South African Butterflies" is the best-planned and best-executed work of its kind that has yet appeared. It cannot fail to promote an accurate study of the Lepidoptera of the country of which it treats; and it may serve as a model for entomologists to follow when writing of the butterflies in other portions of the world.

Mr. F. Moore's book on new Indian Lepidoptera, the third and concluding part of which is before us, is a work of a very different character from Mr. Trimen's, and consists of descriptions of new species from the collection of the late Mr. W. S. Atkinson. Mr. Moore has long been engaged on work of this kind, and every year issues scores of descriptions of Lepidoptera, chiefly Heterocera, of India. His former position as Assistant Curator to the Indian Museum placed him in communication with a number of correspondents, who have helped him to gather together probably the most important collection of Indian Lepidoptera in existence. Without such a collection no work like the present could be undertaken. We confess, however, to a feeling of despair as to the future of the subject treated of when we glance at the descriptions before us. They are descriptions of the barest kind, scarcely relieved by a few comparisons, and with hardly a note to break the tedious monotony of the frequent repetition of the same characters over and over again. Whether future workers will be able to determine species by them without reference to the types is more than we can say, but we do not envy them the task of trying the experiment. And here we note with regret that the types of these species are not to be found in our National Collection, but in the possession of Dr. Staudinger, of Dresden, and some of them in Mr. Moore's own cabinets. This might have been otherwise had more interest been shown by our home authorities in the productions of our great dependency.

On the title-page of this part it is stated that members of the families Pyralidæ, Crambidæ, Geometridæ, Tortricidæ, Tineidæ are treated of, but in the body of the work new species are referred to no less than twenty-three other families of Heterocera. In the present state of the classification of Heterocera such an oversight is hardly to be wondered at. No serious attempt has been made for many years to place the classification of the moths on a sound and definite basis. The old systems are to a great extent obsolete, and the more recent attempts to modify them, by their halting and spasmodic character, have increased rather than lessened the confusion.

Mr. Moore has introduced a number of new generic names into this work, but he seldom gives any clue to the relationship of the proposed new genus. On p. 283 he commences descriptions of some "additional species" by introducing five new generic names for sections of the great genus *Papilio*. Whether this genus should be divided into many or left as a large aggregate of species is a disputed point, but we have no hesitation in condemning the plan here adopted of thrusting these names upon us in this piecemeal fashion. To anyone who will give the whole subject a careful examination and work out the diagnostic characters of the groups of this wonderful genus we are prepared to give a patient and respectful hearing; but to name sections here and there, with brief descriptions which are anything but diagnostic, is a practice to be deprecated.

Three coloured plates accompany this part, on which eighty-seven species are depicted. These are carefully drawn and nicely coloured, and form a substantial addition to the book.

We note that the first sheet of this part bears the date of September 5, 1887, but the title-page that of 1888. The meaning of this is not obvious, as the former is valueless in face of the later date of the title-page and wrapper.

FACTORS IN LIFE.

Factors in Life. By H. G. Seeley, F.R.S. "People's Library Series." (London: Society for Promoting Christian Knowledge, 1888.)

THE book before us is one of the useful series of household guide books, published by the Society for Promoting Christian Knowledge and intended to instruct the people in some of the more important laws of health. There are so many guide books on this subject at the present time that Prof. Seeley has, we feel sure, found it a difficulty of no slight kind to put before his readers the material he had in hand, so as to feel that he was supplying anything that by its novelty could be considered acceptable. Happily the enormous importance of his theme has come to his aid, and has enabled him to bring forth an essay which makes up in earnestness whatever it may, by very necessity, want in originality; for health is like truth—it can never be confirmed enough, nor have too many able expositors.

The factors in life treated of by our author are health, food, and education. Health he defines, very tersely, as "the condition of life in which the body produces more energy than is lost in performing our work"; and then he proceeds to indicate the various methods, habits, and practices by which it can be secured by the individual and by the community at large. With much prudence the Professor dwells on the obstacles that lie in the way of health from the expense that attends their application. He illustrates this uncommonly well in regard to cleanliness. "The difficulty," he says, "of securing the universal practice of the habit is chiefly a matter of expense. There are few pleasures more costly than perfect cleanliness, since it implies labour in every detail." Here, too, he enforces what all practical sanitarians have foreseen, that such labour can never be satisfactory unless the woman of the house, the wife, can direct and take part in it, "because servants can in no other way become of the same flesh and blood as their employers. Personal cleanliness to be of any value must extend to all members of a household. It is as important for the servants as for the mistress, for they are often exposed to greater chances of infection, and have greater capacity for diffusing disease. If the cook and the kitchen are not scrupulously clean, the health of the household suffer with every touch given to food, and many an obscure derangement of health which baffles medical skill is due to this poison of dirt." Touching the question of national cleanliness, we are very glad to find Prof. Seeley spotting the greatest of all political evils of a social kind—the evil of allowing a monopoly to companies for the supply of fresh water to the community. "The wisdom of the State," he affirms, "never permitted any greater obstacle to come between the people and their health than the monopoly of water companies who make water an article of trade;" from which saying we only dissent in regard to one word—the word wisdom, for which the truer word folly ought, we think, to be substituted.

Prof. Seeley takes a decisive view of the duties of the members of the profession of medicine, to whom he would apply the drastic reformation inaugurated by that heathen Chinee, who makes the doctor earn his fees not by treating the man that is sick, but by keeping the man that is whole always free from sickness. The doctor, according to this prescription, would keep up the health of the household by contract, through which plan there would be no necessity for sick-hospitals, sick-beds, or any other of the extensive and costly methods now in use for keeping up the cure of disease. The whole art of medicine would be an art of prevention; and cure, now the almost sole object of the highest skill in medicine, would be quite subordinate to prevention. But where then would poor medical science be landed? Every man would be his own general practitioner, every housewife would be a physician, every old woman who had gained most experience from observation of preventive measures would be a consulting physician, and there would be nothing to cure. Fie on you! learned, if not jealous, Professor, for suggesting such a heartless disintegration of the great and noble sciences of pathology and therapeutics. The next time we meet you we will not speak to you unless you publicly recant such brazen heresy, and repent in dust and ashes. Seriously, the idea of such a change is not far off, and indeed has, to some extent, commenced amongst the more advanced members of the educated community. It is an idea that will spread far and wide, and in half a century or so may be the fashion of the time.

On the topic of food our author is very explicit, and is strong in his recommendations to feeders generally that they should distinguish carefully between foods that are *bonâ fide* foods, and those which are merely stimulants. Tea and drinks of its class owe their popularity to their power of arresting waste or nervous exhaustion, and this constitutes their superiority over alcoholic drinks. Neither, perhaps, is food in the popular sense of the term. "Wine and its allies give a fillip to the nervous system, which enables exceptional work to be done at the price of increased nervous exhaustion, and draw a bill on the strength which must be met at a short date: while tea and its allies enable increased work to be done by making the dormant strength available, and discount, on favourable terms, the bills we hold on nervous energy." This is sound and plain teaching, told in a concise form, that deserves to be retold by all who have the advantage of learning from the volume before us.

We are glad to see that Prof. Seeley inclines calmly and judiciously to the advocacy of a more distinctive national leaning towards vegetable products as foods. He sees that the ease with which animal foods can be prepared for the table is greatly to the advantage of their popularity, until a better system of cookery is established throughout the land, in which vegetable foods shall play a more distinguished part than they have ever yet played in this country up to the present date. "The sum," he tells us, "that is annually spent on animal food in this country is more than £114,000,000, or upwards of a ninth of the national income; while the sum spent on bread, potatoes, and vegetables combined is £127,000,000. By a reformed diet it is probable that a substantial saving of about £30,000,000 a year might be made in the cost of nitrogenous food alone, without any serious change in national habits, and with advantage in every way."

Turning lastly to the essay on education as a factor in life, we find excellent rules for combining education with health, and both with good morals. "Education begins earliest in childhood, ends only in death, and survives death itself in its effects on after time." In fact, "Nature has appointed no period for education." These are some of the wise and prudent sayings which the author places before his readers, with many others on which we have not space to dwell. But it would not be just to conclude without directing attention to the *summum bonum* of educational efforts which, in his last pages, Prof. Seeley impresses on his countrymen. He deals here with the subject of education on its religious side. The religious feeling is, he contends, partly an inherited character of the race, and partly the product of education. But, unless it permeates and saturates life so that every act and endeavour of existence has a basis which unites them into one sustained movement onward towards higher things, he should not express what he conceives the religious side of the education of life should be. The sciences are the sisters of religion, in that they unfold something of the laws by which the universe is governed and by which the life of man is directed. "They are thus far the stepping-stones of faith. And those who have learned that health is the reward of moral discipline, that mental vigour may be augmented by the wise or moral use of food, and that education is the systematic exercise of moral responsibility in any or all the affairs of life, may find that in the practice and the pursuit of the truths of science they are conscious of a religious education which is a light to their feet." The words are true. The words are a true gospel—a gospel new and true and ever-extending; and we congratulate the religious Society which has had the courage to publish them, as heartily as we congratulate the author who has had the good sense and moral faith to send them forth for publication.

THE LANDSLIP AT ZUG.

Die Catastrophe von Zug, 5 *Juli*, 1887. (Zürich: Hofer und Burger, 1888.)

AN account of this catastrophe, written by Prof. Bonney, who visited the scene of ruin, has already appeared in the pages of NATURE (vol. xxxvi. p. 389). The present volume, compiled from official documents, gives a fuller history and more minute details of the results of the slip than were at that time accessible. It mainly consists of an elaborate report, written by Dr. A. Heim, the well-known Professor of Geology at Zürich, "Ober-ingenieur" R. Moser, and Dr. A. Burkli-Ziegler, to which are appended brief accounts of the incidents of the catastrophe, and of that which occurred in 1435, and lastly, a note on the disposal of the fund raised for the benefit of the sufferers. Plans and sections (extracted from the series which was attached to the above report) accompany the book, and indicate very clearly not only the amount of the mischief done, but also its cause, which, as already stated in these pages, is the existence of a deep deposit of silt beneath the superficial gravelly soil. The latter is but a very few feet thick, and suffices for the foundation of the less important buildings; the former constitutes the shelving bed of the lake to a depth of more than 100 feet. Borings made at various stations on the land, not far from the lake margin, have shown that this material remains incoherent to nearly the above depth, after which it becomes stronger. Hence there is always a danger of the underlying silt being squeezed outwards into and upon the bed of the lake, and the plans and sections furnished with the present volume show precisely how the accident occurred. There appear to have been some premonitory indications of the coming mishap, in addition to the subsidence in the new pier wall, which had already excited alarm. The inhabitants of certain houses, which afterwards fell, had observed sundry small displacements, which were especially shown by the jamming of doors and windows; cracking noises also had once or twice been heard. But the actual catastrophe was very sudden. About 3.20 p.m. the end of the quay wall, which had been completed up to a sort of little bastion, began to crack and sink. A quarter of an hour later came the first great slip, which caused the loss of seven lives. Except for some minor slips, there was then a pause for rather more than three hours, and then at 6.50 p.m. the second and greater slip occurred. A graphic account is given of the terror caused by this second catastrophe, which caused the loss of four more lives. A third, but comparatively unimportant, slip occurred at 10.15 p.m.

From the plan and sections it is evident that the second slip affected the larger area, both of the land and of the lake bed. Each slip forced the loose silt horizontally outwards, so as to form a delta-like deposit on the lake floor, thus diminishing the depth of the water sometimes by about 4 or 5 yards. At the first slip a triangular piece of ground, measuring about 80 yards along the shore, and some 40 yards to its apex inland, was destroyed, and the "delta" produced by this, which in outline resembles a rather stout pear, is about 250 yards across the wider part, and apparently extends to about 450 yards from the shore. By the second slip not only a much larger piece of the land (with a rudely oblong boundary) was removed, but the lake bed opposite to it, for a distance of 220 yards, appears to have slipped, so as to form a kind of broad trench, resulting in an interval of deeper water some 50 yards wide. The material thus removed was deposited over the deeper part of the lake bed, covering a space not quite so wide as that occupied by the former "delta," but much more than double the length, for its end is placed 1020 metres from the shore, at a depth of 44 metres.

These elaborate maps and sections, with the results of investigations (by means of borings) into the nature of the lake bed, the level of the ground water, &c., give a high value to this publication, which may be commended to the notice of architects and engineers, as well as to those interested in the history of Switzerland.

OUR BOOK SHELF.

Turbans and Tails; or, Sketches in the Unromatic East. By Alfred J. Bamford. (London: Sampson Low, 1888.)

THE author of this book does not claim to have anything very new or striking to tell his readers. He has seen a good deal of India and China, and is content with reproducing, in a popular way, the impressions made upon him during his not very exciting sojourn in those countries. He has little to say about "the mild Hindu" or "the man of Han" that tends to make us think more highly of either. Mr. Bamford, like many English travellers, is apt to be impressed by the bad rather than by the good aspects of unfamiliar types of character; and some of his sweeping judgments would no doubt have been considerably modified if, in estimating the intellectual and moral qualities of Orientals, he had remembered more frequently and vividly than he has actually done, that thought and conduct in the East and West cannot always be fairly or wisely measured by the same standards. The book, however, has the merit of being written in a lively style, and the author's judgments, whether sound or unsound, invariably result from his own observation and reflection. Here is one of a good many suggestive anecdotes which brighten his pages: "Of what caste are you?" asked an Englishman of a native of India. "Oh," replied the native, "I'm a Christian—I take brandy shrab, and get drunk like you."

The Photographer's Note-book. By Sir David Salomons, Bart., M.A. (London: Marion and Co., 1888.)

BOTH amateur and professional photographers, and especially those who travel and take a great number of photographs per day, will find this little book very handy and useful, as it is of a very convenient size and contains enough space for inserting the particulars, such as number of stop, rapidity of shutter, remarks on the light, &c., of each of fifty-one dozen plates.

Formulæ for enlargement and depth of focus and rules for exposure are added, followed by a table, calculated by Messrs. Marion, of the correct quantities to be taken from 10 per cent. solutions to make up developers for all the best known plates. The book concludes with various tables, such as area enlarging, enlarging by linear dimensions, and equivalent focal lengths of lenses of different sizes and makers.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

"Cloud Electric Potential."

I DESIRE to draw the attention, more particularly of your electrical readers, to the following paragraph on p. 651 of the eighth edition (1884) of Deschanel's "Natural Philosophy," part iii., which appears distinctly at variance with the theory o thunderstorms as explained in Prof. Silvanus Thompson's "Elementary Lessons," and similar elementary treatises:—

"The coalescence of small drops to form large ones, though it increases the electrical density on the surfaces of the drops does not increase the total quantity of electricity, and *therefore cannot directly influence the observed potential.*"

Surely this entirely omits the fact that the capacity of a sphere is equal to its radius, and thus in the case of eight equal spheres coalescing into one (which is taken by Prof. Thompson), not merely would the density be doubled, but the potential of the *same quantity* would be increased *four* times.

In the well-known case given by Prof. Tait for the formation of a raindrop the potential of the same quantity might be increased fifty million times.

The source of the energy which is the cause of the increased potential in this case, is probably the molecular force of cohesion released during the act of condensation and union, the cohesion and the electricity being oppositely placed, so that while the former is running down hill (as it were) the latter is obliged to run up; the top of the hill answering to the critical moment for disruptive discharge.

In view of these facts, it seems to me that if the above sentence is not altogether erroneous, it is certainly ambiguous, and liable to breed false notions in the mind of the unreflecting and too credulous student. E. DOUGLAS ARCHIBALD.

Transparency of the Atmosphere.

IT may be, I think, desirable to correct an error which has crept into all the accounts of the extraordinary transparency of the atmosphere observed here last week. It occurred on Sunday, the 8th, and not on Monday, the 9th inst. I can confirm the several details as to the objects visible to the unassisted eye. But in one respect this effect was surpassed on August 20, 1887, when the double flash of the Dunkirk light, distant from this place about forty-five miles, was visible for several hours. This light could not be seen here on the 8th inst.

Pavilion Hotel, Folkestone, July 16. J. PARNELL.

Preserving the Colour of Flowers.

IN response to the inquiry of "A. W.," perhaps you will allow me to say that many years ago I met with Mdlle. d'Angeville, the first lady to ascend Mont Blanc. She possessed the largest and best preserved collection of Alpine flowers I have ever seen, and she assured me she never used anything but cotton-wool in her press, changing it, of course, frequently. Her gentians, pedicularias, and other delicate plants were perfect in colour; and having tried her plan myself, although with less care, and therefore with less success, I still have Alpine flowers which have retained their colour for twenty years.

54 Doughty Street, July 17. A. W. BUCKLAND.

Distribution of Animals and Plants by Ocean Currents.

IN connection with Miss Buckland's letter on this subject it may be interesting to note that, during a visit to Orotava, Teneriffe, in April 1887 (about the time mentioned by your correspondent), I observed and gathered a quantity of pumice-stone upon the seashore, the high tide mark being literally strewed with it. It seemed probable that it had been deposited there some weeks or possibly months previously, as, had there been any quantity floating about in the sea, I should have noticed it, being engaged at the time tow-netting in the neighbourhood and in the adjacent Canary Islands. There was no evidence of vegetable debris having accompanied the pumice, nor did I notice any pieces with barnacles attached.

Liverpool, July 13. ISAAC C. THOMPSON.

A Curious Resemblance

WHILST walking by the sea on the cliffs last Sunday, I perceived at a distance of about 1500 yards a flight of nearly forty ducks, travelling at a good pace 2 or 3 feet above the level of the water. To me they appeared exactly what the so-called "sea-serpent" would, eight or ten of the birds flying close together and forming the head, whilst the rest trailed behind and formed the body and tail. At intervals they disappeared. This was caused, I think, by the birds changing their course and flying either directly away or towards me; the former, I believe, in my case.

Some time afterwards I saw two other flights, and these resembled the first exactly, those with me also being surprised at their "snake"-like appearance. W. J. LOCKYER.

Thanet, July 16.

The "Sky-coloured Clouds."

THERE was a very bright display of these clouds last night. I could not perceive anything of them up to 10 p.m., though the sky was clear, but by 10.18 they had become conspicuous, and were brightest, so far as I observed, near midnight.

I have seen very little account in any English paper of the visibility of these clouds beyond England, nor do I know whether they have been seen elsewhere than in Northern Europe. Has there been anything published on these points in English?

Neither have I seen any reference to the extensive observations of Herr O. Jesse at Steglitz, with his suggestions to observers. He considers it very important that this unusually favourable opportunity should be utilized for learning the motions of currents at great heights in the atmosphere. He suggests that photographs taken simultaneously from two places at a distance of say 20 kilomètres would be useful for ascertaining the height of the clouds; but for this purpose the necessity arises of being able to calculate very accurately the azimuths and altitudes of different points in the photograph. Their height can likewise be determined, though less accurately, by observations of the limit of sunshine upon them. Herr Jesse proposes another way also, viz. by throwing an intense beam of electric light on the clouds; but I should doubt the practicability of this.

The direction and rate of motion could be best made out, he says, by the use of a cloud-mirror. The changes that take place in the forms of the clouds before they have moved far make it difficult to ascertain their motion accurately.

Herr Jesse further thinks the intensity of the light of the clouds in different positions should be determined; also that the sky should be examined in the day-time with a polariscope and photometer in the hope that the presence of the matter of the clouds, then invisible to the eye, might be revealed.

Sunderland, July 13. T. W. BACKHOUSE.

An Unusual Rainbow.

SINGULARLY enough I can record the appearance of a rainbow after sunset similar to that described by Mr. S. A. Hill (NATURE, March 15, vol. xxxvii., p. 464). I was not aware there was anything unusual in it until I read Mr. Andrew's communication, or would have written to you about it. I do not remember on what day I saw the rainbow, but it was about the date of that observed by Mr. Andrew. I called my wife's attention to it, and attributed it to the brilliant glow of the sunset tints. It had a secondary bow, and Mont Kogie as a dark background. E. L. LAYARD.

British Consulate, Noumea, May 15.

TIMBER, AND SOME OF ITS DISEASES.[1]

IX.

IF the leaves are stripped from a timber-tree early in the summer, or during their young conditions in the spring, the layer of wood produced in the current year—and probably even that formed next year—will be poor and thin. This is simply a fact of observation, and does not depend on what agent deprives the tree of its leaves. Those oaks which suffered so greatly from the ravages of certain tiny caterpillars this last summer (1887)—many of them having all their leaves eaten away before July—will have recorded the disaster by a thin annual ring of wood: it is true the more vigorous trees produced (at the expense of what stores of food materials remained over) a second crop of leaves in August, and so no doubt the zone of wood will prove to be a thin double one, but it is at the expense of next year's buds.

[1] Continued from p. 170.

Now there are very many foes which injure the leaves of our timber-trees, and I wish to show, as clearly as possible in a short article, how it comes about that injury to the leaves means injury to the timber. The sum total of the matter is that the substances which are to be sent down to the cambium, and converted through its agency into wood, are produced in the cells of the leaves: consequently, from our point of view, when an insect or a fungus consumes the substance of the leaves, it consumes timber in prospective. Similarly, when the leaves are removed from a tree by any agent whatever, the latter is robbed in advance of timber. A leaf, generally speaking, is an extended, flattened portion of a branch, covered by a continuation of the epidermis of the branch, and containing a continuation of its other tissues—the vascular bundles of the branch being continued as the venation, and the cellular cortex reappearing as the green soft tissue of the leaf. The epidermis of the leaf is so pierced at hundreds or thousands of nearly equi-distant points, that gases can enter into or escape from all its tissues: at these points are the so-called *stomata*, each stoma being a little apparatus which can open and close according to circumstances.

These openings lead into excavations or passages between the loose cells of the softer leaf-tissue, and if we supposed a very minute creeping organism to enter one of the stomata, it would find itself in a labyrinth of intercellular passages: supposing it able to traverse these, it could pass from any part of the leaf to any other between the cells; or it could emerge again from the leaf at thousands of places—other stomata. In traversing the whole of the labyrinth, however, it would pass over many millions of times its own length Moreover it would find these intercellular passages filled with a varying atmosphere of diffusing gases—oxygen, nitrogen, the vapour of water, and carbon-dioxide being the chief. It would also find the cell-walls which bound the passages damp, with water continuous with the water in the cells. If we suppose our hypothetical traveller threading the mazes of these passages at night, and able to perceive the changes which go on, it would find relatively little oxygen and relatively much carbon-dioxide in the damp atmosphere in the passages; whereas in the daylight, if the sun was shining brightly on the leaves, it would find the atmosphere rarer, and relatively little carbon-dioxide present, but an abundance of oxygen. These gases and vapour would be slowly moving in and out at the stomata by diffusion, the evaporation of the watery vapour especially being quicker on a dry, hot, sunny day.

Inside the cells between which these tortuous passages run, are contained structures which have much to do with these changes. Each of the cells I am considering contains a lining of protoplasm, in which a nucleus, and a number of small protoplasmic granules, coloured green, and called chlorophyll corpuscles, are embedded: all these are bathed in a watery cell-sap.

Now, putting together in a general manner some of the chief facts which we know about this apparatus, it may be said that the liquid sap inside the cells gives off water to replace that which escapes through the damp cell-walls, and evaporates into the above-named passages and out through the stomata, or at the surface. This evaporation of the water is in itself the cause of a flow of more water from behind, and this flow takes place from the vascular bundles forming the so-called venation of the leaf, coming directly from the wood of the stem. The course of this water, then, is from the soil, through the roots, up the young wood and into the venation of the leaf, and thence it is drawn into the cells we are considering. But this water is not pure water: it contains in solution small quantities of salts of lime, potash, magnesia, nitric, sulphuric, and phosphoric acids, as well as a little common salt, and traces of one or two other things. It is, in fact, of the nature of ordinary drinking-water, which always contains minute quantities of such salts: like drinking-water, it also contains gases (oxygen, nitrogen, carbon-dioxide) dissolved in it

It follows from what has been said that the cell-sap tends to accumulate small increasing quantities of these salts, &c., as the water passes away by evaporation. But we must remember that the living contents—the protoplasm, nucleus, and the green chlorophyll-corpuscles—use up many of these salts for their life-purposes, and other portions pass into the cell-walls.

It will thus be seen that the green chlorophyll-corpuscles are bathed by a fluid cell-sap, the dissolved gaseous and mineral contents of which are continually changing, even apart from the alterations which the life-processes of the living contents of the cell themselves entail. We may say that the chlorophyll-corpuscles find at their disposal in the cell-sap, with which they are more or less in direct contact, traces of salts, oxygen, carbon-dioxide, and of course water, consisting of hydrogen and oxygen.

Now we have the best possible reasons for knowing that some such changes as the following occur in these chlorophyll-corpuscles, provided they are exposed to sunlight: they take up carbon-dioxide and water, and traces of minerals, and by means of a molecular mechanism which is as yet unexplained in detail, they perform the astonishing feat—for it represents an astonishing transformation when regarded chemically and physically—of tearing asunder, by the aid of the light, the carbon, hydrogen, and oxygen of the carbon-dioxide and water, and rearranging these elements in part so as to form a much more complex body—starch, or an allied compound, oxygen being at the same time set free.

It is of course not part of my present task to trace these physiological processes in detail, or to bring forward the experimental evidence on which our knowledge of them is based. It must suffice to state that these compounds, starch and allied substances, do not remain in the chlorophyll-corpuscles, but become dissolved and carried away through certain channels in the vascular bundles of the venation, and thence pass to wherever they are to be employed as food. The chemical form in which these substances pass from one cell to another in solution is chiefly that of grape-sugar, and it is a comparatively easy observation to make that the cells so often referred to contain such sugar in their sap.

We are only concerned at present with the fate of a portion—but a very large portion—of this starch and sugar: we can trace them down the vascular bundles of the venation, through the leaf-stalk, into the cortex, and eventually to the cambium-cells; and it is necessary to be quite clear on the following points: (1) the cambium-cells, like all other living cells which contain no chlorophyll, need to be supplied with such foods as sugar, starch, &c., or they starve and perish; (2) since these foods are prepared, as we have seen, in the leaves, and in the leaves only, it is obvious that the vigour and well-being of the cambium depend on the functional activity of the leaves.

We have already seen how the cambium-cells give rise to the young wood, and thus it will be clear how the formation of timber is dependent on the functional activity of the leaves. Moreover, it ought to be mentioned, by the way at least, that it is not only the cambium which depends upon the leaves for its supplies—all the roots, young buds, flowers, and fruits, &c., as well as the cortex and cork-forming tissues, are competitors for the food supply. Now it is clear that if we starve the buds there will be fewer leaves developed in the following year, and so next year's cambium will again suffer, and so on.

I have by no means traced all the details of even the first ramifications of the complex network of correlations implied by this competition of the various organs and tissues for the food supplies from the leaves; but probably the following proposition will be generally clear:—If the leaves are stripped, the cambium suffers starvation to a greater or less extent, depending on the intensity of its competition with other tissues, &c.; of course a starved cambium will form less wood, and, it may be added, the timber will be poorer.

Again, even if the leaves are not stripped quickly from the tree, but the effect of some external agent is to shorten their period of activity; or to occupy space, on or in them, and so diminish the amount of leaf-surface exposed to the light and air; or to block up their stomata, the points of egress and ingress for gases and water; or to steal the contents of the cells—contents which should normally be passed on for the growth, &c., of other parts of the tree—in all or any of these ways injury to the timber may accrue from the action of the agent in question. Now there are numbers of parasitic fungi which do all these things, and when they obtain a hold on pure plantations or forests, they may do immense injury before their presence is detected by anyone not familiar with their appearance and life-histories.

The great difficulty to the practical forester who attempts to deal with these "leaf diseases" is at least twofold; for not only are the leaves so numerous and so out of reach that he can scarcely entertain the idea of doing anything directly to them, but (and this is by no means so clearly apprehended as it should be) they stay on the tree but a short time as a rule, and when they fall are a continual source of re-infection, because the spores of the fungi are developed on them. It is a curious fact that those fungi which are known to affect the leaves of forest-trees nearly all belong to two highly-developed groups—the Uredineæ and the Ascomycetes—and the remarkable biological adaptations which these parasites exhibit for attacking or entering the leaves, passing through periods of danger, and so on, are almost as various as they are numerous. Some of them, such as the *Erysipheæ* or mildews on beeches, oaks, birches, ashes, &c., only form small external patches on the leaves, and do little if any harm where the leaf-crown is large and active; others, such as many of the very numerous *Sphæriaceæ* and their allies, which form small dark-coloured flecks and spots on leaves, may also be looked upon as taking only a slight tax from the leaves. Even in these cases, however, when the diseases become epidemic in certain wet seasons, considerable damage may accrue, because two chief causes (and many minor ones) are co-operating to favour the fungus in the struggle for existence: in the first place, a continuously wet summer means loss of sunlight and diminished transpiration, &c., to the leaves, and so they form smaller quantities of food materials; and secondly, the damp in the atmosphere and leaves favours the fungi, and so they destroy and occupy larger areas of leaf surface.

It should be mentioned here, by the way, that all leaves of all trees are apt to have fungi on them in a wet summer, but many of these are only spreading their mycelia in all directions over the epidermis, in preparation, as it were, for the fall of the leaf: they are saprophytes which feed on the dead fallen leaves, but cannot enter into them while yet alive. In some cases, however, this preparation for the fall is strikingly suggestive of adaptation towards becoming parasites. I will quote one instance only in illustration of this. On the leaves of certain trees in Ceylon, there was always to be found in the rainy season the much-branched mycelium of a minute *Sphæria*: this formed enormous numbers of branches, which, on the older leaves, were found to stop short over the stomata, and to form eventually a four-celled spore-like body just blocking up each stoma on which it rested. So long as the leaf remained living on the tree, nothing further occurred; but wherever a part of the leaf died, or when the leaf fell moribund on the ground, these spore-like bodies at once began to send hyphæ into the dying tissue, and thus obtained an early place in the struggle for existence among the saprophytes which finished the destruction of the cells and tissues of the leaf.

There is another group of fungi, the *Capnodieæ*, which form sooty black patches on the leaves, and which are very apt to increase to a dangerous extent on leaves in damp shady situations: these have no connection with the well-known black patches of *Rhytisma* from which the leaves of our maples are rarely free. This last fungus is a true parasite, its mycelium penetrates into the leaf tissues, and forms large black patches, in and near which the cells of the leaf either live for the benefit of the fungus alone, or entirely succumb to its ravages: after the leaf has fallen, the fungus forms its spores. Nevertheless, although we have gone a step further in destructiveness, foresters deny that much harm is done to the trees—no doubt because the foliage of the maples is so very abundant. Willows, pines, and firs suffer from allied forms of fungi.

But it is among the group of the *Uredineæ* or rusts that we find the most extraordinary cases of parasitism, and since some of these exhibit the most highly developed and complex adaptations known to us, I propose to select one of them as the type of these so-called "leaf diseases." This form is *Coleosporium Senecionis* (*Peridermium Pini*), rendered classical by the researches of several excellent botanists.

It is true, *Coleosporium Senecionis* is not in some respects the most dangerous of these fungi—or, rather, it has not hitherto been found to be so—but in view of the acknowledged fact that foresters have not as yet been able to devise practical measures against the ravages of these numerous rust-fungi, and since we are as yet very ignorant of the details of the biology of most of them, it seems advisable to choose for illustration a form which shows in a distinct manner the complexities of the subject, so that those interested may see in what directions biologists may look for new results. That the story of this fungus is both complicated and of great biological interest will be sufficiently evident from the mere recital of what we know concerning it.

H. MARSHALL WARD.

(*To be continued.*)

MICHELL'S PROBLEM.

FOR the last two hundred years the attention of logicians and mathematicians has been directed to the inverse principles of the theory of probability, in which we reason from known events to possible causes. Two different methods of calculation are in use, which give approximately the same results. According to the celebrated theorem of James Bernoulli, "If a sufficiently large number of trials is made, the ratio of the favourable to the unfavourable events will not differ from the ratio of their respective probabilities beyond a certain limit in excess or defect, and the probability of keeping within these limits, however small, can be made as near certainty as we please by taking a sufficiently large number of trials." The inverse use of this theorem is much more important and much more liable to objection and difficulties than the direct use. In the words of De Morgan, "When an event has happened, and may have happened in two or three different ways, that way which is most likely to bring about the event, is most likely to have been the cause."

The second principle, due to Bayes, is thus given by De Morgan, "Knowing the probability of a compound event, and that of one of its components, we find the probability of the other by dividing the first by the second."

These principles have been accepted by the great majority of thinkers, and freely used by Laplace, Poisson, Herschel, and De Morgan. Stanley Jevons ("Principles of Science") gives a luminous account of the value of the theory, and accepts Michell's views: "If Michell be in error, it is in the methods of calculation, not in the general validity of his reasoning and conclusions."

On the other hand, Leibnitz, Kant, Forbes, Boole, and Mill ("Logic," xvii., xviii., xxv.), while allowing some value to the theory, doubt if it can be rigorously applied to obtain definite numerical results.

The interest and importance of the subject, and the length of time which has elapsed since any detailed discussion of it has been undertaken, furnish an excuse for the following suggestions, which are made in the hope that they may elicit more valuable arguments and opinions.

More than a century ago, Michell (Phil. Trans., 1767, p. 243) attempted to find the probability that there is some cause for the fact that the stars are not uniformly distributed over the heavens, but frequently form binary combinations or larger groups. Michell's results are quoted with approval by Laplace ("Théorie des Prob," p. 63), and by Herschel ("Astronomy," p. 607), though the latter mentions that Michell's data are too small, and immediately afterwards quotes Struve's solution of the same problem, which seems to be inconsistent with Michell's. I select Michell's problem for discussion, since it has been accepted by high authority and vigorously attacked, and for the sake of simplicity in the calculations shall confine my remarks to binary combinations.

Michell's statements are not very clear, and his arithmetical methods are cumbrous, but his argument may be condensed as follows: "What, it is probable, would have been the least apparent distance of any two or more stars anywhere in the whole heavens, upon the supposition that they had been scattered by mere chance?" Imagine any star situated on the surface of a sphere ($S = 4\pi r^2$) of radius r, and surrounded by a circle of radius a ($= r \sin \theta$, where θ is the angle subtended by a at the centre of the sphere), the area of this small circle is $s = \pi a^2 = \pi r^2 \sin^2\theta$. The probability that another star, "scattered by mere chance," should fall within this small circle is $\frac{s}{S}$, and that it should not fall within it $\frac{S - s}{S}$. But there is the same chance for any one star as for any other to fall within the circle, hence we must multiply this fraction into itself as many times as the whole number of stars (n) of equal brightness to those in question. "And farther, because the same event is equally likely to happen to any one star as to any other, and therefore any one of the whole number of stars (n) might as well have been taken for the given star as any other, we must repeat the last found chance n times, and consequently $\left(1 - \frac{s}{S}\right)^{n^2}$ will represent the probability that nowhere in the whole heavens any two stars among those in question would be within the given distance (a) from one another, and the complement of this quantity to unity will represent the probability of the contrary."

In the case of the two stars, β Capricorni, Michell takes $n = 230$, $\theta = 3' \, 20''$. Hence

$$p = \frac{s}{S} = \frac{(\sin 3' \, 20'')^2}{4} = 1/4254519,$$

which Michell takes as 1/4254603; and

$$Q = \left(1 - 1/4254603\right)^{n^2} = 1 - \frac{52900}{4254603} = 1 - 1/80\cdot4;$$

or, according to Michell, the probability is 80/81 that no two stars equal in size to β Capricorni shall fall so near to one another as they do.

Prof. J. D. Forbes (*Phil. Mag.*, December 1850) objects to the entire principle upon which Michell's work is based, and has pointed out some errors in detail. Todhunter ("Theory of Prob.," p. 334) and Boole ("Laws of Thought," p. 365) countenance these objections; but before discussing them it will be well to mention other attempts to solve the same problem.

Struve ("Cat. Nov.," p. 37) has used an entirely different method. The possible number of binary combinations of n stars is $\frac{n(n-1)}{1 \cdot 2}$; and the chance that such a pair should fall on a small circle of area s is s/S, where S is the surface of the portion of the sphere in which n has been counted. Hence the chance that any pair of stars should fall within the circle is $n(n-1)s/2S$.

Taking S as the surface from $-15°$ of declination to the North Pole, $n = 10229$, and $\theta = 4''$, Struve finds $p = 0\cdot007814$.

Herschel ("Ast.," p. 607), either in error or by a re-calculation from different data, quotes Struve as finding that the probability is 1/9570 against two stars of the 7th magnitude coming within 4″ by accident.

Applying Struve's formula to Michell's data for β Capricorni, we have

$$1 - \frac{230 \times 229}{2} \times \frac{1}{4254603} = 1 - 1/161\cdot5,$$

or 161/162, as the probability that no two such stars fall within the given area.

Forbes, with the aid of a mathematical friend, offers the following solution:—Suppose the n stars are represented by dice, each with $v(>n)$ sides, where v represents the number of small circles in the spherical surface, or S/s. The chance of two stars falling into one circle is the same as that two dice show the same face.

The total number of arrangements without duplication is—

$$v \,.\, \overline{v-1} \,.\, \overline{v-2} \,.\,.\,.\,.\, \overline{v-n+1},$$

and the total number of falls is v^n; hence the probability of a fall without duplication is—

$$v \,.\, \overline{v-1} \,.\, \overline{v-2} \,.\,.\,.\,.\, \overline{v-n+1}/v^n;$$

and the chance that two or more dice show the same face is—

$$1 - \lfloor v / \lfloor v - n \,.\, v^n.$$

In the case of β Capricorni $v = 4254603$, and $n = 230$. Evaluating by Stirling's theorem, Forbes gives $p = 0\cdot00617 = 1/160$ nearly, which does not differ much from $n^2/2v$.

A recalculation has given me $p = 1/162$. The result then agrees with that of Struve and differs from that of Michell.

The following suggestions are due in substance chiefly to Boole and Forbes, but their language has been freely altered, and misapprehension of their meaning may therefore be feared.

In all such cases an hypothesis ("the random distribution of stars") is assumed, and the probability of an observed consequence ("the appearance of a double star") calculated. The small probability of this result of the assumed hypothesis is held to imply that the probability of the hypothesis is equally small, and therefore the probability of the contrary hypothesis is very large.

According to Boole, "the general problem, in whatever form it may be presented, admits only of an *indefinite* solution," since in every solution it is tacitly assumed that the *a priori* probability of the hypothesis has a definite value, generally 0 or 1, and also a definite probability is assigned to the occurrence of the event observed if the assumed hypothesis were false.

In Michell's problem it is assumed that the stars are either scattered at random or obey a general law: no notice is taken of the possible case that a general law holds for stars within a certain distance from our system, beyond which an entirely different law may obtain. Again, the subjection of each system to a separate intelligence is tacitly ignored.

The probability of an event is the value of the expectation of its occurrence existing in the mind of the thinker: "We must again warn the reader that probabilities are in his mind, not in the urn from which he draws" (De Morgan, "Enc. Met.," 414); but in the solution of these problems this subjective value is converted with startling ease into a much more objective and concrete expression. As Forbes puts it, "The doubt existing whether an event still future, which may happen in many different ways, shall occur in one particular way is not equivalent to an inherent improbability of its happening, or having happened, in that way"

We do not assume that a friend is speaking untruly when he tells us that, out of 10001 seats, the number of his ticket is 453, yet the antecedent probability is 1/10000 against the truth of his statement. The chances are greatly against ten stars out of 230 appearing as binary combinations; but, according to one view of the meaning of "random distribution," that arrangement is no more unlikely than any other, and we should be no more surprised to hear that one rather than another is the actual one. Forbes objects that "to assume that 'every star is as likely to be in one position as another,' is not the expression of the idea of random or lawless distribution." The expression seems to me to be true, but its interpretation into mathematical symbols has been far too closely restricted both by Michell and Forbes.

"Michell assumes that, with random distribution, the chance of finding a star in a space is proportional to the space, or that a perfectly uniform distribution would be that alone which would afford no evidence of causation."

Suppose the whole surface of the sphere cut up into minute equilateral triangles, and a star placed at each collection of angular points. Each star is the middle point of a regular hexagon, and at a distance, a, from six other stars. If we imagine the six stars to be fixed, and the central star shot out from the centre of the sphere so as to fall within the hexagon, that it may not fall within a distance, r, of any other star it must fall in a regular hexagon, the side of which is $(a - r)$ situated symmetrically within the larger hexagon. The probability of the star falling within this smaller hexagon is expressed by $\frac{(a-r)^2}{a^2}$, which becomes less and less the more nearly r equals a; that is, the more nearly the distribution is truly uniform. When $r = a$, the expression becomes 0, or the probability of exactly uniform distribution is *nil*, and apparently uniform distribution is due solely to the imperfections of our instruments. Michell, however, seems to assume this probability to be 1, or certainty. Struve's method is open to the grave objection that he assumes that the total possible number of binary combinations really occur. Applying his formula to calculate a value for n which makes the chance a certainty, we find that, if 2917 stars are scattered over the sphere, it is a certainty that each will be withinf 3′ 20″ of another! Of the three methods, that of Forbes seems to be the least open to objection.

Besides these fundamental difficulties in principle, there are several very doubtful points in the calculation which may be worthy of a brief notice.

Michell considered the whole surface of the sphere, though in his time the examination of the southern hemisphere was hardly complete enough to furnish the requisite data. The stars do not lie on the surface of a sphere, but scattered through infinite space, so that two stars, the angular distance between which is apparently small, may in reality be very far apart. Suppose that the nearer star lies on the surface of our imaginary sphere, the probability that the direction of the other star is within 15° of the surface is only about one-fourth. Hence the number of apparently double stars must be reduced to a considerable but unknown extent.

Forbes throws considerable doubt on the correctness of raising a second time to the power n. Struve's multiplication by n's seems to prove very curious conclusions. Mr. Venn's reasons for dissenting from Michell's solution will be found well worthy of perusal ("Logic of Chance," p. 260).

SYDNEY LUPTON.

VEGETABLE RENNET.

THE idea that the protoplasm or living substance of both animals and plants is essentially similar, if not quite identical, has long been accepted by both physiologists and botanists. This similarity is most easily seen in the very lowest members of both kingdoms; in fact, for a very long time doubt existed in the case of many organisms—*e.g.* Volvox—as to which kingdom they should properly be included in. Even now it is hardly possible to formulate a definition of "plant" or "animal" which shall put all into their proper positions. When we go higher up the scale in both the animal and the vegetable world, this difficulty of course disappears, on account of the differences of organization and development. It is not difficult even here to trace a remarkable similarity of properties in the living substance, which leads to the conception that not only is protoplasm practically the same in animal and vegetable, but that its activities in the two cases—that is, the metabolic processes which accompany, and are in a way the expression of, its life—are fundamentally the same. In both kingdoms we have as the sign of its life the continual building up of the living substance at the expense of the materials brought to it as food, and the constant breaking down of its substance with the consequent appearance of different organic bodies, which are strictly comparable in the two cases. The vegetable protoplasm produces starch, the animal glycogen—both carbohydrate bodies of similar composition and behaviour. In both organisms we meet with sugars of precisely similar character. The proteid bodies long known to exist in animals, and classed into albumins, globulins, albumoses, peptones, &c., have been found to be represented in vegetables by members of the same groups, differing but in minor points from themselves. We have fats of complex nature in the animal represented by oils of equal complexity in the vegetable, their fundamental composition being identical; even the curious body lecithin, so long known as a constituent of nervous tissue in the animal, having been procured from the simple yeast plant.

Further, the changes which give rise to these bodies, or which bring about various transformations of them, have been in very many cases demonstrated to be due to similar agencies at work in both the animal and vegetable organism In many cases, no doubt, they are produced by the actual splitting up of the protoplasm itself; but apart from this we have their formation in large quantities by the agency of bodies which are known as unorganized ferments, and which are secreted by the protoplasm for the purpose of such formation. Perhaps no line of research in vegetable physiology in recent years has been so productive of good results as the investigations that have been made into the occurrence of such bodies, and the comparison of them with those that are met with in the animal organism. Diastase in vegetables, and the ferments of saliva and of pancreatic juice in animals, possess the same power of converting starch into sugar. The peptic and tryptic ferments of the stomach and pancreas respectively have been shown to have representatives in the vegetable kingdom, and these not only in such cases as the carnivorous plants, but to be actually made use of in such truly vegetable metabolism as the processes involved in the germination of the seed. The conversion of albumins and other indiffusible proteids into a further stage than that of diffusible peptone—

that of leucin in the animal, and asparagin in the vegetable—has been shown to be the work of such a ferment in the two cases. These ferments, too, are interchangeable to a certain extent, for those of the alimentary canal are capable of digesting the proteids of vegetable bodies, while those of the latter can similarly split up the animal albumins, fibrin, and other forms of proteid.

The essential similarity of the metabolism is also indicated by the appearance in the two cases of complex bodies of somewhat similar constitution which are quite comparable with each other. In the vegetable kingdom these bodies are known as alkaloids; in the animal they have for the past ten years or more been known as ptomaïnes. They are among the products of the destructive decomposition of proteids. Thus *cadaverin*, a body found in putrefying animal matter, is apparently to be looked upon as belonging to the same group of bodies as *muscarin*, the poisonous principle found in several species of mushroom.

Perhaps the latest development of the same idea has been the discovery of ferments in the vegetable kingdom which are comparable in their action with the rennet which is obtainable from the stomach of many young animals, particularly the calf. In an extract of such a stomach taken while secretion of gastric juice is proceeding, or in the gastric juice itself, is a principle which has the power of curdling milk—a property taken advantage of by the farmer in the process of manufacturing cheese. The *casein*, which is the proteid concerned in cheese-making, is, under appropriate conditions, converted by this body into an insoluble form, which, for want of a better name, may be called briefly cheese. The conversion is not to be confused with the loose curdling which takes place when milk becomes sour from putrefactive changes or from the addition of an acid, for it is a true coagulation, resembling the clotting of blood. Now, recent investigations show us that in many plants a similar ferment exists, which possesses an identical power, producing, when added to milk, a clot which is quite indistinguishable from that which is formed under the action of animal rennet. The list of such plants is continually increasing, but they do not appear to be grouped at all on the lines of the recognized natural orders. Ranunculaceæ, Solanaceæ, Cucurbitaceæ, Compositæ, Galiaceæ, and others, furnish us with conspicuous examples.

At a meeting of the Society of Natural Science of Stockholm, held about four years ago, the Secretary brought before the notice of the meeting the fact that the common butterwort (*Pinguicula vulgaris*) possessed the very curious property of causing a clotting of milk when the vessels in which the milk was contained had been first rubbed over with the plant. No explanation was offered of the phenomenon, but a suggestion was made that the power might be due to the presence of micro-organisms. Judging from analogy with other plants since discovered to possess the same property, it is far more likely to be due to a specific unorganized ferment. The occurrence of this in Pinguicula is very significant, as bearing on the similarity of the metabolism in animals and vegetables, for Pinguicula is one of the carnivorous plants, digesting, by the aid of its secretions, flies which it captures in its leaves. We have so associated in the same plant a proteolytic and a rennet ferment, a condition which at once recalls the gastric juice of animals, in which both these bodies are present.

One of the most interesting of the plants which contain this ferment, or vegetable rennet, is the so-called "Naras" of the West Coast of Africa (*Acanthosicyos horrida*), a species of Cucurbitaceæ. The plant was described in detail by Welwitsch, in 1869, when its peculiar physiological property was unknown. A more detailed description, given by Marloth, has recently appeared, which deals, among other points, with this power. The plant is to be met with in dry, sandy, and desert places in Namaqua Land, Whale Bay, and the Mozambique district. It is very singular in its habit and appearance, consisting of long, spiny, weak-looking branches running almost on the surface of the sand, and being at intervals buried therein and again emerging. The stem is very short, so that the plant looks like a system of creeping spiny branches, some of which measure 20 feet or more in length. The root system is similarly developed, long creeping roots penetrating, in some cases, for a distance of 100 feet through the sand. The long spiny branches seem destitute of leaves, for these are quickly deciduous and sometimes abortive, and while they remain upon the shoots they are closely adpressed to them, and are stiff and horny in texture. At the base of each leaf are two strong spines, which persist after the leaf has fallen. The flowers are borne in the axils of the leaves, between the spines. The male and female flowers are found on separate plants; the former are sessile, the latter shortly stalked. The ripe fruit is of considerable size, much like an orange in appearance. It has a very powerful and pleasant aroma, and its pulp is very juicy and agreeable to the taste. In the unripe condition it is bitter and uneatable. According to Marloth, the natives eat it to a very great excess, both fresh and in the form of "Naras cake," a preparation of it made by drying the expressed pulp and juice in the sun. The power to appreciate its excellence seems to be confined to the natives of the part, for strangers partaking of it for the first time are said to pass through strange and painful experiences after their banquet.

Its power of causing the clotting of milk is well known among the natives of the part, who use it freely for that purpose. The ferment is contained in considerable quantity in the juice, the pulp, and the rind of the fruit. It is absent from the branches, from the seeds, and from all parts of the unripe fruit. It is soluble, according to Marloth, in alcohol of 60 per cent. strength, an extract of the pulp made with that fluid retaining the power to coagulate the milk. It is not identical with the principle which gives the fragrance to the ripe fruit, nor to that which gives the bitter taste to it when still young. The ferment is destroyed by boiling, but will remain for an almost indefinite time in the dried rind. Marloth, in his experiments, found that an extract of pulp dried to a friable condition in the sun was quite active in causing coagulation. The writer had the opportunity recently of examining some dried rind and some old seeds.[1] An extract of these materials, made with 5 per cent. solution of common salt, showed the ferment in abundance in the rind, but absent from both the testa and the interior of the seeds.

Another plant, occurring nearer home, has the same property. This is the common yellow Galium (*G. verum*). In his "Popular Names of British Plants," Prior speaks of its peculiarity as being known in the sixteenth century, when Matthioli wrote of it, "Galium inde nomen sortitum est suum quod lac coagulet." In the West of England, particularly Somersetshire and Herefordshire, it is still the custom of dairymen to put this plant into the milk they have devoted to cheese production, to "set" it. The plant has a long straggling stem, bearing at short intervals whorls of small leaves, in the axils of which are numerous panicles of yellow flowers. The practice is to put the whole plant, or as much of it as is above ground, into the milk, but the active principle seems to be located in the flowers. The white Galium (*G. Aparine*) is said to be devoid of the property.

The common traveller's joy (*Clematis Vitalba*) is another instance of the occurrence of this ferment. It is peculiar in one respect, the property appearing to be

[1] This material was kindly furnished by Mr. W. Thiselton Dyer, F.R.S., Director of the Royal Gardens, Kew.

situated in the tissue of the stem, probably the soft bast. In most other cases it seems to be attached somehow to the reproductive parts of the plant. The quantity that can be extracted from Clematis is, however, much less than from the other plants spoken of.

The ferment has also been found in the petals of the artichoke (*Cynara Scolymus*).

An account of the occurrence of this vegetable rennet would not be complete without its including the researches of Dr. Sheridan Lea on *Withania coagulans* (Proceedings of the Royal Society, 1883). These have, besides their scientific value, a direct bearing upon the commercial aspect of the question. Many of the natives of India refuse to have anything to do with cheese prepared by means of animal rennet, and there is consequently there a large field for the employment of the plant. Some years ago Surgeon-Major Aitchison sent home an account of the peculiar property of the Withania. The shrub grows freely in Afghanistan and Northern India, and the natives there have for a long time employed an aqueous extract of the capsules to curdle their milk. Some dried material sent from thence to Kew was used by Dr. Lea in his investigations. Withania is a genus of the order Solanaceæ, and has a capsular fruit, containing a large number of small seeds. In the dried material these seeds were enveloped in a coating of a peculiar resinous matter, which was probably the dried juice of the capsules in which they had ripened. The ferment was found to exist to a very slight amount in the stalks of the fruits, and to be extremely abundant in the seeds. From the ground seeds it could be extracted easily by maceration with solution of common salt and by treatment with glycerine. So extracted, it was found to be destroyed on boiling, but to be able to withstand moderately prolonged exposure to alcohol. Its activity in a fairly strong extract was quite equal to that of most commercial samples of rennet prepared from the stomach. It could, moreover, be kept with as great security as the latter by the aid of common salt and a little alcohol. Its commercial value is somewhat interfered with by the presence in the seeds, and in their extracts, of a peculiar yellowish-brown colouring-matter, which cannot be separated without destroying the rennet.

Since the publication of Dr. Lea's researches the writer has met with the ferment in the unripe seeds of *Datura Stramonium*, a plant belonging to the same order, Solanaceæ. In this plant, though present in the unripe seeds, it appears to be absent from them when ripe. Its exact distribution is, however, not yet determined.

The occurrence of this property in so many plants, and these not at all closely connected in other ways, leads to the consideration of what must be its physiological significance. It is perhaps not difficult to see why rennet should occur in the stomachs of young animals whose food consists chiefly of milk, but its importance in the vegetable kingdom must be independent of such a function. Further researches, still in progress, may perhaps throw some light upon this point. It is significant so far to notice that its occurrence is mainly in those parts which are especially connected with the reproduction of the plant, a fact which seems to point to a possible function in connection with the storage of proteid food materials for the nutrition of the embryo during germination.

J. R. GREEN.

THE METEORIC SEASON.

WE have now arrived at a period of the year which is full of interest to meteoric observers. The number of meteors visible has greatly increased, as compared with preceding months, and apart from this, observations may be pursued without the discomfort and inconvenience so often experienced on the cold starlight nights of autumn and winter. The impending return of two rich showers is an additional incentive to those who may contemplate giving a little time to this interesting branch of astronomy.

From observations at Bristol on the nights of July 8, 11, and 12 last, it appears certain that the *Perseids* (which attain a maximum on August 10, when the radiant is at $45° + 57°$) had already commenced. On July 8 twenty-five meteors were counted between 11h. and 13h. 30m., and these included six paths which denoted a well-defined radiant at the point $3° + 49°$, a little south of Cassiopeia's Chair. The visible traits of the individual meteors traced from this radiant were identical with those exhibited by the *Perseids* which are displayed in August, and the fact that this radiant seen on July 8 is far west of the radiant usually remarked on August 10, does not negative the presumed identity of the two showers. The *Perseid* radiant which endures a considerable time, changes its position amongst the stars from night to night, and the extent and direction of this displacement will be seen by a reference to NATURE, vol. xxxvi. p. 407, where I have described a number of observations secured at this station in July and August of last year.

When the moon leaves the evening sky towards the close of the present month, observers should watch for the reappearance of the *Aquarids* which are usually seen in marked abundance about July 27, 28, and 29. The radiant is near δ Aquarii, and the meteors are rather slow, usually ascending from low in the south-east, and the brighter ones throw off trains of sparks. Early *Perseids* are also numerous at the end of July, and the radiant is then closely south of the well-known star cluster χ Persei. Observers should register the paths of the meteors and determine the precise place of the radiant on each night of observation.

Bristol, July 13. W. F. DENNING.

NOTES.

THE proposal that a Professorship for the exposition of the Darwinian theory should be established in connection with the Sorbonne has received the sanction of the Sorbonne authorities. Three members of the Committee by which the matter was decided were opposed to the scheme, but they did not vote against it. They simply refrained from voting. The Sorbonne has asked that the name of the proposed chair shall be changed. One or other of the three words, "evolution," "morphology," "phylogeny," is to be substituted for "philosophy."

THE Birmingham meeting of the Photographic Convention of the United Kingdom will be held from the 23rd to the 28th of July. A programme of excursions and local arrangements has been issued. The Convention will be opened on the evening of the 23rd inst., by the Mayor of Birmingham, at a *conversazione* to be held in the Masonic Hall in connection with an exhibition of photographs and photo-apparatus.

ON Thursday, the 12th inst., the anniversary meeting of the Sanitary Institution of Great Britain was held in the theatre of the Royal Institution. The Chairman, Mr. Edwin Chadwick, in opening the proceedings, claimed credit for the Sanitary Institution of Great Britain and like institutions for a large proportion of the reduced death-rate of the metropolis, which was now 14 in 1000. London in that respect compared very favourably with other places, the death-rate in Paris being 27, Vienna 30, and St. Petersburg 40. The medals and certificates awarded to the exhibitors at the Sanitary Exhibition held at Bolton in 1887 having been distributed by Mr. Chadwick, Dr. B. W. Richardson delivered an address on "The Storage of Life as a Sanitary Study." He began by referring to instances of long life in lower animals and in man. These, he said, by some peculiar process as yet but little investigated, held life as a long possession, and to this faculty he applied the term "The Storage of Life." The problem which the lecturer placed before the society was stated as follows:—Certain proofs of the power of the human body to lay or store up life to a prolonged period are admitted. What are the conditions which favour such storage, and how can we promote the conditions which lead to it? He stated the conditions in the following order, hereditary qualification, the virtue of continence, maintenance of balance of bodily functions, perfect temperance, and purity from implanted or acquired diseases. In estimating the value of temperament as connected with life storage, he maintained that the bilious and sanguine temperaments are best for long life, the nervous and lymphatic the worst. In dealing with what he called all-round temperance, he showed that whatever quickened the action of the heart beyond its natural speed and force was a stimulant, and in proportion to the unnatural tax inflicted by stimulation there was a reduction in the storage of life. Dr. Richardson spoke also of the prevention of the damaging diseases, where the art of the sanitarian comes into most effective play. A vote of thanks was accorded to the lecturer.

THE annual meeting of the Liverpool Astronomical Society was held at Liverpool on the 9th inst., when the report of the Council for the past year was read. It appears that since the last annual meeting more than 200 new members have been elected, and that the work carried on by the Society has increased in a commensurate degree. The balance sheet shows a small sum in the hands of the treasurer, so that the financial condition is satisfactory, though there has been a large outlay for printing the Journal. Mr. T. G. E. Elger succeeds Mr. Denning as President, and Mr. Rowlands is appointed Secretary in the place of Mr. W. H. Davies, who has resigned. In commenting upon the withdrawal of Mr. Davies, the Council refer to the earnestness, zeal, and ability displayed by him in performing the arduous duties of his office during a long period, and attribute the rapid development of the Society to his untiring efforts on its behalf.

THE thirty-fifth General Meeting of the German Geological Society will be held at Halle from August 13 to 15.

WE regret to announce the death of M. Jean-Charles Houzeau de Lehaie, Honorary Director of the Royal Observatory of Brussels, member of the Belgian Royal Academy of Sciences. He died at Schaerbeck on the 12th inst. M. Houzeau was in his sixty-eighth year.

THE death is announced of Dr. Johann Odstreil, an eminent mathematician and physicist of Vienna.

A LOWER THAMES VALLEY BRANCH of the Selborne Society has been formed. Its operations will extend on both sides of the river from Hampton to Putney inclusive. The inaugural meeting was held on Monday in the coffee-room of the Star and Garter Hotel, Richmond, the Duke of Cambridge in the chair. The objects of the Selborne Society are to secure the preservation from unnecessary destruction of such wild birds, animals, and plants as are harmless, beautiful, or rare; to discourage the wearing and use for ornaments of birds and their plumages, except when the birds are killed for food or reared for plumage; to protect places and objects of interest or natural beauty from ill-treatment or destruction; and to promote the study of natural history. It is proposed that the new branch of the Society shall devote a part of its funds to the purchase of works on natural history for the free libraries of Richmond, particularly such as throw light upon the natural history of the Thames Valley, and encourage a love of nature in the young.

THE first Annual Report of the National Association for the Promotion of Technical Education has now been issued. It contains a full account of the objects and work of the Association. Its main work up to the present may, according to the Report, be roughly divided as follows:—(1) the publication of leaflets, pamphlets, addresses, and other papers, and the circulation of this literature throughout the country; (2) the holding of public meetings and conferences, and the delivery of lectures and addresses on subjects connected with the work of the Association; (3) Parliamentary work; (4) formation of an agricultural section; (5) commercial education; (6) the organization of branches and local committees to co-operate with the Central Association. Besides the work falling under these heads, the Association has been the means of supplying much information to inquirers on various subjects connected with technical education, and has promoted the movement in other ways. The committee are strongly of opinion that there is a wide field for the future operations of the Association. They urge that branches should be started in all large towns which are now without them, and that every opportunity should be taken by conferences, and in various other ways, to spread sound information on the question of technical education, on which, as the Report truly says, in spite of the great increase in public interest, much lamentable ignorance still remains.

THE Indian Government has adopted an important resolution on the subject of State education. It recommends that wherever possible Government schools should be substituted for private ones, and that the education staff should be strengthened by the engagement of specialists in Great Britain. The resolution deals largely with the question of technical education, and urges that as a beginning an industrial survey should be made of each province.

HAVING been charged with the supervision of a new and complete edition of the "Works of Galileo," to be shortly undertaken at the expense of the Government and under the patronage of the King of Italy, Prof. Antonio Favaro, of the Royal University, Padua, earnestly begs all librarians, curators or trustees of museums, collectors of old manuscripts and autographs, and all those engaged in researches touching the history of science, to give him any information in their power respecting any Galileian documents, which may assist him in carrying out this difficult undertaking.

AT the meeting of the Scientific Committee of the Royal Horticultural Society on July 10, the plague of caterpillars, &c., was one of the subjects discussed. Mr. O'Brien alluded to the abundance of earwigs (Forficula) this season. Mr. Wilson drew attention to the local distribution of the caterpillars. In one garden in his neighbourhood none of the pests were found, while in others there was scarcely a leaf left on the trees. At Wisley Mr. Wilson had found that exposure to east wind was associated with the presence of the insects. Thus the trees in one line of plums, fully exposed, were stripped of their foliage, while in another line of the same variety close by, on the same description of soil, but where the trees were sheltered by a furze fence, not a leaf was injured.

THE Kew Bulletin for July opens with a paper containing much information on Bhabur grass, which closely approaches esparto in habit and in the possession of the technical qualities necessary for paper manufacture. In another paper there is an interesting extract from a letter by Mr. William Fawcett, giving his first impression of the vegetable resources of the Cayman Islands, which are situated in the Caribbean Sea, about 200 miles to the west of Jamaica. In association with the Governor of Jamaica Mr. Fawcett lately visited these lonely and little-known islands for the purpose of investigating a disease which has existed for some time among the cocoa-nut palms at Grand Cayman. Valonia in Cyprus and prickly pear in South Africa form the subjects of two other sections; and the number closes with an account of the true star anise of China, prepared by Sir J. D. Hooker for the current issue of the Botanical Magazine.

THE Annual Report of the Royal Botanic Gardens, Trinidad, for 1887, by Mr. J. H. Hart, Superintendent, has been issued. In an interesting historical sketch Mr. Hart notes that the Trinidad garden has been in existence seventy years, and is the oldest botanical garden which has been continuously maintained in working order within the circuit of the British West Indies. Mr. Hart was appointed Superintendent in 1886, and assumed charge in March, 1887, after eleven years' service in Jamaica. One of his first objects was to make provision for the proper arrangement and storage of herbarium specimens, and he is able to report, thanks chiefly to the interest in the matter taken by the Governor (Sir W. Robinson), that the herbarium is already established on a sound basis. He hopes that when the material is all arranged it will be among the first of West Indian herbariums, if not the very first. "To show the value of such work," says Mr. Hart, "and especially the value attached to the Trinidad flora, I may state that I have already received four applications for sets of the Trinidad plants; as these will bring exchanges from a like number of countries possessing a flora of great value to us for the comparison and identification of our own, these offers will be taken up as early as possible. Prof. W. Thiselton Dyer, Director of Kew, in a letter recently received, says: 'In Trinidad itself there must be an enormous amount of work still to be done.' Trinidad stands unique among the other islands by the possession of a flora which combines the West Indian with the South American, and has besides many plants which are only known to occur within its own boundaries, or, in other words, are pecular to the island itself."

ACCORDING to intelligence received at New York on July 14, Honduras had been visited by severe storms and earthquake shocks, which had caused great damage to property, but no loss of life.

THE fourth yearly report of the Berlin branch of the German Meteorological Society for the year 1887, shows that the number of members has increased from thirty-seven in January, 1884, the time of its foundation, to 117. The President for the year 1888 is Dr. Vettin. The proceedings of the monthly meetings have been reported in our Notices of Societies, &c. The present report contains an account of the special rainfall investigations at twenty stations in and near Berlin, and comparisons of the different rain gauges employed.

THE British Consul at Bussorah on the Persian Gulf in his last report states that a remarkable improvement has taken place in the climate of the country round Bussorah with the substitution of date and wheat cultivation for that of rice. The malarious fever, to which Bussorah gave its name, is now comparatively rare; and sallow complexions and worn looks, which some years ago were universal, are now no longer seen. The north-west wind, which prevails in the hot weather, instead of being moist and clammy, as it used to be, is dry and hot. The month of September, when the marsh which is formed yearly by the overflow of the Euphrates is drying up, is still the least healthy season. December and January are cold, July and August intensely hot. The rest of the year is very much like the spring and summer of Southern Europe.

THE administration report of the Meteorological Reporter for the North-West Provinces and Oudh for the year 1887–88, states that there are now nineteen first-class observatories and 275 rainfall stations reporting regularly to the central office. Records of rainfall and temperature are kept at numerous dispensaries all over the Jeypore territory. At the majority of the stations the old float gauge is still used, but gauges of this kind are gradually being replaced by Symons's 5-inch gauge, with improved results. Mr. Hill has under discussion a valuable series of temperature and humidity observations made at various heights above the ground. Amongst the interesting results published we may specially mention the sunshine observations at Allahabad. No less than 89 per cent. of the possible amount was recorded in November, 1887; the lowest percentage was 34·5 in August, and the mean for the year was 67·9 per cent.

THE composition of persulphide of hydrogen has at last been satisfactorily determined by Dr. Rebs of Jena. The history of this substance has been a most remarkable one; it has by turns been awarded almost every conceivable formula from H_2S_2 to H_2S_{10}. The results of Dr. Rebs' researches, however, go to show that it possesses the formula H_2S_5, first assigned to it many years ago by Berthollet, and that it is a true pentasulphide of hydrogen. It was prepared pure by the following method: A solution of soda in alcohol was saturated with sulphuretted hydrogen gas, and an equal bulk of alcoholic soda afterwards added to the sodium sulphydrate thus formed. After agitation the fluid solidified to a white crystalline mass of sodium sulphide, to which flowers of sulphur were added in the proportion necessary to form the required polysulphide of soda. The di- and tri-sulphides prepared in this manner crystallized out, but the tetra- and penta-sulphides remained in solution. They were then freed from alcohol in a current of hydrogen, and the residue dissolved in water out of which all the air had been expelled. In order to obtain persulphide of hydrogen, the solutions were poured into cylinders containing concentrated hydrochloric acid kept cool by ice. Sulphuretted hydrogen gas was immediately evolved, and an emulsion formed, which on standing became clear, and small oily drops of persulphide of hydrogen settled out and united to form an oil. After decantation of the supernatant liquor and washing with ice-cold water, the oil was eventually dried and analysed. The analyses show most conclusively that all the four polysulphides of soda, when their aqueous solutions are poured into hydrochloric acid, yield one and the same polysulphide of hydrogen, viz. the pentasulphide H_2S_5. To complete the proof the four polysulphides of potassium were similarly treated, with like result; more interesting still, Dr. Rebs shows that the sulphides of barium behave in a precisely analogous manner, forming nothing but H_2S_5. When the pentasulphides are employed there is a simple exchange of metal for hydrogen, but with the lower persulphides a decomposition of the corresponding sulphide of hydrogen first formed occurs into pentasulphide and sulphuretted hydrogen. Pentasulphide of hydrogen is a bright yellow, mobile, transparent oil, possessing an odour peculiar to itself. When dry it may be preserved in a closed tube without decomposition, but in contact with water it breaks up rapidly, with evolution of sulphuretted hydrogen and separation of sulphur.

THE tobacco-plants in the Russian Government of Bessarabia have of late years suffered greatly from disease, which has almost threatened ruin to the industry of tobacco growing. Prof. Lindemann, having been asked as a specialist to study the subject, has found three kinds of disease, the most important of which by far is a kind of consumption to which the plant is subject, caused chiefly by larvæ of the beetle *Opatrum intermedium*, Fisch. This grub attacks the underground part of the stem and the leaves. The female lays her eggs from the middle of April to that of May, and in loose ground not yet covered by the plants. The larva lives two and a half months, and the pupa stage is fourteen days. The insect does not breed till the following spring. The larva feeds at first mostly on wild plants, *Atriplex* and *Convolvulus*, but never on *Leguminosæ*. It attacks Gramineæ (maize, wheat, &c.), but only the embryo of the grain, and when germination has begun the grain is avoided. Though the time of possible attack is thus short, maize culture in Bessarabia has suffered much in this way. To protect the tobacco, Prof. Lindemann recommends sowing the fields in the end of March with mustard or rape, so that the insect at the time of egg-laying may be hindered by a thick cover of vegetation. Another insect (*Pedinus femoralis*, F.) acts just like Opatrum, but does more harm to maize than to tobacco. Prof. Lindemann further describes two minor diseases affecting the leaves, and making the tobacco unsalable. One (*thrips*) is also caused by an insect; the other (*mosaic disease*) seems to be due to some condition of the ground.

THE Indian Museum has begun to issue what promises to be a most useful series of "Notes on Economic Entomology." Two numbers, by Mr. E. C. Cotes, first assistant to the Superintendent of the Indian Museum, have been published—the first presenting a preliminary account of the wheat and rice weevil in India; the second dealing with the experimental introduction of insecticides into India, and including a short description of modern insecticides and methods of applying them.

THE new number of the "Internationales Archiv für Ethnographie" (Band I., Heft III.) contains, besides various collections of short notes, the conclusion of Herr J. Büttikofer's excellent paper on the natives of Liberia, and an account, by Herr A. Woldt, of objects of interest brought by Captain Jacobsen from certain districts of the Amoor in 1884-85, and now preserved among the treasures of the Berlin Ethnographical Museum. These objects are valuable on account of the light they throw on customs connected with Shamanism.

AN instructive paper on the osteology of Porzana Carolina (the Carolina Rail), by Dr. R. W. Schufeldt, has been republished from the *Journal of Comparative Medicine and Surgery*. As defined by the American Ornithologists' Union, the order Paludicolæ, containing the Cranes, Rails, &c., is primarily divided into two sub-orders, the (1) Grues or the true Cranes, and (2) the Ralli, containing the Rails, Coots, and Gallinules, &c. The family *Rallidæ* occur in this latter group, wherein the genus *Porzana* is well represented by the subject of Dr. Schufeldt's memoir—the common Sora or Carolina Rail. A complete account of the osteology of this ralline form has never been published, yet its skeleton contains many points of interest, to say nothing of importance when it is compared with other types. When his material better admits of it, Dr. Schufeldt proposes to thoroughly compare the anatomy of the several forms of American Cranes and Rails.

THE Cavendish Lecture, delivered at the West London Hospital by Sir William Stokes, has just been published. The subject is "The Altered Relations of Surgery to Medicine."

ACCORDING to the report of the Medical Missionary Society's Hospital in Canton for 1887, the medical class numbered twelve Chinese, of whom four were women. The students are required to pay a fee, which is fixed at twenty dollars a year for three years, over which period the course extends. They support themselves and buy their own books. Western medicine and surgery are slowly but surely advancing in China, and it is now time that schools of a high order were established. The publication of many medical books, the establishing of hospitals, in which millions of patients have been treated, the training of hundreds of students, the skill of the European physicians practising in the open ports—all tend to educate China and prepare the way for greater things.

THE Russian Statistical Committee having made minute inquiry as to the number of blind people in Russia, it appears that blindness is very unequally distributed among the different nationalities inhabiting the Empire. While there are only 8 blind people for each 10,000 Poles, 10 for as many Lithuanians and Jews, and 19 for Russians and Letts, the figures rise to 22 with the Esthonians, 35 with the Bashkirs, 41 with the Mordovians, 51 with the Tartars and Tcheremisses, 63 with the Tchuvashes, and 83 with the Votyaks. Blindness is thus much more widely spread among the Ural-Altayans, and especially among the Finnish-Mongolian stems, than among the Aryans and Semites, although the conditions of all these races, so far as poverty is concerned, are much the same. It is worthy of note that one-eighth of all cases of blindness in Russia are due to small-pox, and one-half only to direct eye diseases.

A JOINT exhibition will be made at the "Cincinnati Centennial" by the National Museum, the Smithsonian Institution, the U.S. Geological Survey, and the Bureau of Ethnology. The law providing the necessary funds was not approved until May 28, so that there has been little time for preparation; but "the Government scientific exhibits," says *Science*, "will be in Cincinnati in good season, and will constitute one of the most interesting features of the exposition." In the Department of Anthropology the National Museum will exhibit cases of objects showing the geographical distribution and physical characteristics of the races of men, and the processes and results of some of the most primitive arts. It will also exhibit a collection illustrating Biblical archæology, and a collection of remains of prehistoric man in Europe, Asia, and America. In connection with the same department the Bureau of Ethnology will have a good exhibition. It has chosen as its special subject the Pueblo of Zuñi, its arts and industries; and it will show various models of Indian mounds of the Mississippi.

THE additions to the Zoological Society's Gardens during the past week include a Mona Monkey (*Cercopithecus mona* ♂) from West Africa, presented by Miss Edith Frank; a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Miss Chester; a Brown Capuchin (*Cebus fatuellus* ♂) from Guiana, presented by Mr. Roger M. Dodington; a —— Capuchin (*Cebus* ——) from Columbia, presented by Mr. H. B. Whitmarsh; a Grand Eclectus (*Eclectus roratus*) from Moluccas, a Red-sided Eclectus (*Eclectus pectoralis*) from New Guinea, presented by Lt.-Col. R. Wolfe; two Corn Crakes (*Crex pratensis*) British, presented by Mr. R. B. Spalding; a Green Turtle (*Chelone viridis*) from the West Indies, presented by Baron Henry de Worms; two Hog-nosed Snakes (*Heterodon platyrhinos*), a —— Snake (*Cyclophis æstiva*), two Carolina Anolises (*Anolis carolinensis*) from North America, presented by Mr. H. E. T. Glover; two European Tree Frogs (*Hyla arborea*) European, presented by Mr. Lionel A. Williams; a Tuberculated Iguana (*Iguana tuberculata*), two Common Boas (*Boa constrictor var. diviniloqua*), a —— Snake (*Dromicus ater*) from the West Indies, presented by the West Indian Natural History Exploration Committee; two Ruffed Lemurs (*Lemur varius*) from Madagascar, a Hyacinthine Macaw (*Ara hyacinthina*) from Northern Brazil, three Red and Blue Macaws (*Ara macao*) from Central America, four Spotted Tinamous (*Nothura maculosa*) from Buenos Ayres, deposited; two King Crabs (*Limulus polyphemus*) from North America, purchased; two Mule Deer (*Cariacus macrotis*) born in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JULY* 22–28.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on July 22

Sun rises, 4h. 12m.; souths, 12h. 6m. 11·8s.; sets, 20h. 0m.: right asc. on meridian, 8h. 8·9m.; decl. 20° 9′ N. Sidereal Time at Sunset, 16h. 4m.

Moon (Full on July 23, 6h.) rises, 19h. 39m.; souths, 23h. 53m.; sets, 4h. 10m.*: right asc. on meridian, 19h. 57·3m.; decl. 20° 25′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	3 6	10 54	18 42	6 56·0	19 10 N.
Venus.....	4 23	12 20	20 17	8 22·3	20 34 N.
Mars......	12 49	17 48	22 47	13 51·2	12 34 S.
Jupiter....	15 10	19 34	23 58	15 37·6	18 37 S.
Saturn....	4 58	12 43	20 28	8 45·4	18 43 N.
Uranus...	11 8	16 47	22 26	12 51·0	4 46 S.
Neptune..	0 11	7 58	15 45	4 0·2	18 55 N.

* Indicates that the setting is that of the following morning.

Comet Sawerthal.

July.	h.	Right Ascension. h. m.	Declination. ° ′
22	0	1 7·9	51 59 N.
26	0	1 7·3	52 38

Occultations of Stars by the Moon (visible at Greenwich).

July.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
22	*o* Sagittarii	4	0 42	1 15	173 235
23	20 Capricorni	6	21 35	22 46	64 269
26	74 Aquarii	6	0 57	2 11	80 308

July.	h.	
23	—	Total eclipse of Moon: first contact with penumbra 2h. 57m.: first contact with shadow 3h. 55m., shortly after which, at 4h. 10m., the Moon sets at Greenwich.
23	22	Jupiter stationary.
24	4	Venus at least distance from the Sun.
27	13	Venus in conjunction with and 0° 35′ north of Saturn.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m	
U Cephei	0 52·4	81 16 N.	July 25,	20 50	*m*
R Piscium	1 24·9	2 18 N.	,, 22,		*M*
W Virginis	13 20·3	2 48 S.	,, 25,	3 0	*M*
U Boötis	14 49·2	18 9 N.	,, 22,		*m*
δ Libræ	14 55·0	8 4 S.	,, 27,	0 18	*m*
V Coronæ	15 54·5	39 55 N.	,, 26,		*M*
U Ophiuchi	17 10·9	1 20 N.	,, 24,	2 50	*m*
			,, 24,	22 58	*m*
U Sagittarii	18 25·3	19 12 S.	,, 27,	1 0	*m*
β Lyræ	18 46·0	33 14 N.	,, 27,	23 0	*M*
η Aquilæ	19 46·8	0 43 N.	,, 25,	23 0	*M*
X Cygni	20 39·0	35 11 N.	,, 25,	0 0	*M*
δ Cephei	22 25·0	57 51 N.	,, 23,	2 0	*m*
R Pegasi	23 1·0	9 56 N.	,, 28,		*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near δ Cassiopeiæ	20	59 N.	Very swift. Streaks.
The *Perseids*	25	53 N.	Swift. Streaks.
The *Aquarids*	340	13 S.	Max. July 28.

GEOGRAPHICAL NOTES.

The last survey of the Austrian Alps, we learn from the Proc. R.G.S., has already led to some important, if not altogether unexpected results. Thus the Marmolata, the highest dolomite, is reduced from 11,464 feet to 11,016 feet. The Antelao comes next, reaching, according to the new Italian survey, 10,874 feet. Mr. D. Freshfield pointed out in 1875, in his "Italian Alps," that the two highest points of the Primiero group do not differ by 159 metres, as then indicated in the Government survey, but are almost equal in height. The new measurements show a difference of only 16 feet between them, and reverse the advantage. The figures are subjoined:—

	Last survey. m.	Old survey. m.	Previous Cadaster measurement. m.
Cima di Vezzana	3191	3061	3317
Cimon della Pala	3186	3220	3343

The Cima di Vezzana is therefore 10,470 feet and the Cimon della Pala 10,454 feet. The remaining peaks of the Primiero group gain or lose only a few feet by the new measurements.

Mr. W. J. Archer, British Vice-Consul at Chiengmai, has written an interesting Report of a journey he made in his district last year. This journey extended north along the Meping River, north-east to Chiengsin on the Cambodia River, south and east to Nan on the Nam Nan, then westwards across the Meyom, by Lakhon to Chiengmai. Several maps accompany the Report, which add considerably to our knowledge of the topography of the region visited. Mr. Archer, writing of the new capital of Müang Fāng, describes the manner in which this and similar new settlements were formed in Siam. In such new colonies, as the people spread out over the districts around, other settlements were gradually formed at a distance from the capital. A large body of immigrants, or a number of families from the same locality, generally form a separate settlement, especially if they are of a different race from the original settlers; and if they settle in the capital they usually have a separate quarter allotted to themselves. This is characteristic of all the settlements in Siam, both in the larger cities and in the provinces. In Bangkok the inhabitants of the different quarters have gradually become amalgamated; but not far from the capital the colonies of former captives of war still retain their language and customs, and keep up little intercourse with their conquerors. In the northern country the separation is as complete, and the area of Chiengmai, for example, is divided into numerous quarters, each inhabited almost exclusively by people of a different race; and many of the villages in the provinces are also colonies of refugees or captives. Mr. Archer is of opinion that the country of the Thai Yai (literally "great Siamese"), or its vicinity, is the cradle of the Thai people, who thence gradually flowed southward. The Thai family has numerous divisions, differing more or less in appearance, language, and costume, though it is not difficult to trace the common type through all. The whole subject of the gradual development and modifications of the Thai race is a very interesting one from an ethnological point of view, and, Mr. Archer thinks, well worthy of research for the light it may throw on the early history of Indo-China. Mr. Archer gives many useful notes on the various hill-tribes of the country, whose distribution and characteristics deserve careful investigation. It is to be hoped he may have further opportunities of exploring the region and collecting additional information.

The Council of the Russian Geographical Society have issued a memorandum with regard to the teaching of geography in the Universities. This memorandum will probably be taken as a basis for the impending organization of University teaching and degrees in geography in Russia. "Geography," the Council write, "being a study of the laws and associations of phenomena of the physical and organic life of the earth, it implies a serious preliminary study of natural sciences. Without a serious knowledge of the laws of physics, it is impossible to reason upon the laws dealing with the physical features of the globe. For recognizing its true place in the solar system, its figure and movements, the knowledge of astronomy and geodesy is absolutely necessary. The origin of the present features of the surface of the earth cannot be dealt with without a knowledge of geology and mineralogy. Botany and zoology are necessary for studying the laws of the distribution of organisms; while a knowledge of anatomy and physiology is necessary for the study of anthropology, phyto-geography, zoo-geography, and anthropo-geography, and so on." The experience of the German Universities having shown how difficult it is for the student to master all these subjects if he merely follows the usual lectures of the Natural Sciences Faculty, the Council express a hope that special courses, appropriate to the requirements of geographical students, may be opened in physics, astronomy and geodesy, chemistry, mineralogy and petrography, geology and the study of soils (a branch which has lately received a good deal of attention in Russia), zoology, anatomy and zootomy, physiology, history, literature, comparative philology, and the leading principles of political economy and statistics. Psychology being intrusted in Russian Universities only to Professors chosen from among the clergy, the Council urge that it should be introduced into the Natural History Faculty. As to geography proper, they advise, first, that there shall be two separate Professors for geography and anthropology, and point out the absolute impossibility of combining both sciences in one professorship. They propose, moreover, to divide the course of geography into two distinct parts, physical geography (*Erdkunde*) and special geography (*Länderkunde*). Historical geography is excluded from the programme, its contents belonging partly to history and partly to the *Länderkunde*. Although fully recognizing the difficulty of having lectures in all the above-named subjects especially appropriated to the needs of geography, the Council suggest that *privat-docents* might supply the new want. But if this is found to be impossible, they advise that the students who wish to take either geography or anthropology as their specialty should be left to select in the above-named group of sciences those subjects which would best suit them. Students might thus take any one of the three chief directions opened to the geographer—namely, that of the geologist-geographer, the biologist-geographer, or the anthropologist-geographer.

THE MULTIPLICATION AND DIVISION OF CONCRETE QUANTITIES.[1]

I HAVE recently been laying stress on the fact that the fundamental equations of mechanics and physics express relations among *quantities*, and are independent of the mode of measurement of such quantities; much as one may say that two lengths are equal without inquiring whether they are going to be measured in feet or metres; and indeed, even though one may be measured in feet and the other in metres. Such a case is, of course, very simple, but in following out the idea, and applying it to other equations, we are led to the consideration of products and quotients of concrete quantities, and it is evident that there should be some general method of interpreting such products and quotients in a reasonable and simple manner. To indicate such a method is the object of the present paper.

For example, I want to justify the following definition, and its consequences: Average velocity is proportional to the distance travelled and inversely proportional to the time taken, and is measured by the distance divided by the time, or, in symbols, $v = s \div t$. As a consequence of this, the distance travelled is equal to the average velocity multiplied by the time, or $s = vt$. The following examples will serve to illustrate what I mean:—

(i.) If a man walks 16 miles in 4 hours, his average speed is $\frac{16 \text{ miles}}{4 \text{ hours}} = 4 \times \frac{1 \text{ mile}}{1 \text{ hour}} = 4$ miles an hour, the symbol $\frac{1 \text{ mile}}{1 \text{ hour}}$ denoting a speed of a mile an hour, in accordance with the definition.

Similarly, $\frac{1 \text{ foot}}{1 \text{ second}}$, or shortly, $\frac{\text{ft.}}{\text{sec.}}$, denotes a velocity of a foot per second. The convenience of this notation is that it enables us to represent velocities algebraically, and to change from one mode of measurement to another without destroying the *equation*.

Thus $\frac{16 \text{ miles}}{4 \text{ hours}} = \frac{4 \text{ miles}}{1 \text{ hour}} = \frac{4 \times 1760 \times 3 \text{ feet}}{60 \times 60 \text{ seconds}} = 5{\cdot}9 \ \frac{\text{ft.}}{\text{sec.}}$ $= 5{\cdot}9$ feet per second.

(ii.) The distance travelled in 40 minutes by a person walking at the rate of $4\frac{1}{2}$ miles an hour $= \frac{4\frac{1}{2} \text{ miles}}{1 \text{ hour}} \times 40 \text{ minutes} = \frac{4\frac{1}{2} \text{ miles}}{3} \times 2 = 3$ miles.

Such concrete equations are used by a considerable number of people, I believe, but I have not seen any attempt at a general method of interpreting the concrete products and quotients involved.

Now, I think I cannot do better by way of clearing the ground before us than quote what Prof. Chrystal says in his "Algebra" about multiplication and division. He begins by saying that multiplication originally signified mere abbreviation of addition; and then (on p. 12) he says:—

"Even in arithmetic the operation of multiplication is extended to cases which cannot by any stretch of language be brought under the original definition, and it becomes important to inquire what is common to the different operations thus comprehended under one symbol. The answer to this question, which has at different times greatly perplexed inquirers into the first principles of algebra, is simply that what is common is the formal laws of operation [the associative, commutative, and distributive laws]. These alone define the fundamental operations of addition, multiplication, and division, and anything further that appears in any particular case is merely a matter of some interpretation, arithmetical or other, that is given to a symbolical result, demonstrably in accordance with the laws of symbolical operation."

"*Division*, for the purposes of algebra, is best defined as the inverse operation to multiplication."

I will begin by considering instances, and then go on to the general case.

A product of a number and a concrete quantity presents no difficulty. All that is necessary is to define that the order of stating the product shall not alter its meaning—that is, that the *commutative* law shall hold—that,

$$e.g.,\ 2 \times 1 \text{ foot} = 1 \text{ foot} \times 2 = 2 \text{ feet}.$$

The *distributive* law is satisfied; thus,

$$2 \text{ feet} + 3 \text{ feet} = (2 + 3) \text{ feet} = 5 \text{ feet}.$$

In interpreting the meaning of the product of two concrete quantities, we have to be careful that in the interpretation nothing shall violate the laws of *numerical* multiplication; *i.e.* if any numerical factors occur, they must be able to be multiplied in the ordinary way, and placed before the final concrete product, which must, of course, represent something which varies directly with both quantities.

Thus 4 feet × 2 yards must be equal to 8 × 1 foot × 1 yard.

Now a *rectangle*, whose sides are 4 feet and 2 yards, is eight times the rectangle whose sides are 1 foot and 1 yard, so that, if we define the product of two lengths as representing a rectangle whose sides are these lengths respectively, we are not violating any multiplication law as regards the numerical multipliers; and we can compare one such rectangle with any other whose sides are of different lengths, by ordinary multiplication and division among such numbers as arise, and by interpretation of the concrete products in accordance with the definition.

$$\begin{aligned} \text{Thus, } 4 \text{ feet} \times 2 \text{ yards} &= 8 \times 1 \text{ foot} \times 1 \text{ yard}, \\ &= 24 \times 1 \text{ foot} \times 1 \text{ foot}, \\ &= 24 \text{ square feet}, \\ &= 24 \times 12 \text{ inches} \times 12 \text{ inches}, \\ &= 3456 \text{ square inches}, \\ &\quad \&c. \end{aligned}$$

Here we have applied the commutative law so as to bring the numerical factors together for multiplication, and have interpreted the remaining concrete products in accordance with the definition.

The general result is that $ab = \alpha\beta \,.\, a'b'$, if $a = \alpha a'$, and $b = \beta b'$, *i.e.* a rectangle whose sides are a, b is $\alpha\beta$ times a rectangle with sides a', b', if $a = \alpha a'$, and $b = \beta b'$.

From this example I think we can see that a concrete product may properly be used to represent any quantity that varies directly as the several concrete factors, and that, being so represented, it may, by use of the ordinary rules of multiplication, be compared with any other concrete product of the same kind; that is to say, that, *generally*, $ab = \alpha\beta \,.\, a'b'$, if $a = \alpha a'$, and $b = \beta b'$, where α, β are numerical factors, and a, a' are different amounts of one kind of quantity, and b, b' of another kind.

Similarly, a concrete quotient may be used to represent a quantity which varies directly as the concrete numerator and inversely as the concrete denominator, and may, by the ordinary rules of multiplication and division, be compared with any other quantity of the same kind.

Indeed, I may go further and assert that a concrete product or quotient (the latter including the former) MUST, if it is to have any meaning at all, represent a quantity varying directly as the concrete factors in the numerator and inversely as those in the denominator, and that the general use of such representation is for comparison of the complex quantity with a standard of the same kind. Or, generally, we may say it should be used, whenever we wish, in our work, to give as full and explicit a representation to the complex quantity as possible.

The operation of multiplying [and dividing] concretes may be separated into two parts: the *formation* of the products, and the *simplification* of them; and this latter process may be again considered in two parts: the simplification of the numerical factors, *i.e.* ordinary multiplication and division, and the simplification of the concrete factors, *i.e.* cancelling where possible, and, finally, interpretation.

[1] Paper read at the General Meeting of the Association for the Improvement of Geometrical Teaching, on January 14, 1888, by A. Lodge, Cooper's Hill, Staines.

The first part of the multiplication is the *representation* of a complex quantity which is proportional to the several factors in the numerator, and inversely proportional to those in the denominator; the second part is the comparison between the particular complex quantity and a standard of the same kind. The representation may be temporary, *i.e.* adopted for the solution of a particular problem; or it may be permanent, *i.e.* adopted throughout a whole subject.

Thus, if a, b are two lengths, the product ab is always used to represent a *rectangle* whose sides are a, b respectively; though we *might* have agreed to use it as a representation of a parallelogram with sides a, b containing an angle of (say) 60°; and of course we might find a number of things which in some particular problem might be represented by ab, but all such quantities must agree in this property, viz. that in the problem in question they shall vary jointly as a and b.

Our right to cancel among concretes may be established once for all in some such way as the following:—

Let $a = \alpha a'$, $b = \beta b'$, and therefore $ab = \alpha\beta \cdot a'b'$, as before. Now, if we proceed to deduce a formally from the equation $ab = \alpha\beta \cdot a'b'$, we shall get $a = \frac{\alpha\beta \cdot a'b'}{b}$, which reduces down to its known value $\alpha a'$ if we allow b in the denominator to cancel against its equivalent $\beta b'$ in the numerator. (This cancelling is really an application of the law of association to the quotients.)

By such methods as this we can establish once for all our right to apply the formal laws of multiplication and division to concrete products and quotients, when such concrete products and quotients represent quantities varying directly as the concrete numerator and inversely as the concrete denominator; though, indeed, for that matter a very little practice in the use of such concrete representations renders one's perception of that right almost intuitive. In fact, in all cases a student would very soon perceive that the standards involved in the various equations might be treated exactly like numbers, and he would also learn from the resulting expressions $\left(e.g.\ \frac{\text{foot}}{\text{sec.}}, \frac{\text{foot}}{(\text{sec.})^2}, \text{\&c.}\right)$ to appreciate the meaning of the *dimensions* of quantities with a thoroughness unattainable in any other way.

All questions dealing with mixed standards, or change of standards, present no difficulty when this method is adopted.

Here is a good example of the concrete method. Two ton-masses placed a yard apart attract each other with a force equal to the weight of one-eighth of a grain. Calculate the mass of the earth in tons.

Solution.
$$\frac{\text{earth} \times \frac{1}{8}\text{ grain}}{(4000\text{ miles})^2} = \frac{1\text{ ton} \times 1\text{ ton}}{(1\text{ yard})^2}$$
$$\therefore \text{mass of earth} = \frac{1\text{ ton}}{\frac{1}{8}\text{ grain}} \times \left(\frac{4000\text{ miles}}{1\text{ yard}}\right)^2 \text{tons}$$
$$= \text{\&c.}$$

It is most important that the student should be taught to notice that physical equations can only be among quantities of the same kind, or that, if there are quantities of different kinds in the equation, then the equation is really made up of two or more independent equations which must be separately satisfied, each of these being only among quantities of the same kind. So we may consider generally that, in any equation, all the terms must represent quantities of the same kind.

But I want to call attention to the fact that merely the dimensions of a quantity do not always fix the kind of quantity. For example, the moment of a force is of the dimensions of work, and yet it is not work, and cannot exist as a term in an equation involving *work* terms. Again, the circular measure of an angle is not a pure number, though it is of zero dimensions as a pure number is; and that it is not a pure number is evident physically, for a moment of a force × an angle = work.

Now these are special cases of certain general laws as to direction which hold among the terms of an equation involving directed quantities, *but in which the symbols themselves do not include the idea of direction* (for I wish to confine myself strictly to ordinary algebraical equations).

The laws are: firstly, if any term is independent of direction, every term must be also independent of direction, or involve ratios between *parallel* vectors, and so by cancelling direction become independent of it.

E.g. if a body is projected with velocity V at angle α with the horizon, it reaches its greatest height in the time $\frac{V \sin \alpha}{g}$.

Here both numerator and denominator are vertical vectors, and therefore the directions cancel as they ought.

Secondly, if any term involve only one vector, the other terms must also, after such simplification of directions as possible, involve *the same* vector only.

E.g. Horizontal range of projectile $= \frac{2V^2 \sin \alpha \cos \alpha}{g}$, where $V \sin \alpha$ and g are vertical vectors, and $V \cos \alpha$ is horizontal, so that the whole expression is a horizontal vector, as it should be.

Again, if any term involve a product (or ratio) between two vectors including any angle, every term must, after such cancelling and simplification of directions as possible, also involve a product (or ratio) between two vectors including the same angle.

The most frequent cases are those where a term consists of a product of parallel, or mutually perpendicular directed quantities, in which case every term must do the same.

It is not easy to see what law holds in cases where a greater number of directed quantities occur in each term, except in the simple case where one term consists of a product of a number of parallel vectors, in which case every term must do the same.

The general law is, I believe, that if any term consists in its simplest form of a product or quotient of certain vectors, which will form a kind of solid angle, then every term must also involve an exactly *similar* solid angle of vectors. However, I have not followed this out, as it does not seem likely to be a useful test in its general form.

The following are simple examples of some of the above laws:

$$\left.\begin{aligned} b &= a \cos C + c \cos A \\ a^2 &= b^2 + c^2 - 2bc \cos A \end{aligned}\right\} \text{ in a triangle;}$$
$$y = mx + c;$$
$$\sin (A + B) = \sin A \cos B + \cos A \sin B.$$

This last example should be considered in connection with the ordinary geometrical proof, where it will be seen that each term on the right is a ratio between lines inclined to each other at the angle 90° − (A + B), just as the left-hand side is.

An angle is the ratio between the arc and radius of a circle, and if it multiplies a radius, changes it into an arc. Thus, if by applying a force P at the end of an arm a, a body is turned through a small angle θ, the work done is $Pa\theta$; *i.e.* the product of P into the arc through which it has been acting, which is a product of *parallel* vectors, as it must be besides having to be of right dimensions if it is to represent work. This expression is also the product of the moment of the force into the small angle turned through, so that, if we wish to connect the moment of a force with work, we must say:—

The moment = the work *per radian* which can be done,

$$\text{or simply, moment} = \frac{\text{work done}}{\text{angle turned through}}.$$

Now I do not wish to insist that in dealing *practically* with mechanical problems it is necessary always to include the standards as well as the numerical multipliers in the equations, for it would be an intolerable nuisance to have to do so. In complicated cases, however, I think the student should test the dimensions of each term in his equation, so as to avoid gross mistakes. But it is in trying to *understand* the fundamental equations in any subject that it appears to me important to express particular examples of them as fully as possible.

For practical purposes any numerical equations we may desire may be deduced from the fundamental equations.

For example, the connection between the height (h) of an observer above the sea with the distance (d) of his horizon, is $d^2 = 2Rh$, where R is the radius of the earth; and we can deduce from this the numerical relation between the height in *feet*, and the distance of vision in *miles*. For if f be the number of feet in h, and m the number of miles in d, so that $h = f$ feet, and $d = m$ miles, the equation becomes

$$(m\text{ miles})^2 = 2R \times f\text{ feet},$$
$$= 8000\text{ miles} \times f\text{ feet};$$
$$\therefore f = m^2 \frac{(\text{miles})^2}{8000\text{ miles} \times 1\text{ foot}} = \frac{5280}{8000} m^2,$$
$$= \tfrac{2}{3} m^2 \text{ approximately};$$

i.e. the observer's height in feet $= \frac{2}{3}$ of the square of the distance of his view in miles.

This is a strictly numerical equation, deduced for practical purposes from the concrete equation $d^2 = 2Rh$.

It cannot, I think, be too clearly impressed on the student that, when any quantity is expressed by a number, that number is the *ratio* of the quantity to some standard of the same kind.

To take the preceding example, f is the number of feet in the height h.

$$i.e.\quad h = f \text{ feet},$$

$$\therefore f = \frac{h}{1 \text{ foot}} = \text{the } ratio \text{ of } h \text{ to 1 foot.}$$

$$\text{Similarly } m = \frac{d}{1 \text{ mile}} = \text{the } ratio \text{ of } d \text{ to 1 mile.}$$

So that the full expression for the relation $f = \frac{2}{3}m^2$ is :—

$$\frac{\text{height}}{1 \text{ foot}} = \frac{2}{3} \text{ of} \left[\frac{\text{distance}}{1 \text{ mile}}\right]^2.$$

My position, therefore, as regards numerical equations, is this: That the numbers which appear are only short methods of stating pure ratios, and that such short methods are eminently useful in dealing with practical problems, but do not help a student to grasp the fundamental principles of a subject.

There is another simple way in which numerical equations can be deduced from the fundamental ones; viz. by so choosing the standards of measurement that every term may be expressed in terms of the same standard, which may then be omitted, leaving only a relation among the numerical coefficients of that standard.

To enable this to be done, all the standards of subsidiary quantities are so chosen that, when expressed in terms of certain primary standards, their coefficients shall be unity. When this is systematically done, all the standards are usually called *units*, apparently because if you arbitrarily put *unity* for each primary standard, the subsidiary ones will become equal to unity also.

For example, if a foot and a second are chosen units of length and time, a foot per second is the unit of velocity. For, the full expression for a foot per second is $\frac{1 \text{ foot}}{1 \text{ sec.}}$; and if you put 1 foot = 1, and 1 sec. = 1, the fraction $\frac{1 \text{ foot}}{1 \text{ sec.}}$ becomes equal to 1 also.

This plan certainly enables the working numerical equations to be very easily deduced from the fundamental ones, with which indeed they thus become identical in form, but there is great danger lest this fact should make us lose sight of the important fact that they are only special deductions from the higher kind of equation—from the true fundamental equations which exist among the quantities themselves.

DISCOVERY OF ELEPHAS PRIMIGENIUS *ASSOCIATED WITH FLINT IMPLEMENTS AT SOUTHALL.*

A PAPER with the above title was lately read by Mr. J. Allen Brown before the Geologists' Association. It is of more than ordinary interest to geologists since an attempt has lately been made to show that the mammoth became suddenly extinct by the action of a vast flood seemingly universal in its operation, due to some convulsion or cataclysm, which also changed the climate of Northern Europe.

During last year some important drainage works were carried out at Southall, and sections were exposed in the Windmill Lane, a road running from Greenford, through Hanwell, across the Great Western Railway to Woodlake, skirting Osterley Park, as well as in Norwood Lane, leading from Windmill Lane, south-westward.

The remains of the mammoth were discovered in Norwood Lane at the 88-foot contour, about 550 yards from its junction with the Windmill Lane. They were embedded in sandy loam, underlying evenly stratified sandy gravel, with a thin deposit of brick earth, about 1 foot in thickness, surmounting the gravel—in all, about 13 feet above the fossils. The tusks were found curving across the shore or excavation, attached to the skull, parts of which, with the leg-bones, teeth, &c., were exhumed, other bones being seen embedded in one side of the cutting. Probably the entire skeleton might have been removed if the excavation could have been extended, and if there had been appliances at hand for removing the fossils, which were in a soft pulpy condition.

The author obtained some of the bones in a fragmentary state, including parts of the fore-limbs and jaw, with portions of the tusks as well as two of the three teeth found, which were much better preserved. The remains were quite unrolled, and the joints and articulations of the leg-bones and the teeth were unabraded. There can hardly be a doubt, from the report of the workmen, that the bones of the fore-part of the elephant, if not of the entire skeleton, were in juxtaposition.

Several implements were found in Norwood Lane, in close proximity to the remains, and a well-formed spear-head, nearly 5 inches in length, of exactly the same shape as the spear-heads of obsidian until recently in use among the natives of the Admiralty Islands, and other savages, was discovered in actual contact with the bones; smaller spear-head flakes, less symmetrically worked, were also found at this spot. They are formed for easy insertion into the shafts by thinning out the butt ends, similar to those found abundantly by the author at the workshop floor, Acton, and described by him in his recently published work, "Palæolithic Man in North-West Middlesex." Among the implements found at this spot are an unusually fine specimen of the St. Acheul or pointed type, 8 inches long, of rich ochreous colour and unabraded, and a well-formed lustrous thick oval implement pointed at one extremity, rounded at the other, about 5 inches in length, also unrolled.

From the adjacent excavations in the Windmill Road several good specimens of Palæolithic work were also obtained, including two dagger implements, with heavy unworked butts, and incurved sides converging to a long point; these were evidently intended to be used in the hand without hafting. Also an instrument characteristic of the older river drift, convex on one side, and slightly concave on the other near the point, and partly worked at the butt. With these were two rude choppers or axes, two points of implements with old surfaces of fracture, a shaft-smoother or spoke-shave, and several flakes.

It is remarkable that most of the principal types of flint implements which characterize the oldest river-drift deposits are represented in this collection from the vicinity of the remains of the elephant.

Mr. J. Allen Brown accounts for the deposit of fossils and associated human relics at this locality by the fact that the underlying Eocene bed rises to within 2 or 3 feet of the surface a few yards west of the spot where the bones and implements were found, while towards the Uxbridge Road and upper part of the Windmill Lane the drift deposits thicken, until at no great distance they have a thickness of 14 to 17 feet. Thus the river drift rapidly thins out, and the upward slope of the London Clay reaches nearly to the surface at about the 90-foot contour. As the level at which the fossils were found (13 feet from the surface) would represent the extent of the erosion and in-filling of the valley which had taken place, it is probable that the higher ground formed by the up-slope of the London Clay then formed the banks of the ancient river; or if another thick bed of drift should be found still further west in a depression of the Tertiary bed such as often occurs, the intervening higher ground would form an island in the stream. In either case a habitable land surface would be formed with shallow tranquil water near the banks, not impinged upon by the current, which afterwards set in the direction of this spot as shown by the coarser stratified gravel above the loamy bed and remains.

The author is thus led to the conclusion either that the carcass of the elephant drifted into the shallow tranquil water near the bank, or else, as seems more probable from the presence of so many weapons near the spot, including the spear-head found with the remains, that the animal was pursued into the shallow water by the Palæolithic hunters and there became bogged. Whatever hypothesis may be accepted, there is no evidence of any greater flood or inundation than would often occur, under the severe climatal conditions which prevailed during the long period that intervened between the formation of the higher brenches of river drift and that of the mid terrace, only 25 to 30 feet above the present river, in which the remains of the mammoth and the extinct Quaternary Mammalia are more frequently met with under similar conditions. Nor does there appear to be any more reason for ascribing the extinction of the great Quaternary Pachyderms to a sudden catastrophe or cataclysm than there is for the extinction of some other Pleistocene animals, such as the great Irish elk, which lived on into, or nearly into, historic times. The difficulty involved in this hypothesis is still further increased by the fact that other animals, such as the reindeer and others of northern habit, as well as southern forms like the hippopotamus, were not utterly destroyed with their contemporaries by the same cause, but merely migrated to regions more suited to them, as the climate and other conditions of this country changed.

Exhibits.—Bones, teeth, and part of the tusks of mammoth, and associated flint implements from Southall. A flint implement from the lacustrine (?) bed at the Mount, Ealing (190 to 200 O.D.) (See the author's paper, Proceedings Geologists' Association, vol. x. No. 4). A flint apparently worked by man from the Weybourn Crag, beneath the "Forest bed" near Cromer. A Palæolithic scraper found on the beach near Cromer, &c.

THE POISONOUS SNAKES OF THE BOMBAY PRESIDENCY.

AT a recent meeting of the Bombay Natural History Society, a paper was read by the Honorary Secretary, Mr. H. M. Phipson, on the "Poisonous Snakes of the Bombay Presidency." He produced for inspection specimens of the following poisonous snakes, all of them having been killed in the Presidency of Bombay.

Colubrine.—(1) *Ophiophagus elaps*; (2) *Naga tripudians*; (3) *Bungarus arcuatus*; (4) *Callophis trimaculatus*; (5) *Callophis nigrescens.*

Viperine.—(6) *Daboia elegans*; (7) *Echis carinata*; (8) *Trimeresurus anamallenis*; (9) *Hypnale nepa.*

With regard to the first species, the *Ophiophagus elaps*, it is perhaps the largest poisonous snake in the world, sometimes measuring over 15 feet. It is also called the "king cobra" or "hamadryad," and is not very common, though widely diffused, being found in the Andamans, the Philippines, Borneo, Java, and Sumatra. On account of expanding a "hood," it is frequently mistaken for the cobra, but the head-shields of the hamadryad differ very much from those of the cobra. The second species, *Naga tripudians*, or cobra, is found all over India, and up to the height of 8000 feet in the Himalayas. There are many varieties, differing in colour and marking, to which the natives give different names, thinking them distinct species; but in such matters the native knowledge is not very extensive. Thus they believe that all the hooded cobras are females, and that the males are harmless. What they call the male is in reality only the common Indian rat snake (*Ptyas mucosus*). They also state that the rat snake is proof against the poison of the cobra. But this is not the case. Last year the young ones hatched in the Society's rooms attacked a small Malay python put into their cage, when they were only a few days old, and bit at it viciously, and the python died in a few hours after its removal to another cage. Once a year, during the rainy season, the cobra lays from twelve to twenty eggs. In one specimen shown by Mr. Phipson, the young one is seen just as it is emerging from the egg. The tooth with which it cuts its way out is shed as soon as it has served its purpose. When born, the young cobras measured about 7½ inches long, and were very fat; but at the end of a few months they were about 9 inches in length, but had lost all their plumpness. It is very remarkable that the original nutriment got out of the egg should be able to sustain them so long. On account of its timidity and the great ease with which it can be tamed, it is the only snake with which the snake-charmers will have anything to do. By attracting its attention with one hand, it may be easily seized round the body with the other; and so long as the hand or any other object is kept moving before its eyes, it will never turn to bite the hand that holds it. This is the simple fact the knowledge of which the charmers turn to such advantage in their well-known performances. The snake is taken from its basket, and a slight stroke across the back brings it at once into a defensive attitude. The constant motion of the musical instrument before the snake keeps it watchful and erect, and not the music produced. As a matter of fact, snakes have no external ears, and it is extremely doubtful whether the cobra hears the music at all. The charmers say that the adder of the East, the *Daboia*, has no ear for music, because they cannot operate on it as they do on the cobra. It is rather interesting to note that this has been the belief since David's time at least—"like the deaf adder that stoppeth her ear; which will not hearken to the voice of charmers." (3) The krait (*Bungarus arcuatus*) is an exceedingly poisonous snake, and is quite common in nearly every part of India. One specimen taken in the Bombay Presidency contained a "brown tree snake" (*Dipsas gokool*) and in another specimen was found a *Ptyas mucosus*, thus showing that this species eats snakes. The common *Lycodon aulicus*, one of the non-poisonous snakes, is very much like the krait, but they can be distinguished by the presence in the krait of large hexagonal scales down the centre of the back. (4) The *Callophis trimaculatus* has no popular name. It is undoubtedly poisonous, and lives on other snakes, very likely the Calamariæ. (5) *Callophis nigrescens*, which grows to about 4 feet in length, is black in the upper parts and red in the lower.

(6) The first class of the Viperine snakes is the *Daboia elegans* called by Europeans in India the Chain Viper and in Ceylon the Tic Polonga. The fangs are very long, and for this reason, together with its fierceness, it is the most dreaded snake in India. Its poison acts differently from that of the cobra. Its tenacity of life is really wonderful, it having been known to live for a whole year without food. The length of this snake rarely exceeds 5 feet. (7) The *Echis carinata* and the last-named class are the only true vipers in India. The harmless "brown tree snake" (*Dipsas gokool*) is frequently confused with the *Echis carinata*, but they are easily distinguished by the scales on the head of the latter, while the *Dipsas gokool* has plates or shields. (8) The green tree viper (*Trimeresurus anamallensis*) is one of the family of Crotalidæ or pit vipers, so named from the pit or cavity beneath the eye and the nostril, of which family the terrible rattlesnake of America is a member. In India there are eight species of Trimeresuri, but up to the present only one has been found in Bombay, though it has been stated that another species, *T. strigatus* has been seen far up the country. (9) The head-quarters of the *Hypnale nepa*, or Carawala, are in Ceylon, but it is commonly found along the Malabar coast.

These classes include all the poisonous land-snakes. All the true sea-snakes are poisonous, and of these, amongst others, the following are in the Bombay collection: *Hydrophis diadema, Hydrophis robusta, Hydrophis curta, Hydrophis aurifasciatus, Hydrophis Phipsoni, Hydrophis Guntheri, Hydrophis Lindsayi, Hydrophis chloris, Enrhydrina bengalensis, Pelamis bicolor.*

SCIENTIFIC SERIALS.

Rendiconti del Reale Instituto Lombardo, May.—Foraminifera of Mount S. Colombano Lodigiano, by Dr. Ernesto Mariani. A classified list is given of these organisms, collected chiefly by Profs. Maggi and Balsamo Cuvelli in the district stretching from the right bank of the Lambro to within a few miles of the Po. The prevalence of Miliolidæ and allied forms shows that this fauna, which mostly still survives in the surrounding seas, flourished in the warm shallow waters which at a remote epoch flooded the plains of Lombardy.—On the use of the lucimeter in agriculture, by Prof. Giovanni Cantoni. The author's recent experiments with this instrument, first designed by Bellani, show that it is calculated to render great service to husbandry in combination with the thermometer and psycrometer.—Alberto Brambilla continues his paper on a certain class of algebraic surfaces; and Prof. A. Scarenzio has some remarks on the therapeutic properties of the arsenical thermal waters of Acquarossa, near Biasca, on the old St. Gothard road in the Canton of Ticino.

June 7.—On the normal curves of genus p of various spaces, by Prof. E. Bertini. Clifford's fundamental theorem is here established by a more synthetic method than any hitherto published demonstrations. The theorem itself (Philosophic Transactions, 1878, p. 681) is here announced in the following modified form:—A curve of genus p and order $n > 2p - 2$ cannot belong to a space of more than $n - p$ dimensions.—On the proposed sanitary legislation for Italy, by D. C. Zucchi. A calculation is made that by the adoption of such measures as are enforced by the Local Government Boards in Great Britain, the average mortality of the population might be reduced from over 27 to under 20 per thousand. This is shown to be equivalent to the rescue of 100,000 lives, whose labour for 300 working days represents an annual sum of nearly £5,000,000 at present lost to the nation.—Meteorological observations made at the Royal Observatory of Brera, Milan, for the month of May.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 14.—"The Minimum-point of Change of Potential of a Voltaic Couple." By Dr. G. Gore, F.R.S.

In this communication is described the following very simple method of detecting the influence of the minimum proportion of chlorine or other soluble substance, &c., upon the electromotive force of a voltaic couple (see NATURE, vol. xxxviii. p. 117).

Take a voltaic couple, composed of an unamalgamated strip of zinc or magnesium (the latter is usually the most sensitive), and a small sheet of platinum, immersed in distilled water; balance its electric potential through an ordinary galvanometer of about 100 ohms resistance by that of a precisely similar couple composed of portions of the same specimens of the same metals, immersed the same moment as the other pair in a separate quantity of the same water; and gradually add to one of the two cells sufficiently small and known quantities of an adequately weak solution of known strength in a portion of the same water, of the substance to be used, until the balance is upset, and take note of the proportions of the substance and of the water then contained in that cell. In the present experiments a magnesium platinum couple was employed.

The minimum proportions required with several substances were as follows: potassic chloride, between 1 part in 3875 and 4650 parts of water; potassic chlorate, between 1 in 4650 and 5166; hydrochloric acid, between 1 in 516,666 and 664,285; and with chlorine between 1 in 15,656,500,000 and 19,565,210,000.

The proportion required of each different substance is dependent upon very simple conditions, viz. unchanged composition of the voltaic couple, a uniform temperature, and employing the same galvanometer. The apparently constant numbers thus obtained may probably be used as tests of the purity or of the uniformity of composition of the dissolved substances.

The "minimum-point" varies with—(1) the chemical composition of the liquid; (2) the kind of positive metal; (3) to a less degree with the kind of negative metal; (4) the temperature at the surface of the positive metal, and at that of the negative one; and (5) with the kind of galvanometer employed.

The order of the degree of sensitiveness is manifestly related to that of the degree of free chemical energy of the liquid; also to the atomic and molecular weights of the dissolved substances, and to the ordinary chemical groups of halogens. The greater the degree of free chemical energy of the dissolved substance, and the greater its action upon the positive metal, the smaller the proportion of it required to change the potential.

As the "minimum point" of a chemically active substance dissolved in water is usually much altered by adding almost any soluble substance to the mixture, measurements of that point in a number of liquids at a given temperature with the same voltaic pair and galvanometer will probably throw some light upon the degree of chemical freedom of substances dissolved in water.

"On the Change of Potential of a Voltaic Couple by Variation of Strength of its Liquid." By Dr. G. Gore, F.R.S.

This paper contains a series of tables of measurements of the electromotive forces of a voltaic couple composed of unamalgamated zinc and platinum in distilled water, and in aqueous solutions of different strengths of the following substances: potassic chlorate, potassic chloride, hydrochloric acid, and bromine. The measurements were made by balancing the potential of the couple by that of a suitable thermo-electric pile (Proc. Birm. Phil. Soc. vol. iv. p. 130) through an ordinary astatic galvanometer of about 100 ohms resistance.

The following are the minimum proportions of those substances required to change the potential of the couple in water: potassic chlorate, between 1 in 221 and 258 parts of water; potassic chloride, between 1 in 695,067 and 1,390,134; hydrochloric acid, between 1 in 9,300,000 and 9,388,185; and of bromine, between 1 in 77,500,000 and 84,545,000 parts.

With each of these substances a gradual and uniform increase of strength of the solution from the weakest up to a saturated one was attended by a more or less irregular change of electromotive force.

By plotting the quantities of dissolved substance as ordinates to the electromotive forces as abscissæ, each substance yielded a different curve of variation of electromotive force by uniformly changing the strength of its solution, and the curve was characteristic of the substance. As the least addition of a foreign soluble substance greatly changed the "minimum-point," and altered the curve of variation of potential, both the curve and the minimum proportion of a substance required to upset the balance of the couple in water may probably be used as tests of the chemical composition of the substance, and as means of examining its state of combination when dissolved. By varying the strength of the solution at each of the metals separately, a curve of change of potential was obtained for each positive metal, but not for every negative one.

"Influence of the Chemical Energy of Electrolytes upon the Minimum-point and Change of Potential of a Voltaic Couple in Water." By Dr. G. Gore, F.R.S.

By means of a zinc-platinum voltaic couple in distilled water, with its electromotive force balanced by that of a suitable thermo-electric pile[1] (Birm. Phil. Soc. Proc. vol. iv. p. 130), the effect of several groups of chemical substances upon the potential of the couple was examined. Measurements were made of the electromotive forces of a series of strengths of solution of each substance, and the results are given in a series of tables.

The minimum proportions of substance required to change the potential of the couple in water were as follows:—

Potassic	iodate,	between	1 in	443	and	494
,,	bromate	,,	1 ,,	344	,,	384
,,	chlorate	,,	1 ,,	221	,,	258
,,	iodide	,,	1 ,,	15,500	,,	17,222
,,	bromide	,,	1 ,,	66,428	,,	67,391
,,	chloride	,,	1 ,,	695,067	,,	704,540
Iodine . . .		,,	1 ,,	3,100,000	,,	3,521,970
Bromine . . .		,,	1 ,,	77,500,000	,,	84,545,000
Chlorine . . .		,,	1 ,,	1,264,000,000	,,	1,300,000,000

On comparing these numbers we find that the proportion of substance required to upset the voltaic balance was largest with the oxygen salts, intermediate with the haloid ones, and least with the free elementary halogens. It was smaller the greater the degree of chemical energy of the substance; thus it was about 400 times less with chlorine than with iodine. And it was smaller the greater the degree of freedom to exert that energy; thus it was about 5,416,000 times less with free chlorine than with potassic chlorate, or 1,570,000 times less than with the combined chlorine of the chlorate, and about 185 times smaller than with potassic chloride, or 88 times less than with the combined chlorine of that salt.

The order or curve of variation of potential by uniform increase of strength of the solution was different with each substance, and was apparently characteristic of the body in each case. A great number of such representative curves might be obtained with a zinc platinum or other voltaic couple in different electrolytes.

June 21.—"Further Researches on the Physiology of the Invertebrata." By A. B. Griffiths, Ph.D., F.R.S. (Edin.), F.C.S. (Lond. and Paris), Principal and Lecturer on Chemistry and Biology, School of Science, Lincoln; Member of the Physico-Chemical Society of St. Petersburg. Communicated by Sir Richard Owen, K.C.B., F.R.S.

I. *The Renal Organs of the Asteridea.*

The digestive apparatus of *Uraster rubens* (one of the Asteridea) is briefly described as follows:—The capacious mouth, found upon the oral side, leads into a short œsophagus, which opens into a wider sacculated stomach with thin distensible walls. There are five large stomach sacs; each of these is situated in radial position and passes into the base of the corresponding ray. Each sac or pouch is kept in its place by two retractor muscles fixed to the median ridge of the ray, which lie between the two ampullæ or water-sacs. Passing towards the aboral side, the stomach forms the well-known pentagonal "pyloric sac." The pyloric sac gives off five radial ducts, each of which divides into two tubules bearing a number of lateral *pancreatic* follicles, whose secretions are poured into the pyloric sac and intestine. The author has proved the nature of this secretion to be similar to the pancreatic fluid of the Vertebrata (Edinburgh Roy. Soc. Proc., No. 125, p. 120). Recently, the secretion found in the five pouches of the stomach (of *Uraster*) has been submitted to a careful chemical and microscopical examination. With a quantity of the secretion uric acid crystals were extracted by the same methods as described in his previous papers (Proc. Roy. Soc., vol. xlii. p. 392, vol. xxxviii. p. 187).

The tests proved the entire absence of urea in the secretion under examination. No guanin or calcium phosphate could be detected in the secretion, although the author has found the latter compound as an ingredient in the renal secretions of the Cephalopoda and the Lamellibranchiata (Edinburgh Roy. Soc. Proc., vol. xiv. p. 230).

[1] This instrument i manufactured by Messrs. Nalder Brcs., Horseferry Road, Westminster.

From this investigation, the isolation of uric acid proves the renal function of the five pouches of the stomach of the Asteridea. These pouches are the homologues of the organs of *Bojanus* and nephridia in the Mollusca, the green glands of the Crustacea, and the segmental organs of worms.

II. *The Salivary Glands of* Sepia officinalis *and* Patella vulgata.

The author has already made a complete study of the nephridia and the so-called "livers" in both these forms of the Invertebrata (see the memoirs, *loc. cit.*). Since then he has studied the chemico-physiological reactions of the secretion produced by the salivary glands of the cuttle-fish and the limpet; these organisms representing two important orders of the Mollusca.

1. *Sepia officinalis.*

There are two pairs of salivary glands in *Sepia officinalis*. The posterior pair, which are the largest, lie on either side of the œsophagus. The secretion of the posterior glands is poured into the œsophagus, while the secretion of the smaller anterior pair of glands passes directly into the buccal cavity. This secretion was tested by similar reactions to those described in a former paper (Edinburgh Roy. Soc. Proc., vol. xiv. p. 230) and with similar results.

There is much in favour of the supposition that the *diastatic ferment* found in these secretions is produced as the result of the action of nerve-fibres (from the inferior buccal ganglion) upon the protoplasm of the epithelium cells of the glands.

The author intends to examine various organs in other genera and species of the Decapoda; especially those inhabiting the Japanese seas.

2. *Patella vulgata.*

The two salivary glands of Patella are well-marked and situated anteriorly to the pharynx, lying beneath the pericardium on one side, and the renal and anal papillæ on the other. They are of a yellowish-brown colour and give off four ducts. The secretion of these glands was examined by the same method applied to the salivary glands of *Sepia officinalis*, and with similar results.

The following table represents the constituents found in the salivary secretions of the two orders of the Mollusca aleady investigated:—

	Cephalopoda.	Gasteropoda.	
	(*a*) Dibranchiata.	(*a*) Pulmogasteropoda.[1]	(*b*) Branchiogasteropoda.
Soluble diastatic ferment	present	present	present
Mucin	present	...	present
Sulphocyanates	present	?	present
Calcium phosphate..	present	?	present

From these investigations, the salivary glands of the Cephalopoda and Gasteropoda are similar in physiological function to the salivary glands of the Vertebrata.

III. *The "Liver" of* Carcinus mœnas.

The "liver" of *Carcinus mœnas* consists of two large glands on each side of the stomach, and extending the whole length of the cephalo-thorax. These organs are of a yellow colour, and consist of numerous cæcal tubes arranged in tufts which are easily seen in a dissection beneath the surface of water.

The secretion of the so-called "liver" of *Carcinus mœnas*, when freshly killed, gives an acid reaction.

From the reactions detailed in the paper the conclusion to be drawn is that the so-called "liver" of *Carcinus mœnas* is pancreatic in function, *i.e.* its secretion is more like the secretions of the pancreas of the Vertebrata than those of a true liver.

Some biologists look upon the vertebrate liver, pancreas, and salivary glands as differentiated bodies of an original pancreas of the Invertebrata. But have not very many forms of the lower animals similar salivary glands to those found in the Vertebrata? And is not the so-called "liver" of the Invertebrata a true pancreas capable of producing the same chemical and physiological reactions as the pancreas of higher forms?

[1] Edinburgh Proc. Roy. Soc., vol. xiv. p. 236.

Physical Society, June 23.—Prof. Reinold, F.R.S., President, in the chair.—The following communications were read:—The photometry of colour, by Captain Abney, F.R.S. This relates to the measurement of light reflected from coloured surfaces and pigments as compared with the quantity reflected from white or black. The apparatus used in the investigation consisted of a spectroscope and camera similar to those used by the author for the production of a patch of monochromatic light, and a small shadow photometer served for the measurement. The screen was made of two parts—one the colour to be tested, and the other white or black according to the standard being used; and the stick was arranged so that the shadows fell near the junction of the two parts. Light reflected from the surface of the first glass prism served to illuminate one shadow; and for the other, monochromatic light of any desired colour could be used. The intensities were adjusted to equality by cutting off more or less of the stronger light by means of a revolving wheel with adjustable sectors, the opening of the sectors being a measure of the luminosity of the pigment. In another arrangement a double-image prism was used to separate the spectrum into two parts. Monochromatic light from one part passed direct to the screen through sectors in a rotating wheel, and monochromatic light from the other spectrum was reflected on the screen at a sufficient azimuth to give a separate shadow, by means of two total reflection prisms. The losses by reflection were allowed for by observing the position of the adjustable sectors required to give equal intensities on a white screen. From the results obtained "colour curves" can be plotted for different pigments, &c., and templates constructed which, when rotated in the path of a spectrum, reproduce the corresponding colour. Carmine, sky-blue, and gold were thus reproduced. By means of templates constructed from "colour curves" any colour may be reproduced at any future time. In course of the experiments many interesting observations on colour-blindness have been obtained by the author and General Festing, some of which were described. A question was asked as to whether it was possible to reproduce any given colour, for no two arc lights could be expected to give exactly equal intensities in all parts of the spectrum. Dr. Thompson requested information regarding the effect of absorption by the different thicknesses of the prism through which the light passed, and thought the results obtained might be different if prisms of other materials were used. The fact mentioned in the paper as to the sky being greenish is well known to artists, who usually mix cobalt blue with yellow to produce the required tint. Dr. Thompson also reminded the members of an experiment he brought before the Society some years ago, in which grass seen through a solution of permanganate of potash appears bright crimson when compared with red colours seen through the same solution. In reply, Captain Abney said that colours could be imitated whatever the source used to produce the spectrum, for the resulting colour is the same as that seen when the "original" is viewed by light from that source. Regarding absorption, &c., by the prism, he did not think any appreciable difference was produced, for the results obtained when using the recomposed spectrum as white light were the same as those got by using light reflected from the surface of the first prism. In conclusion, he directed the attention of physicists to Lord Rayleigh's papers on sky colours, &c., published in the *Phil. Mag.*, which would well repay very careful study.—Note on continuous current transformers, by Prof. S. P. Thompson. Two classes of transformers are considered, viz. *motor-generators* and *commuting transformers*, in which a two-circuit armature is fixed in a revolving magnetic field. Such a field may be produced by using a fixed gramme ring as the field-magnet, and rotating the brushes round its commutator. The formulæ obtained apply equally to both classes. If c_1 c_2 be the numbers of primary and secondary wires on outside of armature; E_1 E_2, e_1 e_2, i_1 i_2, r_1 r_2, the E.M.F., potential difference at terminals, currents, and resistances of primary and secondary respectively, then it is shown that $e_2 = ke_1 - (r_2 + k^2r_1)i$, where $k = \frac{c_2}{c_1}$, which is called the "co-efficient of transformation." Thus the potential difference is the same as if the dynamo part had its resistance increased by k^2r_1. As the currents in the primary and secondary are in opposite directions, the effective self-induction will be very small, hence such machines can be run with little or no sparking. In a previous paper by the same author, similar properties as regards self-induction and resistance were shown to exist in alternating current transformers. From the above equation it is evident that a motor-generator cannot be made to give constant potential when supplied at constant potential except when the internal resistances are very small; but by over-compounding the distributing dynamo the desired result may be obtained. Mr. Kapp agreed with the author as regards motor-generators running with little sparking, but thought the great difficulty in using them commercially would be in preserving the insulation between the circuits, if anything like 2000 volts were used in the primary. He also mentioned the method of producing a rotating field by alternating currents, recently described by Prof. Ferraris and Mr. Tesla, and thought it would be preferable to the one devised by the author of the paper. In reply, Dr. Thompson said that insulation could be easily maintained between the core and windings of brush armatures, and saw no reason why it should present very serious difficulties in continuous current transformers.—On an optical model, by Prof. A. W. Rücker, F.R.S. The model exhibited and described is to illustrate the character of the vibrations in a crystal cut parallel to the axis, when plane-polarized light is incident upon it. A rectangular glass box represents the crystal, and glass plates placed at short distances from each end imitate crossed Nicols. A rod, carrying coloured circular and elliptical rings and straight bars, passes along the axis of the box. These rings are intended to indicate the character of the vibration at the different points at which they are placed. The length of the crystal is supposed to be such that plane-polarized red rays emerge plane-polarized in the initial plane after being successively plane, elliptical, circular, elliptical plane, elliptical circular, elliptical and plane-polarized within the crystal. All the light is quenched by the analysing Nicol. Supposing light of greater frequency (say green) to be used, another rod with green ellipses, &c., is placed in the box, and illustrates that such light emerges elliptically polarized, one component only of which is stopped by the analyzer. This shows how plane-polarized white light, when passed through crystals placed betwen Nicols, may become coloured.—On a new barometer, by Mr. T. H. Blakesley. A uniform glass tube is sealed at one end and a thread of mercury introduced, inclosing a quantity of air. An observation is taken by noting the volumes, A and B, of the inclosed air (as indicated by the divisions on the scale) when the tube is placed vertically with its closed and open ends upward respectively. The height, H, of the barometer is given by the formula $H = \frac{A + B}{A - B} l$, where l is the length of the mercury column in the tube. For convenience, l is made 10 inches. The whole instrument is very portable, weighing only 6 ounces, and measuring about 18 inches long.—In the absence of the author, a paper on the existence of an undulatory movement accompanying the electric spark, by Dr. Ernest H. Cook, was taken as read. When sparks pass between two points placed above a plate on which some powdered substance has been scattered, the particles arrange themselves in circular lines approximately concentric with the projection of the middle line joining the two points. The proximity of the lines is found to be very nearly constant for the same powder, independent of the intensity of the spark used, or the material of the plate. Different powders give different numbers of lines per inch, and mixtures, numbers between those corresponding to their constituents. A great number of substances have been tried, giving numbers between forty and eighty-eight per inch. These extreme numbers were obtained for chalk and silica respectively. The author has found no satisfactory hypothesis by which to explain the results. A number of photographs accompany the paper, showing the character of the figures produced. At the meeting, an apparatus made by the late Dr. Guthrie was exhibited, with which similar figures to those described in the paper could be obtained. It consists of a shallow elliptical dish covered by a glass plate. Sparks are passed between two small knots across one focus, and powder, sprinkled on the bottom, forms into circles about the other focus.

Anthropological Institute, June 26.—Francis Galton, F.R.S., President, in the chair.—Mr. Arthur S. Burr exhibited a collection of pottery and other objects from recent excavations in New Mexico.—Mr. H. O. Forbes exhibited a series of photographs taken by him in New Guinea.—A paper on the Nicobar Islanders, by Mr. E. H. Man, was read. Mr. Man has been resident at the Nicobars for periods amounting in all to about 7 years, viz., 1871–88; during that time he has prepared a vocabulary containing 6000–7000 words, and he has thus been in a position readily to make inquiries from the natives on the various points of ethnological interest connected with their constitution and their culture, and to substantiate from a variety of independent sources all the information he obtained. After giving a description of the islands and sketch of their history, Mr. Man proceeds, working on the lines laid down in the Anthropological Notes and Queries, to a careful consideration of the constitution of the Nicobarese, which he prefaces with an outline of certain facts and ethnic characteristics in support of the racial affinities of the Nicobarese with the Indo-Chinese races. From measurements taken of 150–200 individuals at the different islands, Mr. Man gives the average height of the Nicobarese men as $5\frac{3}{4}$, and of the women as 5 feet, a result which disproves the statements of earlier writers regarding the disproportion which exists between the sexes in respect of size. The coloration of the skin pigment of the face, chest, back, arms, and thighs is found to differ in a more or less marked degree in each individual; the two former are usually of a distinctly lighter shade than the last three. Another error needing correction is the assertion that these people can carry without any trouble 200 cocoa nuts, or 5 cwts., whereas it appears that in spite of their undoubtedly fine physical development the maximum load which a Nicobarese can carry may be reckoned as from 160–180 lbs. In the absence of statistics it is difficult to speak with certainty, but from personal observations extending over 17 years it would seem that the average length of life among these islanders is higher rather than lower than it is among the natives of the adjacent continents: the extreme limit of life actually noted is a little over 70, and 80 may be regarded as the maximum ever attained. With reference to the numerical strength of the aboriginal population, a census taken by Mr. Man a year or two ago proves that nearly half the population of the group is contained in Car Nicobar, where a decided increase is taking place, as is also the case at Chowra Teressa and Bompoka. In the central and southern portions of the Archipelago the small ratio of the juvenile element points, however, to a diminution in those islands of the number of inhabitants. It is satisfactory to learn that, though not entirely exempt from the evils which seem inseparably connected with advance in civilization, it does not appear that the Nicobarese have suffered either physically or morally from their contact with Europeans during the past 19 years.

Entomological Society, July 4.—Dr. D. Sharp, President, in the chair.—Mr. Enock exhibited male and female specimens of a spider received from Colonel Le Grice, R.A., who had captured them at Folkestone on May 27 last. They had been submitted to the Rev. O. Pickard-Cambridge, F.R.S., who identified them as *Pellenes tripunctatus*, a species new to Britain. Mr. Enock also exhibited specimens of *Merisus destructor* (Riley), an American parasite of the Hessian fly.—Mr. Wallis-Kew exhibited larvæ of *Adimonia tanaceti* found in Lincolnshire feeding on Scabious.—Mr. Porritt exhibited a number of specimens of *Arctia mendica*, bred from a batch of eggs found last year on a species of *Rumex* at Huddersfield. Mr. Porritt said that this species, in the neighbourhood of Huddersfield, was often more spotted than the typical form, but he had never before seen anything approaching in extent the variation exhibited in these bred specimens. Out of forty-four specimens not more than eight were like the ordinary type of the species.—Mr. M'Lachlan exhibited specimens of *Palingenia longicauda* received from Rotterdam.—Mr. Jacoby exhibited the following species of Phytophagous Coleoptera from Africa and Madagascar, recently described by him in the Transactions of the Society, viz.:—*Lema laticollis*, *Cladocera nigripennis*, *Oedionychis madagascariensis*, *Blepharida intermedia*, *B. nigromaculata*, *Chrysomela madagascariensis*, *Sagra opaca*, *Blepharida ornaticollis*, *B. laterimaculata*, *Mesodonta submetallica*, *Schematizella viridis*, *Spilocephalus viridipennis*, *Apophylia smaragidipennis*, *Aethonea variabilis*.—M. Alfred Wailly exhibited a large number of species of Lepidoptera and Coleoptera, recently received by him from Assam, from the West Coast of Africa, and from South Africa. He also exhibited eggs and living larvæ of *Bombyx cytheræa*, and made remarks on the life-history of the species.

Mineralogical Society, June 28.—Prof. Jas. Geikie, F.R.S., in the chair.—The following papers were read:—A mangano magnesian magnetite, by Prof. A. H. Chester, Hamilton College, U.S.A.—The distribution and origin of the mineral albatile in Ross-shire, by Hugh Miller, F.R.S., of H. M. Geol. Survey.—Elaterite, a mineral tar in old red sandstone, Ross-shire, by Mr. W. Morrison, Dingwall Academy.—These papers were accompanied by various analyses, by Prof. J. Macadam.—The rock-forming feldspars and their determination, by Mr. Alex. Johnstone, and A. B. Griffiths, F.R.S.E.—A Scottish locality for boruite, with analyses by Prof. Macadam, by Rev. W. W. Peyton.—Minerals of the Treshinish Isles, by Prof. Heddle.—On the zeolites of rye water, Ayrshire, by Prof. Heddle.—Prof. Macadam communicated various analyses of coals, of head dies, and of diatomite.—Minerals were exhibited by the Duke of Argyll, Dr. Balfour, Prof. Macadam, Mr. Peyton, and Dr. Black.

PARIS.

Academy of Science, July 9.—M. Janssen, President, in the chair.—On cyclones, by M. Mascart. Referring to M. Faye's last communication, the author accepts as a concession the remark that at all events in fixed depressions currents arise about the periphery, which have a more or less *convergent* tendency. He also quotes the full text from Mohn's work, showing that this meteorologist admits an *ascending* motion in tropical cyclones, and is consequently opposed to M. Faye's theory.—On the figure of the earth, by M. H. Poincaré. The object of these calculations is to ascertain whether it be possible to find a law for the varying density in the interior of the globe which shall satisfy at once (1) Clairaut's equation; (2) the observed value $\frac{1}{293}$ of the flattening; (3) the observed value 305·6 of the constant of the precession. The conclusion arrived at is that no hypothesis on the law of densities will satisfy these values.—The number of centenarians in France according to the census of 1886, by M. Emile Levasseur. Of the 184 returned as centenarians, 101 are shown to have been classed in this category by error. For 67 of the others no documentary evidence was forthcoming, leaving 16 whose claim to the honour appeared to be fairly well established. Joseph Ribas, the oldest, was born at San Estevan de Litera, in Spain, on August 20, 1770, and was still living at Tarbes in 1888, and is consequently now close upon 118 years old. The greater proportion belong to the south-western provinces, and as far as can be judged from available data there is no reason to suppose that they are either more or less numerous now than in former times.—Formula for the calculation of longitudes by means of chronometers, by M. Caspari. By the formula here worked out the author has determined a correction of 2″·45 for Hai-Phong, Tongking, which differs little from the 2″·93 which M. La Porte has recently obtained by the telegraph.—On the position of Timbuktu, by M. Caron. The approximate position of this place is found to be 16° 49′ N. lat.; 5° 12′ W. long., which differs considerably from Barth's 18° 3′ 45″ N.; 4° 5′ 10″ W.—On the determination of the constants and dynamic coefficient of elasticity of steel, continued, by M. E. Mercadier. These researches lead to the general conclusion that the strictly *elastic* properties shown in vibratory or other phenomena of a transitory nature should be carefully distinguished from the physical properties accompanied by permanent distortion. The former vary but slightly, the latter considerably in the different kinds of hard and soft steel.—On the propagation of the sound produced by firearms, by M. de Labouret. The apparent increase of velocity is explained with M. Journée on the hypothesis that the projectile at each successive instant of its motion through space is the centre of a fresh concussion. The series of observations here recorded gives results for the velocity of the sound, which are mainly in accordance with the theoretic calculations.—A new method for the measurement of the electric resistance of saline solutions, by MM. E. Bouty and L. Poincaré. A process is described by means of which the difficulties may be overcome, which are met with in the application of ordinary methods to salts in solution at temperatures ranging from 300° to 500° C. The results agree sufficiently well with those previously obtained by M. Foussereau by a different process for temperatures from 329° to 355° C.—Actino-electric researches, continued, by M. A. Stoletow. The author here describes an apparatus constructed by him for the purpose of studying the actino-electric currents in diverse gases and vapours, and under diverse pressures. At ordinary pressure he finds little difference between dry air, moist air, and hydrogen, while for carbonic acid the current is nearly twice as strong.—On some compounds of yttrium, by M. A. Duboin. To the few combinations of yttria hitherto obtained by the dry process the author here adds the silicate of yttria, gadolinite of pure yttria, and the crystallized oxide of yttrium.—Syntheses by means of cyanacetic ether, by M. Alb. Haller. By the process already described for the preparation of the corresponding benzoyl, acetyl, propionyl, and other ethers, the author has succeeded in obtaining the synthesis of the orthotoluyl, phenylacetyl, cinnamyl, and dicinnamyl cyanacetic ethers.—On the alkaloids of cod liver oil, by MM. Arm. Gautier and L. Mourgues. It is shown that this substance contains several alkaloids, some very active; but the present paper deals mainly with the leucomaines obtained by the authors from the yellowish oils yielded both by the Norwegian and Newfoundland cod.—On paradoxal deafness and its treatment, by M. Boucheron. This curious affection, the paracousia of Willis, in which the patient is deaf to words uttered in the silence of a room, but not in a noisy street, is here carefully studied and found to be a variety of otopiesis.—A fishing basket for deep sea hauls, and an electric apparatus for illuminating the oceanic depths are described and illustrated, the former by Prince Albert of Monaco, the latter by M. P. Regnard.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Tabular List of Australian Birds: E. P. Ramsay (Sydney).—Flora of the North-East of Ireland: Stewart and Corry (Belfast Naturalists' Field Club).—Lewis's Medical and Scientific Library Catalogue (Lewis).—Charles A. Gillig's Tours and Excursions in Great Britain: S. F. Smart (United States Exchange).—Numerical Examples in Practical Mechanics and Machine Design: R. G. Blaine (Cassell).—Austrian Health Resorts: W. F. Rae (Chapman and Hall).—An Illustrated Manual of British Birds, Part 4: H. Saunders (Gurney and Jackson).—Euclid's Method or the proper way to treat on Geometry: A. H. Blunt.—Experimente über die Bacterienfeindlichen Einflüsse des Thierischen Korpers: Dr. G. Nuttall.—Annals of Botany, vol. 2, No. 5 (Frowde).—Annalen der Physik und Chemie, 1888, No. 8*b* (Leipzig).—The Auk, vol. v. No. 3 (New York).—Notes from the Leyden Museum, vol. x. No. 3 (Leyden).—Studies from the Biological Laboratory, Johns Hopkins University, vol. iv. No. 4 (Baltimore).

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THURSDAY, JULY 26, 1888.

SCIENTIFIC ASSESSORS IN COURTS OF JUSTICE.

PUBLIC attention has lately been called, by various incidents, to the system under which the trial of scientific cases, and especially those in which the respective rights of rival inventors are involved, is at present conducted in courts of justice. Last week Mr. Justice Kay decided a case in which the Edison-Swan United Electric Company were plaintiffs, which lasted twenty-one whole days, or about one-tenth of the legal year; and it is possible that it may occupy very much more time in the Court of Appeal, where every day is equivalent to three days in ordinary courts, because three judges sit here, and again in the House of Lords, if the litigants decide to proceed to extremities, as they very frequently do in cases of this magnitude and importance. At the same time, Mr. Justice Kekewich was engaged in trying another large electric patent case; the Court of Appeal had a similar case occupying it for several days, in the course of which Lord Justice Cotton, who presided, animadverted in somewhat severe terms on the length to which such cases are allowed to run. His Lordship, with the concurrence of the two Lords Justices who sat with him, attributed this to the manner in which counsel spun out their arguments, and urged more brevity and conciseness. Whatever may have been the circumstances in the case to which the Lord Justice adverted, it is certain that the addresses of the eminent counsel engaged in the Edison-Swan case were not responsible for the twenty-one days which it occupied before Mr. Justice Kay—not including seven or eight days for experiments;—by far the greater part of this time was occupied in hearing the contradictory and conflicting evidence of a score of scientific men, many of the greatest eminence, on the points in dispute between the parties. With these points we have absolutely nothing to do here. It is sufficient to say that the case involved the investigation and decision of matters of the utmost complexity respecting the applications of recent electrical discoveries to lighting, and also some obscure questions in the history of these applications. All these exceedingly complicated and difficult questions were tried before an eminent judge, who, as he said himself at the commencement of his judgment, "has not had the requisite scientific training." It was, in fact, necessary to begin by instructing the judge in the elements of electrical science; the propositions which scientific men accept as truisms, or as common knowledge in discussions amongst themselves, had here to be gone over *ab initio* in order to inform the judge's mind respecting the A B C of the problem which he had to solve. As to Mr. Justice Kay's success in the task of acquiring this information, we are quite willing to accept the opinion of one of the leading electrical papers, which says that "the manner in which the judge grasped the bearing of the technical evidence has been the subject of remark amongst everyone present in court."

We have no doubt that a judge, with his trained and experienced mind, would make a very apt pupil; but the process of obtaining knowledge, even in such cases, is not always a very smooth or pleasant one. It is trying to the calmest and most equable mind to be compelled constantly to reconsider information acquired with care and difficulty, to find the views inculcated by one eminent man of science totally contradicted by another equally eminent. It is not surprising that in the maze of conflicting opinions Mr. Justice Kay was unable of his own knowledge to find his way. We reproduced a painful incident in our columns at the time it occurred, with the view of exhibiting to our readers one of the evils of the present system for trying complicated cases, although the circumstance that the case was then *sub judice* precluded us from offering any comment on it. We did this with a view of suggesting, also, that whoever was wrong—the judge or the expert witness—a matter which it did not concern us to inquire into—it is not in the interests of science that scientific men of reputation should put themselves in the position of advocates, thus rendering such treatment possible. Judges are only human, and, so long as men with no scientific training are left to bear unaided the burden of trying cases like that in which the Edison-Swan Company were plaintiffs, with their conflicting evidence, their authoritative opinions one way flatly contradicted by equally authoritative opinions the other, their masses of facts on subjects unfamiliar to the judge, so long must scientific men who are concerned in such cases expect unpleasant *rencontres* of this description either with the perplexed and worried judge or with the counsel on one side or the other. To be a witness at any time in a court of justice is not pleasant; it is an experience we have all to go through, at one time or another, with more or less resignation, supported by the consciousness that we are doing our duty as citizens and aiding the course of justice. But to be a witness in a scientific case on a subject to which you have devoted your life, and with regard to which you have obtained a position of authority, it may be, amongst your fellows who are, of all men in the world, the most capable of judging, and to be compelled to undergo cross-examination of the usual type at the hands of a gentleman who made up his few meagre and jejune facts on the subject from his brief the night before, and who will forget all he knew by the next night—this is hard indeed. But we cannot see how men of science can get out of these inconveniences and unpleasantnesses any more than any other class of the community, so long as the trials of these cases are in the hands of men who know nothing of science, and who have no regular and systematic means of obtaining aid—judicial aid, that is—from those who do.

Lawyers appear to be as discontented with the present system as men of science have reason to be. The principal legal paper went so far the other day as to suggest the formation of a special court for the trial of patent cases. These have increased so much of recent years, consequent on the vast increase of scientific discoveries and their practical applications to the business of life, that the old machinery is no longer adequate to deal with the new situation. Other litigants suffer in their business and pockets; the courts become congested, and the judicial business of the country is seriously impeded. The present arrangements can be satisfactory to no one, except, perhaps, to the few lawyers who are making their fortunes by them. To our minds, no very revolutionary process is needed to render the courts equal to the work. A judge's time in such cases is mainly lost in acquiring the information necessary to enable him to understand the points at issue. On a famous occasion it was said that we should have to educate our masters; litigants in patent cases have to begin by educating their judges. During the course of the Edison case the judge found the evidence on one important point so conflicting, that he suggested the propriety of having experiments made by scientific men on both sides, in the presence of some disinterested man of science, who should report to him on the result. The suggestion was followed: Prof. Dewar and Dr. Hopkinson carried out the experiments on one side, Mr. Crookes and Prof. Silvanus Thompson on the other, the President of the Royal Society being the umpire. In the course of the judgment Mr. Justice Kay acknowledged that Prof. Stokes's report made that "obvious," which he could not previously understand. Prof. Stokes, in fact, was called *in quâ* that particular point as an assessor to the Court. Suppose he had been called in at the beginning, and had sat all through the case, how much time, labour, and unpleasantness would have been spared! How rapidly he would have enabled the judge to narrow down the points at issue, and to understand them! And if Prof. Stokes had been aided by some other independent and qualified man of science, how much sooner and more satisfactorily the whole business would have been concluded. We want, in fact, sworn scientific assessors in courts of justice to aid unscientific judges in arriving with reasonable despatch at reliable conclusions on matters which demand scientific knowledge. Patent cases invariably turn on the construction of a written document—namely, the specification—and this, like all other documents, is a matter for the Court, guided by the rules which apply generally. "But," says Lord Chancellor Chelmsford, "if the terms used require explanation as being terms of art or of scientific views, explanatory evidence must be given, and with this aid the Court proceeds to the office of construction." Now there are two processes already in operation in the High Court of Justice, which it seems to us might well be applied to the determination of these complicated scientific cases, or rather by which disinterested and unbiassed scientific aid might be given to the Court in the determination of cases such as the Edison and Swan case. One is by the system of "referring," the other by assessors. Reference is an every-day proceeding in the Courts in complicated cases. By the 57th section of the Judicature Act of 1873, the Courts are empowered "in any cause or matter requiring any prolonged examination of documents or accounts, or any scientific or local examination which cannot, in the opinion of the Court or a judge, conveniently be made before a jury, or conducted by the Court before its ordinary officers, the Court or judge may at any time, on such terms as may be thought proper, order any question or issue of fact, or any question of account arising therein to be tried either before an official referee, or before a special referee to be agreed on between the parties." The referee or umpire is armed with proper powers, and in due time reports to the Court, which thereupon proceeds to adjudicate upon the case, having got rid of a mass of technical details with which it was incompetent to deal by the instrumentality of the referee who was quite competent. Doubtless it was in pursuance of this power that Mr. Justice Kay referred a portion of the recent case to Prof. Stokes; but suppose the whole matter, the issues having been narrowed down to their real limits, had been referred at the beginning to Prof. Stokes, aided if necessary by some other independent expert, to report the result to the Court, about twenty days of valuable public time would have been spared, and in the end the decision would have commanded a confidence which the judgment of a wholly unscientific judge, however acute, cannot be expected to receive.

But it appears to us that the system of assessors, who sit with the judge in court, and who aid him with their scientific knowledge and experience, would be even more satisfactory. It is in daily use in Admiralty cases. The practice is thus laid down in Messrs. Williams and Bruce's "Admiralty Practice," second edition, p. 441:—"If the questions in the cause depend upon technical skill and experience in navigation or other nautical matters, the judge is usually assisted by two of the Elder Brethren of the Trinity House of Deptford Strond, who sit with him as assessors, and who, at the request of the judge, after hearing all the evidence on each side, advise him on all questions of a nautical character. But in all cases it is with the judge alone that the decision rests." An eminent judge of the Privy Council summed up the duty and position of assessors in these words:—"He (the judge) is advised and assisted by persons experienced in nautical matters; but that is only for the purpose of giving him the information he desires upon questions of professional skill; and having got that information from those who advise him, he is bound in duty to exercise his own judgment. . . . . The assessors merely furnish the materials for the Court to act upon." But what this comes to in practice, circumscribed though the duties of the assessors are in theory, we learn from a remark of the eminent Admiralty judge, Dr. Lushington: "I never yet pronounced a single decree, when I was assisted by Trinity Masters, in which I was not perfectly convinced that the advice they gave me was correct." The presence of the Trinity Masters is secured by either party filing a *præcipe* praying for their attendance. And now all Admiralty cases, in whatever Court, may be tried with the aid of nautical assessors, when this is considered desirable.

Although this system is, as a rule, confined to Admiralty cases in practice, all Courts are empowered to call in the aid of assessors, for by the 56th section of the Judicature Act of 1873, the High Court or Court of Appeal may in any cause or matter in which it thinks it expedient so to do, call in the aid of one or more assessors specially qualified, and try and hear such cause or matter wholly or in part with their assistance. If Prof. Stokes and some other qualified expert had sat with Mr. Justice Kay during the hearing of the recent lighting case, it is scarcely probable that it would have lasted twenty-one days, or that various unpleasantnesses inseparable from the hearing of such a case, which was nothing if not scientific, by a conscientious but unscientific judge, would not have been avoided. There are no reasons why a judge should not be aided in cases of this technical description by scientific experts, as Admiralty judges are by nautical experts; there are a great many why he should. The orderly and effective administration of justice, the weight which should be attached to judicial decisions, the economy of public time, and, we would add, the self-respect of scientific men, and the best interests of scientific discovery, all call loudly for some such reform as that here suggested.

LANGLEY'S NEW ASTRONOMY.

The New Astronomy. By Samuel Pierpoint Langley, Ph.D., LL.D. Illustrated. (Boston: Ticknor and Co., 1888.)

PROFESSOR LANGLEY'S beautiful book does not appeal merely to the intellect. The senses have their share in the gratification its perusal affords. Every turning of a page is a conscious luxury. Each touch of the paper, in which the thickness of vellum is combined with the polish of satin, flatters the finger-tips with a bland caress. In texture, it compares with the paper on which ordinary work-a-day scientific treatises are printed as does a velvet-pile with a Kidderminster carpet. The binding is in a corresponding style of lavish magnificence. The illustrations have obtained the last perfection of finish.

Yet the excellence of their execution is for the most part secondary to their intrinsic merit. Needless to say that photographs figure largely among them. There is a capital sunspot series by Rutherford; there are specimens of Pickering's stellar spectra; besides several coronal autographs, Mr. Common's inimitable Orion nebula, and Rutherford's scarcely yet surpassed print of the moon. Among visual delineations, we meet Bond's admirable views of Donati's comet, Trouvelot's elaborate Saturn, De la Rue's well-known Jupiter, above all, Prof. Langley's own exquisite solar drawings. The surface of the sun has probably never been so perfectly seen as by him; it has certainly never been depicted with such a wealth of trustworthy detail. Some insight into one of the sources of his success is afforded by the following paragraph (p. 17):—

"The surface of the sun," he tells us, "may be compared to an elaborate engraving, filled with the closest and most delicate lines and hatchings, but an engraving which during ninety-nine hundredths of the time can only be seen across such a quivering mass of heated air as makes everything confused and liable to be mistaken, causing what is definite to look like a vaguely seen mottling. It is literally true that the more delicate features are only distinctly visible even by the best telescope during less than one-hundredth of the time, coming out as they do in brief instants when our dancing air is momentarily still, so that one who has sat at a powerful telescope all day is exceptionally lucky if he has secured enough glimpses of the true structure to aggregate five minutes of clear seeing, while at all other times the attempt to magnify only produces a blurring of the image. This study, then, demands not only fine telescopes and special optical aids, but endless patience."

"Endless patience" is, indeed, a *sine quâ non* in nearly all departments of astronomy; but it is not always associated with the skill of eye and hand witnessed to by the representations before us. Nor could they have been brought to bear without instrumental accessories of a more than commonly high quality. The polarizing eyepiece made at Pittsburgh must be one of the best ever employed to blunt the keen edge of the solar rays. "By its aid," our author remarks, "the eye can be safely placed where the concentrated heat would otherwise melt iron. In practice I have often gazed through it at the sun's face without intermission from four to five hours, with no more fatigue or harm to the eye than in reading a book."

The object of the work before us is to advocate the claim of the "New Astronomy"—the astronomy which studies the constitution of the heavenly bodies, as opposed to that which determines their movements—to a larger share of public interest, sympathy, and benefactions than has hitherto been allotted to it. The appearance of the eight chapters of which it consists in the pages of the "Century" magazine, has doubtless already contributed to promote that end. They are written in an eminently popular style, and with much of that Transatlantic freshness by which many a jaded European palate is enticed to renewed enjoyment of wholesome literary fare. They profess to give only a sketch of the results so far attained; but it is a highly stimulating and suggestive one. Intelligible to all, they should be welcomed by readers of every grade of culture desiring to gain acquaintance, almost without an effort, with some of the most surprising encroachments ever yet made by the agile human mind upon the vast realms of the unknown.

The two most interesting, because the most original chapters in the book, are those dealing with the "Sun's Energy." Here Prof. Langley is more especially at home; his opinions carry all the weight that long meditation and laborious research can give them; yet they are expressed not only without dogmatism, but almost with diffidence. The higher value given to the "solar constant" by his inquiries into atmospheric selective absorption, have naturally obliged him to curtail the "life" of the sun. During no more than eighteen million years can the present rate of radiation—supposing it fed by the shrinkage through gravity of the sun's substance—have been maintained in the past. "We say 'present' rate of radiation," our author continues, "because, so long as the sun is purely gaseous, its temperature rises as it contracts, and the heat is spent faster; so that in early ages before this temperature was as high as it is now, the heat was spent more slowly, and what could have lasted 'only' eighteen million years at the present rate might have actually spread over an indefinitely greater time in the past; possibly covering more than all the æons geologists ask for."

This is of course perfectly true. There can be no reasonable doubt that the sun was, in the initial stages of its career, a comparatively murky luminary, rich in the promise of future possession, but scantily distributing, because scantily supplied from, stores of light and heat strictly tied up against the possibility of premature waste for the benefit of generations to come, its heirs by entail. But has there been no compensatory period of extravagance? Has our sun already passed through its "Sirian" phase—if a Sirian phase be indeed an inevitable "moment" in the existence of every star—or is it yet to come? The question cannot at present be answered; but until it is, estimates of the probable past duration, in its illuminative capacity, of the central body of our system, are evidently illusory. The actual radiation of the sun would be not improbably decupled by the sudden change of its atmospheric and photospheric constitution to that of Sirius or Vega. In other words, the stock of energy now sufficing for the expenditure of ten million years would then be dissipated in one million, with a corresponding abridgment in time of the heating and lighting efficacy thus vastly heightened in intensity. The same *caveat* applies—should it be concluded that the Sirian is a later than the solar stage—to attempts to assign a term for the inevitable exhaustion of the great fountain of vital possibilities. The objection is however evaded by Prof. Langley's statement (p. 100) that, *at the present rate*, "the sun's heat-supply is enough to last for some such time as four or five million years before it sensibly fails. It is certainly remarkable," he adds, "that by the aid of our science man can look out from this 'bank and shoal of time,' where his fleeting existence is spent, not only back on the almost infinite lapse of ages past, but that he can forecast with some sort of assurance what is to happen in an almost infinitely distant future, long after the human race itself will have disappeared from its present home. But so it is, and we may say—with something like awe at the meaning to which science points—that the whole future radiation cannot last so long as ten million years."

Our author is sanguine as to the prospect of economically applying the sun's heat to mechanical purposes. "From recent measures it appears that from every square yard of the earth exposed perpendicularly to the sun's rays, in the absence of an absorbing atmosphere, there could be derived more than one-horse power, if the heat were all converted into this use, and that even on such a little area as the island of Manhattan, or that occupied by the city of London, the noontide heat is enough, could it all be utilized, to drive all the steam-engines in the world" (p. 111). No wonder that, enticed by such calculations, "practical men" should devote attention to this unfathomable source of power; and we may well believe, with Prof. Langley, "that some of the greatest changes which civilization has to bring may yet be due to such investigations."

"Future ages may see the seat of empire transferred to regions of the earth now barren and desolated under intense solar heat—countries, which for that very cause, will not improbably become the seat of mechanical and thence of political power. Whoever finds the way to make industrially useful the vast sun-power now wasted on the deserts of North Africa or the shores of the Red Sea, will effect a greater change in men's affairs than any conqueror in history has done; for he will once more people those waste places with the life that swarmed there in the best days of Carthage and of old Egypt, but under another civilization, where man shall no longer worship the sun as a god, but shall have learned to make it his servant."

In his chapter on "Meteors," our author seems to view with a certain degree of favour the suggestion that some of these small bodies "may be the product of terrestrial volcanoes in early epochs, when our planet was yet glowing sunlike with its proper heat, and the forces of Nature were more active" (p. 193). He does not, however, stop to discuss the difficulties besetting this hypothesis; had he done so, he could scarcely have failed to conclude them insuperable. The resistance opposed by the atmosphere of the earth to the upward flight of projectiles from its surface has, for instance, never been sufficiently taken into account. It is quietly assumed that some unspecified and insignificant addition to the initial velocity needed to secure definitive escape in a vacuum, would have sufficed to overcome atmospheric hindrances; whereas the minimum swiftness at starting in the second case should be at least thrice, or quadruple that in the first. The effectiveness of the air in arresting motion is practically exemplified in the continuous meteoric bombardment against which it forms our sole shield. Yet the projectiles composing it possess far higher velocities than terrestrial volcanoes could, under any conceivable circumstances, be supposed to impart. And the few among them that meet the earth's surface are impelled towards it by gravity after their own movement has been wholly, or all but wholly destroyed. Instances must be very rare in which an aerolite has brought down with it in its fall any portion of its orbital speed. Moreover, our present atmosphere is doubtless rare and shallow compared with its pristine condition; while there is no certainty that volcanic action, of an explosive kind, was ever much more energetic than it now is.

Prof. Langley adopts, or rather admits the "temperature-classification" of stellar objects current at the time when his concluding chapter on "The Stars" was written. It speaks volumes for the rapidity with which the "new astronomy" progresses that, in a few short months, this scheme—to which there were always serious objections—should have fallen obsolete. Mr. Lockyer's recent investigations have at least had the effect of rendering a complete revision of ideas on the subject indispensable. The book with which we are just now concerned professes, however, not even to describe, but barely to mention, the various departments, photometric, spectroscopic, and photographic, of stellar physical astronomy, "on each of which," the author justly remarks (p. 248), "as many books, rather than chapters, might be written, to give only what is novel and of current interest. But these," he adds, "are themselves but a part of the modern work that has overturned or modified almost every conception about the stellar universe which was familiar to the last generation, or which perhaps we were taught in our youth."

An English edition of a work which we can recommend as corresponding with singular felicity and charm to the designs of the writer, is in preparation, and will shortly appear. Some photographs of the moon, too recent to be as yet generally known, will probably replace in it such of Mr. Nasmyth's lunar illustrations as figure in the American edition. A. M. CLERKE.

SOAPS AND CANDLES.

Soaps and Candles. Edited by James Cameron, F.I.C., Analyst in the Laboratory, Somerset House. "Churchill's Technological Hand-books." (London: J. and A. Churchill, 1888.)

THE object of this hand-book, as stated in the preface, is to add to the articles originally published in Cooley's "Cyclopædia" additional information from various scattered sources, so as to present, in as small a compass as possible, information which it is hoped may be found useful to technological students and others interested in the industries described. Compression of bulk being a main object, it is assumed that the reader has some degree of acquaintance with various points connected with theoretical and practical chemistry and certain analytical processes, so that details in such cases may be omitted without interfering seriously with the usefulness of the book. In carrying out the work of compilation, the same necessity for economizing space has rendered imperative considerable care in selecting and "boiling down" the matter, derived from some two dozen different sources in the way of English bibliography, for the most part published within the last few years; amongst which may be more particularly mentioned the works on soap-making, candle-manufacture, and allied industries by Morfit, Kurten, Dussauce, Christiani, Ott, Lant Carpenter, and Watt; and the Cantor Lectures of Field ("Solid and Liquid Illuminating Agents") and of Alder Wright ("Toilet Soaps"). References to Continental literature and patents, though comparatively infrequent, are also to be found at intervals throughout the book.

On the whole, it must be admitted that the author has carried out the work of selection and excision, compilation, abstraction, and general editing with great judgment, and that he has succeeded in getting into very small compass not only a large amount of general information, but also a valuable epitome of most, if not all, of the various advances in manufacture and the additions to scientific knowledge that have been made up to the present date in connection with the industries treated of, comprising not merely the production of soap and candles, but also the intimately associated manufacture of glycerin. This latter is quite a modern offshoot from the parent industries, neither of which, however, can claim as high an antiquity as some of the metallurgical operations; for, whilst the property of certain oils and animal fats to become converted into a saponaceous mass by treatment with the lye of wood ashes was known in the first century in an incomplete way, as evidenced by the writings of Pliny, no authentic information is extant leading to the belief that anything of the nature of true soap was known at any much earlier period; the materials referred to by the Old Testament writers as *borith*, and translated "soap" (or, in early editions, "sope"), appearing to have been simply alkaline matter, without any oil or fatty ingredient combined therewith. On the other hand, the manufacture of candles, *i.e.* a wick surrounded by a *solid* fusible matter capable of combustion under such circumstances like oil in a lamp, does not appear to have been practised among the ancients, lamps burning *fluid* oil being their usual source of artificial illumination: probably torches, or thick wicks impregnated with oil, pitch, &c., and sufficiently stiff to be handled, were the earliest form of candle. Not until the fourth century of our era, however, does this crude device appear to have developed into anything approaching the modern form of candle, wax being then used as the combustible matter in the finer kinds, and tallow or other solid animal fat in the coarser descriptions.

The researches of the yet living M. Chevreul, made in the early part of the present century, cleared up the chemical constitution of oils and fats generally, and largely helped to bring about great improvements both in the manufacture of soap and in that of candles: they demonstrated that oils and fatty matters in general are, for the most part, compounds analogous to mineral salts, being produced by the union of a "fatty acid" and an organic compound of weak basic character, *glycerin*, in the same way that a mineral acid and a strong base or metallic oxide will saturate one another to form a salt of the ordinary type; and that soaps are the alkaline salts of the fatty acids contained in the oil, &c., used, the process of "saponification" being simply the elimination of the organic basic constituent, glycerin, by the more powerful alkali employed, potash usually forming a "soft" soap, and soda a "hard" one. By treating the soaps thus formed with mineral acids, the "fatty acids" are similarly displaced from combination with the alkalies, and substances are thus obtained usually less fusible than the original fatty matter, but, like it, capable of being burnt in conjunction with a wick, and frequently with less liability to smoking and charring the wick. The leading developments of the candle industry thence resulting have accordingly been in the direction of producing the fatty acids by saponification (or cheaper processes substantially equivalent thereto), and expression of the more fluid constituents (usually, though somewhat unsystematically, termed *oleine*), so as to render the solid residue, or *stearine*, of higher melting-point, and therefore better suited to form candles not apt to bend in summer or in hot climates; and the use of mineral solid hydrocarbons (paraffin-wax and allied materials from paraffin-oil, petroleum, ozokerite, &c.) as ingredients in combination with, and sometimes to the exclusion of, the stearines thus formed. The more solid fats (tallow, suet, and certain solid vegetable fats) are naturally the substances most largely employed, as furnishing the greatest yield of solid stearine suitable for candle-making; but several oils and semi-fluid products (like palm and cocoa-nut oils), when chilled and pressed, yield a notable quantity of more solid constituents equally available for the purpose. The fluid fatty acids, or "oleines," obtained as by-products in the candle industry, are either neutralized directly by aqueous caustic alkalies, thus forming soaps, or, according to the recent process of *Radisson*, are fused with caustic alkalies (preferably, but not necessarily, potash), whereby oleic acid becomes converted into solid palmitic acid, of sufficiently high melting-point to be capable of employment for making candles.

For the manufacture of soaps, scarcely any fatty matter, whatever its source or lack of purity, comes amiss; it being of course obvious that the coarser kinds are only available for the cheapest scouring soaps, and that only the better kinds can be employed in the production of superior classes of soaps, especially those intended for toilet soaps of high quality (which term by no means applies to all in the market). Recovered greases from wool-scouring and fulling operations, fœtid animal fats from the carcasses of horses, bones, and by-products of glue-manufacture and tanning, &c., greasy matters extracted from dead cats and dogs netted in the rivers and streams, and even that obtained from the scum of sewage, represent some of the least attractive of the sources of oleaginous matter dealt with by the soap-boiler; whilst more or less damaged or rancid oils, unfit for other use, and "foots" (residues containing much impurity, which separate during the processes of refining various kinds of oils), together with the somewhat impure oily matters obtained by the aid of solvents (*e.g.* carbon disulphide) from the marcs or cakes obtained in olive and seed-oil crushing, cocoa-nut and other rank vegetable oils, and animal tallows, lards, suets, &c., imported from abroad, and obtained by treatment usually of such a nature as to render the product more or less malodorous, represent a better class of raw material, suitable, after more or less purification, for the ultimate production of the ordinary kind of household and laundry soaps. The finest varieties of lard, &c., and purified almond and other comparatively choice vegetable oils, and such like superfine materials, constitute the substances actually used in the manufacture of some of the best varieties of toilet soap, and *supposed* to be employed in the production of all such more delicate varieties.

The author briefly but clearly describes the leading processes and methods by means of which useful and even superior qualities of soaps are manufactured in bulk from the more ordinary materials, and the finer kinds from the choicest sources, usually on a smaller scale. Numerous analyses of various sorts of soaps are quoted, and the methods of production of "filled" (*i.e.* adulterated and watered) soaps, and of the composite scouring materials containing silicate of soda and analogous alkaline compounds together with true soap, are adverted to. It might, perhaps, be considered that sufficient stress has hardly been laid on the enormous extent to which such admixture is sometimes carried on in the case of certain articles still sold under the name of soap. When a scouring material contains only one-seventh of its weight of actual soap (mostly from cocoa-nut oil), and about as much silicate of soda and inert soda salts added to "harden" the mass, the balance being water pure and simple; or when a so-called "toilet soap" contains less than two-fifths of its weight of true soap, and nearly as much water, the balance being simply sugar and a more or less marked excess of corrosive alkaline matter (both calculated to act most injuriously on tender and delicate skins), it would be supposed by many that the limit of honest trading and proper description of quality has been pretty closely approached, if not a long way passed, by describing and selling such articles as "soap" at all. In the description of the manufacture of transparent toilet soaps by the process of solution of previously made soap (mostly yellow or resin soap) in alcohol, the author states that "most makers also add a certain proportion of glycerin." It would be more correct to say that in the great bulk of such soap actually sold a very considerable quantity of *sugar* is present; whilst glycerin, although frequently professedly a constituent, is usually conspicuous by its entire absence from the composition—a difference by no means to the advantage of the consumer, if troubled with a sensitive skin, although not of any great consequence to the fortunate possessor of a stout healthy epidermis not easily affected by external influences.

C. R. Alder Wright.

INDIA IN 1887.

India in 1887. By Robert Wallace, Professor of Agriculture and Rural Economy in the University of Edinburgh. With plates and illustrations. (London: Simpkin, Marshall and Co. Calcutta and Bombay: Thacker, Spink and Co., 1888.)

PROFESSOR WALLACE has evidently thrown his heart as well as his brains into his self-imposed task. He wished to know the effect of his own teaching, and that of the college to which he was attached, upon the development of Indian agriculture—and he went to see for himself. Let us hope that Prof. Wallace will have his reward for so unselfish a motive. The key to his position lies in the fact that Indian Government scholarships have been for many years bestowed at Cirencester upon Indian native graduates who have been selected for this purpose, with a view to their subsequent employment in the Agricultural and Forestry Departments of India. His object, as he himself expresses it, is "to induce the Government to alter its plans as regards the Indian Agriculture Department, and to see that ground which has been lost by inexperienced officers is yet capable of being regained by efforts made in the right direction." Quixotic as any attempt may appear to cause a Government department to alter itself, or to quietly submit to alteration, no doubt the best plan is to appeal to the public, and this is what Prof. Wallace has done. He has, no doubt, to some extent courted contradiction and hostile criticism from those already engaged in agricultural improvement in India. His book is not wanting in denunciation of the existing system, the strength of which lies in the strongly practical bias of the writer, who sympathizes with the farmer and his ways, whether found in the stalwart son of the soil in England or Scotland, or in the ryot of India. Their methods are proved methods, their opinions are the result of thousands of years of mental evolution. Prof. Wallace clearly shows an inherent dislike to that kind of innovation which springs from superficial knowledge gained in one part of the globe and thrust upon those who are engaged under totally different circumstances of soil and climate. He insists most properly, we think, that it is a delusion to imagine that any man, however able, can gain a thorough or adequate knowledge of the science and practice of agriculture in two years. Without in the least detracting from the value of two years spent in study at an agricultural college, he insists that the first step is *the study of native agricultural practices* "by men who have been trained in agriculture from their early youth in this country, and who have subsequently acquired a sound knowledge of the sciences bearing on the subject."

In the same spirit he inveighs heavily against the almost universal employment as model farm managers of men who have had no truly agricultural training, either practical or scientific, and who have no intimate knowledge of the native methods of cultivation. The result of this system has been that "many failures have destroyed the confidence of Government; and anything agricultural, that is now being done, is reduced to the merest *minimum*, with a chance any moment of being utterly abandoned."

While these views are forcibly expressed and abundantly illustrated, Prof. Wallace has not forgotten to widen the scope and interest of his very valuable book by copious information as to the products, the agriculture, the cattle, the instruments of husbandry, the habits, and the customs of India. He has placed on record an immense number of facts which must render his book valuable for purposes of reference as well as interesting to the general reader. With reference to the liberal display of photographic representations, executed by Waterston and Sons, the author looks upon them as instructive rather than artistic. The photographs from which they were taken were executed by himself, often under difficulties, but they are none the less accurate, and therefore trustworthy.

With regard to the present arrangement of the book, the first 300 pages are devoted to descriptive matter relating to the cattle and other domesticated animals, the soils, implements of husbandry, and crops of India. Much of the matter may be left by the busy reader, who will find the special views and conclusions of the author reserved for the concluding chapters.

The book is an honest and able attempt to place the peculiarities of Indian agriculture fairly before the British public, and the views of the author with reference to the best methods for developing the agricultural sources of the Indian Empire will, we hope, receive the attention they deserve.

OUR BOOK SHELF.

Incwadi Yama: or Twenty Years' Personal Experience in South Africa. By J. W. Matthews, M.D. (London: Sampson Low, 1887.)

DR. MATTHEWS left England in 1864, soon after he had taken his medical degree. He settled, in the first instance, at Verulam, in Natal, where he was appointed a district surgeon. Afterwards he became familiar with many different parts of South Africa, and especially with the Diamond Fields, the inhabitants of which twice returned him at the head of the poll to represent them in the councils of their country. He is not a very skilful writer, but any one who will take the trouble to read his long and somewhat elaborate narrative will be rewarded by obtaining a great amount of solid and more or less interesting information. He has naturally much to say about the population of the Diamond Fields, and about the process of diamond mining, and on these subjects he speaks with the authority of one who presents the results of direct personal observation. He has also brought together a good many curious and instructive facts about the native tribes; and his descriptions of scenery, if not brilliant from a literary point of view, at any rate suffice to convey a general impression of some of the districts he has visited. The work will be of considerable service to Englishmen who think of settling in South Africa.

First Elements of Experimental Geometry. By Paul Bert. (London: Cassell and Co., 1888.)

THE book of which this is a translation was M. Paul Bert's last work, and, like his earlier books of a similar kind, it is written in a style that cannot fail to interest children. His aim is to go straight to the goal, and, as he tells us in the preface, the goal of experimental geometry in elementary schools is, not a knowledge of the properties of different figures, but the power of measuring objects round about us. By the time the pupil has reached the third or fourth lesson he has learnt how to measure the height of a tree, and by so doing has done a practical piece of work, and begins to take an interest in the subject.

The book is divided into nine parts, containing in all about forty lessons. The measurement of straight lines, plane areas, solids, lengths of curved lines, &c., are dealt with in the first seven parts; the eighth shows the methods of constructing various geometrical figures and the instruments employed; Part 9 consists of the elements of land surveying and of plan drawing.

The illustrations and diagrams are numerous and well chosen throughout, and the work has been well translated. At the end of the volume exercises have been added for the use of teachers which are not found in the French version, the translator telling us that "the extraordinary character of our table of weights and measures has made it almost impossible to reproduce with the neatness and clearness of the original the numerous examples which are based upon the metrical system."

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Renewed Irruption of *Syrrhaptes.*

THANKS to your kindness in printing a note of mine a few weeks since (p. 103), I have received from your correspondents a large amount of help in the task I have undertaken; but there is, to me at least, a complete blank as regards observations of *Syrrhaptes* this year in France. It is almost impossible for the invasion to have missed that country, since Italy and Spain even have been visited in greater force than upon any one of the former occasions, yet not a word of the birds being seen in France on the present occasion has come to me, notwithstanding the inquiries I have made of French ornithologists. I would ask such of your readers as may be in that country to send me any tidings they may obtain. In 1863 there were at least a dozen French localities recorded, and in some of them large flocks were seen. I can hardly suppose that it has been otherwise this year.

ALFRED NEWTON.

Magdalene College, Cambridge, July 23.

Dr. Romanes' Article in the *Contemporary Review* for June.

MY attention has been directed to an article entitled "Recent Critics of Darwinism," by Dr. Romanes in the June number of the *Contemporary Review.* While the anonymous writer of a recent article in the *Edinburgh Review* is rightly exposed for quoting what he believes to be the opinions of men whose writings he can never have read, or at least can never have understood, it is somewhat unfortunate that Dr. Romanes should have fallen into the similar error of not making himself acquainted with views which he professes to express. He states (on page 841) that while Cope, Semper, Geddes, and Seebohm have argued "that any proof of natural selection as an operating principle opens up the *more ultimate problem* as to the *causes* of the variations on the occurrence of which this principle depends," Weismann and Poulton, on the other hand, "have not so much concerned themselves with this more ultimate problem." As it is unlikely that Dr. Weismann will have the opportunity of replying to this statement, it is only right to point out that this eminent zoologist has most certainly concerned himself very earnestly with this ultimate problem, and that his original and important theories upon the subject will be found in two of his recent papers, viz. "Die Continuität des Keimplasma's als Grundlage einer Theorie der Vererbung," Jena, 1885, and "Die Bedeutung der sexuellen Fortpflanzung für die Selektions-Theorie," Jena, 1886.

I should not have troubled to write this reply on account of the allusion to myself, and I agree with Dr. Romanes in the belief that my work does not throw any light upon the causes of variation. There are however many zoologists who believe that it has such a bearing, and indeed it seems only natural that writers (such as Dr. Romanes himself) who retain the Lamarckian conception of the direct influence of surroundings in causing the variations of the higher animals, should believe (as I think wrongly) that they see evidence for the soundness of their views in the results of experiments in which the colours of insects have been completely modified in a single generation by the action of environment. EDWARD B. POULTON

Oxford, July 15.

The Thunder-Axe.

THOSE who are interested in the study of anthropology need no reminder as to the European belief in a connection between ancient stone weapons and thunder. It would be mere waste of time if I quoted instances of this connection; but it may not be devoid of interest to some of your readers if I bring to their notice a modern account of the thunder-weapon, as described to-day by a New Zealander. The account may also be of service to those studying another branch of anthropology—that concerning the influence and value of ancient and modern creeds warring in the minds of semi-civilized peoples. I shall make no comment of my own, but proceed to give a translation of a tale printed (in Maori only) in the pages of the native newspaper, the *Korimako*. The few words in it which were not understood by those acquainted with the ordinary Maori speech, I referred to old men well versed in the dialect of that part of New Zealand.

"*The finding of Te Awhiorangi.*

"The tribes of this island have hitherto only heard of Te Awhiorangi, but have not seen it. We, Ngarauru—that is, the people descended from Rangitaupea, our ancestor who hid the axe—have never seen it until now . . . One of our settlements, called Okutuku, is near Waitotara. Twenty natives from this settlement proceeded in a party for the purpose of gathering the edible fungus (Hākekākeka) for the purpose of sale. With the party went a young woman whose name was Tomairangi (Dew of Heaven), the wife of Te Potonga Kaiawha. This girl was a perfect stranger in the district: she did not know where the sacred (tapu) places were; she belonging to the Ngaitahu (a South Island tribe), but her father was of us, the Ngarauru. The girl wandered away by herself, looking here and there, searching for trees on which the fungus grew. She saw a tree on which there was fungus, and laid her hand on it, but suddenly there came the flash of the Axe. Following with her eyes the direction of the flash, she saw the Axe close against the foot of a Pukatea tree; a cry of terror broke from her, and she fled screaming. At the same time the thunder roared, the lightning flashed, and blinding hail burst forth in sudden storm, increasing her terror almost to madness. Her husband heard her cries as she flew along: but an old man, called Te Rangi Whakairione, directly he heard her shrieks, understood the reason of the outcry, so he began to chant an incantation, and the fury of the storm abated. When the party had assembled together in the open land, the old priest asked which of them had been to Tieke; whereupon the girl asked 'Where is Tieke?' The old man answered that it was beyond the turn at Waione. Tomairangi replied, 'I have been there, but I did not know it was a sacred place: I saw something that looked like a spirit, and I am full of great fear.' Then all the party went to ascertain what it was, and then they found that it was indeed the lost sacred Axe, Te Awhiorangi. After Te Rangi Whakairione had chanted another incantation over it, they all took hold of the Axe, and wailed over it. When the crying had ceased, they brought the Axe back to the settlement. All the tribe knew that the Axe was somewhere in that vicinity, for our ancestor Rangitaupea had passed the secret on to his children in the words, 'Te Awhiorangi is at Tieke on the plain close above the Cave of the Dead.' Until now that place has been unvisited, being entirely sacred till this day, the 10th of December, 1887. Then gathered all Ngarauru and some of the Whanganui and Ngatiapa tribes, in number 300 persons, and at dawn the next day the sacred thing was hung up on a tree that all might see it. The priests, Kapua Tautahi and Werahiko Taipuhi were at the head of the procession as they approached the place: they reciting charms and incantations as they moved along with the people following. All the people carried green branches in their hands as an offering to Te Awhiorangi. When the concourse drew near the place, *successive peals of thunder and flashes of lightning rent the air;* then came down a dense fog, making it dark as night. The Tohunga (priests) stopped the thunder and dispersed the darkness by their incantations. When the light again appeared, the people offered the green branches, together with a number of Maori mats, &c.; then they made lamentations, and sang the old songs in which the ancient Axe was spoken of by their forefathers."

Thus far the native account. Then follows an enumeration of the articles offered up as propitiation; then a description of the axe, which appears to be a huge and beautiful specimen of the stone weapon, so highly polished that the face of the beholder may be seen reflected in it. Afterwards, the pedigree, or rather the mythological history, of the axe, showing how (name by name) it had been handed down from the first Maori chief who came to New Zealand (Turi), and that it had descended to him, through the great god Tane, from the primæval pair, Heaven and Earth (Rangi and Papa). But our chief interest in it is the thunder heralding its finding.

EDWARD TREGEAR.

Wellington, N.Z., June 11.

The Dispersion of Seeds and Plants.

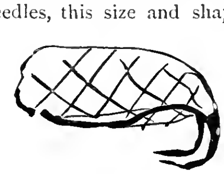
I HAVE read with much interest Mr. Morris's communication on the above subject (NATURE, vol. xxxvii. p. 466), and can corroborate most of what he states from personal observation. I can also remove his doubt respecting the germination of the seeds of the *Guava* and *Passiflora*, to which may also be added the *Tomato*.

I have adopted the "earth system" in my w.c., and from the place where the earth is deposited may always be gathered fine young plants of the three genera named above.

Thousands of acres of pasturage have been destroyed in this island by the distribution by birds of the *Lantana*, which was unfortunately introduced here by the first Roman Catholic missionaries, to form a hedge for their property at St. Louis or Conception. The "*Gendarme* plant" (an *Asclepiad*) was brought here in a pillow by a *gendarme* from Tahiti. It was a seed attached to a wing of silk cotton. The *gendarme* shook out his pillow; the wind carried the seed to a suitable spot, and now it vies with the *Lantana* in destroying our pastures.

I have shot the Great Fruit Pigeons of Fiji and this island with several seeds of the *Canarium* (?) in their crops, as Mr. Morris says, as big as hen's eggs. The seeds of water-plants are conveyed, with the eggs of fresh-water Mollusca, to vast distances, adhering to the hairs and feathers of the legs of water birds—ducks, herons, and waders of all sorts. In London the basins of the fountains in Trafalgar Square were peopled by *Lymnea* brought thither from the Serpentine, attached to the feathers of the sparrows who bathed, first in one, and then in the other.

Another plant which occurs to me as being largely indebted to man for its distribution, is that known as the "Cape Gooseberry," which is a native of South America (I forget its botanical name). The Kaffirs call it the "White man's plant," and say it follows the white man everywhere. I know it is found in India, Ceylon, Africa, Fiji, New Caledonia, New Hebrides. I really believe boiling it into jam does not destroy the vitality of the seeds. We have just got a plant here, bearing a lovely flower, but whence it comes no one knows. It has hard wooden seed capsules, each furnished with two hooks as hard as steel and as sharp as needles, this size and shape. These, hooking



into the hide of any animal, would be carried for days until forcibly dislodged.

The "Bathurst burr" (*Xanthium spinosum*) was introduced into the Cape in a cargo of wool wrecked at Cape Lagulhas, and spread out to dry, first there, and then at Simon's Town, at both of which places the "burr" sprang up. I believe and hope I destroyed the first and last plant of it that sprang up in New Zealand some twenty-five years ago. The seed had been brought in the living fleece of a fine merino ram. The owner of the pasture was cherishing the "wonderful new plant," and was not a little horrified when I took out my knife and carefully cut it down. He was more horrified when I told him what it was.

The seeds of some of the Indian banians, I believe, require to pass through the bodies of birds to enable them to germinate. A minute bird (*Diceum*) feeds on them, and is so small that its dropping cannot fall clear of the branch on which it sits, consequently it is glued to the bark and takes root. Sometimes this takes place on a palm tree; the roots then run down the trunk, and finally smother their host.

British Consulate, Noumea, May 15. E. L. LAYARD.

Indian Life Statistics.

ALTHOUGH Mr. Hill (in NATURE of July 12, p. 250) refers to the *Holi* festival as among possible influences in causing variations of births, he does not say whether he considers lucky and unlucky months and years, which so largely affect marriages in India, as incidents which may have an effect.

HYDE CLARKE.

TIMBER, AND SOME OF ITS DISEASES.[1]

X.

IN the months of April and May, the younger needle-like leaves of the Scotch pine are occasionally seen to have assumed a yellow tinge, and on closer examination this change in colour, from green to yellow, is seen to be due to the development of what look like small orange-coloured vesicles standing off from the surface of the epidermis, and which have in fact burst through from the interior of the leaf (Fig 31). Between these larger orange-

Fig. 31.—To the left is a pair of leaves of the Scotch pine, with the blister-like *Æcidia*. *a*, of *Peridermium Pini* (var. *acicola*) projecting from their tissues: these blisters are orange-yellow in colour, and contain spores, as shown in Fig. 33. Between the blisters are the minute *spermogonia*. *b* To the right is a small branch, killed at *a a a* by *Peridermium Pini* (var. *corticola*), the blister-like yellow *Æcidia* of the fungus being very conspicuous. (Reduced, after Hartig.)

yellow vesicles the lens shows certain smaller brownish or almost black specks. Each of the vesicular swellings is a form of fungus-fructification known as an *Æcidium*, and each of the smaller specks is a fungus-structure called a *Spermogonium*, and both of these bodies are developed from a mycelium in the tissues of the leaf. I must employ these technical terms, but will explain them more in detail shortly: the point to be attended to for the moment is that this fungus in the leaf has long been known under the name of *Peridermium Pini* (var. *acicola*, *i.e.* the variety which lives upon the needle-like leaves).

On the younger branches of the Scotch pine, the Weymouth pine, the Austrian pine, and some others, there may also be seen in May and June similar but larger bladder-like orange vesicles (*Æcidia*) bursting through the cortex (Fig. 31); and here, again, careful examination shows the darker smaller *Spermogonia* in patches between the *Æcidia*. These also arise from a fungus-mycelium in the

Fig. 32.—Blisters (*Æcidia*) of *Peridermium Pini* (var. *corticola*) on a branch of the Scotch pine: some of the *Æcidia* have already burst at the apex and scattered their spores, *b*, *b*; the others are still intact. (Natural size, after Hess.)

tissues of the cortex, whence the fungus was named *Peridermium Pini* (var. *corticola*). It is thus seen that the fungus *Peridermium Pini* was regarded as a parasite of pines, and that it possessed two varieties, one inhabiting the leaves and the other the cortex: the "varieties" were so considered, because certain trivial differences were found in the minute structure of the *Æcidia* and *Spermogonia*.

If we cut thin vertical sections through a leaf and one of the smallest *Æcidia*, and examine the latter with the microscope, it will be found to consist of a mass of spores

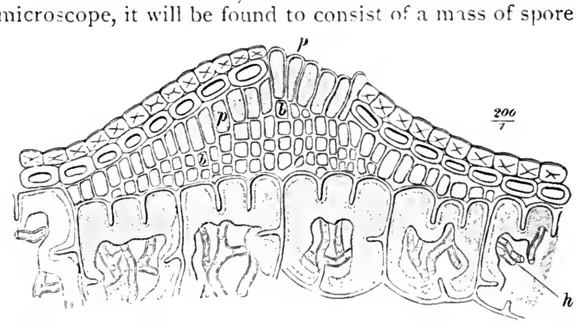


Fig. 33.—Vertical section through a very young *Æcidium* of *Peridermium Pini* (var. *acicola*), with part of the subjacent tissue of the leaf. *h*, the mycelium of the parasitic fungus running between the cells of the leaf: immediately beneath the epidermis of the leaf, the ends of the hyphæ give rise to the vertical rows of spores (*b*), the outermost of which (*p*) remain barren, and form the membrane of the blister-like body. The epidermis is already ruptured at *p* by the pressure of the young *Æcidium*. (After R. Hartig: highly magnified.)

arranged in vertical rows, each row springing from a branch of the mycelium: the outermost of these spores—*i.e.* those which form a compact layer close beneath the epidermis—remain barren, and serve as a kind of membrane covering the rest (Fig. 33, *p*). It is this membrane which protrudes like a blister from the tissues. The hyphæ of the fungus are seen running in all directions between the cells of the leaf-tissue, and as they rise up and form the vertical chains of spores, the pressure gradually forces up the epidermis of the leaf, bursts it, and the mass of orange-yellow powdery spores protrude to the exterior enveloped

[1] Continued from p. 272.

in the aforesaid membrane of contiguous barren spores. If we examine older *Æcidia*, it will be found that this membrane bursts also at length, and the spores escape.

Similar sections across a *Spermogonium* exhibit a structure which differs slightly from the above. Here also the hyphæ in the leaf turn upwards, and send delicate branches in a converging crowd beneath the epidermis; the latter gives way beneath the pressure, and the free tips of the hyphæ constrict off very minute spore-like bodies. These minute bodies are termed *Spermatia*, and I shall say no more about them after remarking that they are quite barren, and that similar sterile bodies are known to occur in very many of the fungi belonging to this and other groups.

Sections through the *Æcidia* and *Spermogonia* on the cortex present structures so similar, except in minute details which could only be explained by lengthy descriptions and many illustrations, that I shall not dwell upon them; simply reminding the reader that the resemblances are so striking that systematic mycologists have long referred them to a mere variety of the same fungus.

Now as to the kind and amount of damage caused by the ravages of these two forms of fungus.

In the leaves, the mycelium is found running between the cells (Fig. 33, *h*), and absorbing or destroying their contents: since the leaves do not fall the first season, and the mycelium remains living in their tissues well into the second year, it is generally accepted that it does very little harm. At the same time, it is evident that, if very many leaves are being thus taxed by the fungus, they cannot be supplying the tree with food materials in such quantities as if the leaves were intact. However, the fungus is remarkable in this respect—that it lives and grows for a year or two in the leaves, and does not (as so many of its allies do) kill them after a few weeks. It is also stated that only young pines are badly attacked by this form: it is rare to find *Æcidia* on trees more than twenty years or so old.

Much more disastrous results can be traced directly to the action of the mycelium in the cortex. The hyphæ grow and branch between the green cells of the true cortex, as well as in the bast-tissues beneath, and even make their way into the medullary rays and resin-canals in the wood, though not very deep. Short branches of the hyphæ pierce the cells, and consume their starch and other contents, causing a large outflow of resin, which soaks into the wood or exudes from the bark. It is probable that this effusion of turpentine into the tissues of the wood, cambium, and cortex, has much to do with the drying up of the parts above the attacked portion of the stem: the tissues shrivel up and die, the turpentine in the canals slowly sinking down into the injured region. The drying up would of course occur if the conducting portions are steeped in turpentine, preventing the conduction of water from below.

The mycelium lives for years in the cortex, and may be found killing the young tissues just formed from the cambium during the early summer: of course the annual ring of wood, &c., is here impoverished. If the mycelium is confined to one side of the stem, a flat or depressed spreading wound arises; if this extends all round, the parts above must die.

When fairly thick stems or branches have the mycelium on one side only, the cambium is injured locally, and the thickening is of course partial. The annual rings are formed as usual on the opposite side of the stem, where the cambium is still intact, or they are even thicker than usual, because the cambium there diverts to itself more than the usual share of food-substances: where the mycelium exists, however, the cambium is destroyed, and no thickening layer is formed. From this cause arise cancerous malformations which are very common in pine-woods (Fig. 34).

Putting everything together, it is not difficult to explain the symptoms of the disease. The struggle between the mycelium on the one hand, which tries to extend all round in the cortex, and the tree itself, on the other, as it tries to repair the mischief, will end in the triumph of the fungus as soon as its ravages extend so far as to cut off the water-supply to the parts above: this will occur as soon as the mycelium extends all round the cortex, or even sooner if the effusion of turpentine hastens the blocking up of the channels. This may take many years to accomplish.

So far, and taking into account the enormous spread of this disastrous disease, the obvious remedial measures seem to be, to cut down the diseased trees—of course this should be done in the winter, or at least before the spores come—and use the timber as best may be; but we must first see whether such a suggestion needs modifying, after learning more about the fungus and its habits. It appears clear, at any rate, however, that every diseased tree removed means a source of Æcidiospores the less.

Probably everyone knows the common groundsel, which abounds all over Britain and the Continent, and no doubt many of my readers are acquainted with other species of the same genus (*Senecio*) to which the groundsel belongs, and especially with the ragwort (*Senecio Jacobæa*). It has long been known that the leaves of these plants, and of several allied species, are attacked by a fungus, the mycelium of which spreads in the leaf-passages, and gives rise to powdery masses of orange-yellow spores, arranged in vertical rows beneath the stomata: these powdery

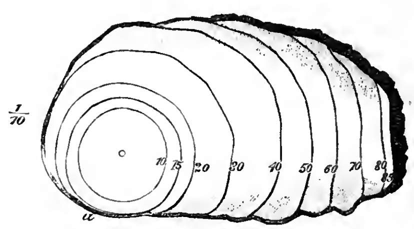


FIG. 34.—Section across an old pine-stem in the cancerous region injured by *Peridermium Pini* (var. *corticola*). As shown by the figures, the stem was fifteen years old when the ravages of the fungus began to affect the cambium near *a*. The mycelium, spreading in the cortex and cambium on all sides, gradually restricted the action of the latter more and more: at thirty years old, the still sound cambium only extended half-way round the stem—no wood being developed on the opposite side. By the time the tree was eighty years old, only the small area of cambium indicated by the thin line marked 80 was still alive; and soon afterwards the stem was completely "ringed," and dead, all the tissues being suffused with resin. (After Hartig.)

masses of spores burst forth through the epidermis, but are not clothed by any covering, such as the *Æcidia* of *Peridermium Pini*, for instance. These groups of yellow spores burst forth in irregular powdery patches, scattered over the under sides of the leaves in July and August: towards the end of the summer a slightly different form of spore, but similarly arranged, springs from the same mycelium on the same patches. From the differences in their form, time of appearance, and (as we shall see) functions, these two kinds of spores have received different names. Those first produced have numerous papillæ on them, and were called *Uredospores*, from their analogies with the uredospore of the rust of wheat; the second kind of spore is smooth, and is called the *Teleutospore*, also from analogies with the spores produced in the late summer by the wheat-rust. The fungus which produces these uredospores and teleutospores was named, and has been long distinguished as, *Coleosporium Senecionis* (Pers.). We are not immediately interested in the damage done by this parasite to the weeds which it infests, and at any rate we might well be tempted to rejoice in its destructive action on these garden pests: it is sufficient to point out that the influence of the mycelium is to shorten the lives of the leaves, and to rob the plant of food material in the way referred to generally in my last article.

What we are here more directly interested in is the following. A few years ago Wolff showed that if the spores from the *Æcidia* of *Peridermium Pini* (var. *acicola*) are sown on the leaf of *Senecio*, the germinal hyphæ which grow out from the spores *enter the stomata of the Senecio leaf, and there develop into the fungus called Coleosporium Senecionis.* In other words, the fungus growing in the cortex of the pine, and that parasitic on the leaves of the groundsel and its allies, are one and the same: it spends part of its life on the tree and the other part on the herb.

If I left the matter stated only in this bald manner, it is probable that few of my readers would believe the wonder. But, as a matter of fact, this phenomenon, on the one hand, is by no means a solitary instance, for we know many of these fungi which require two host-plants in order to complete their life-history; and, on the other hand, several observers of the highest rank have repeated Wolff's experiment and found his results correct. Hartig, for instance, to whose indefatigable and ingenious researches we owe most that is known of the disease caused by the *Peridermium*, has confirmed Wolff's results.

It was to the brilliant researches of the late Prof. De Bary that we owe the first recognition of this remarkable phenomenon of *heterœcism—i.e.* the inhabiting

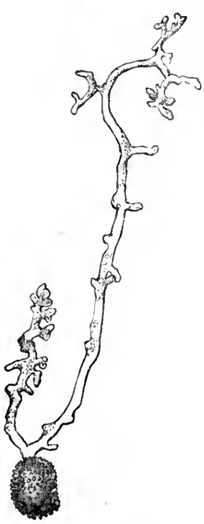


Fig. 35.—A spore of *Peridermium Pini* germinating. It puts forth the long, branched germinal hyphæ on the damp surface of a leaf of *Senecio*, and one of the branches enters a stoma, and forms a mycelium in the leaf: after some time, the mycelium gives rise to the uredospores and teleutospores of *Coleosporium Senecionis*. (After Tulasne: highly magnified.)

more than one host—of the fungi. De Bary proved that the old idea of the farmer, that the rust is very apt to appear on wheat growing in the neighbourhood of berberry-bushes, was no fable; but, on the contrary, that the yellow *Æcidium* on the berberry is a phase in the life-history of the fungus causing the wheat-rust. Many other cases are now known, *e.g.* the *Æcidium abietinum*, on the spruce firs in the Alps, passes the other part of its life on the Rhododendrons of the same region. Another well-known example is that of the fungus *Gymnosporangium*, which injures the wood of junipers: Oersted first proved that the other part of its life is spent on the leaves of certain Rosaceæ, and his discovery has been repeatedly confirmed. I have myself observed the following confirmation of this. The stems of the junipers so common in the neighbourhood of Silverdale (near Morecambe Bay) used to be distorted with *Gymnosporangium*, and covered with the *teleutospores* of this fungus every spring: in July all the hawthorn hedges in the neighbourhood had their leaves covered with the Æcidium form (formerly called *Rœstelia*), and it was quite easy to show that the fungus on the hawthorn leaves was produced by sowing the *Gymnosporangium* spores on them. Many other well-established cases of similar heterœcism could be quoted.

But we must return to the *Peridermium Pini*. It will be remembered that I expressed myself somewhat cautiously regarding the *Peridermium* on the leaves (var. *acicola*). It appears that there is need for further investigations into the life-history of this form, for it has been thought more than probable that it is not a mere variety of the other, but a totally different species.

Only so lately as 1883, however, Wolff succeeded in infecting the leaves of *Senecio* with the spores of *Peridermium Pini* (*acicola*), and developing the *Coleosporium*, thus showing that both the varieties belong to the same fungus.

It will be seen from the foregoing that in the study of the biological relationships between any one plant which we happen to value because it produces timber, and any other which grows in the neighbourhood there may be (and there usually is) a series of problems fraught with interest so deep scientifically, and so important economically, that one would suppose no efforts would be spared to investigate them: no doubt it will be seen as time progresses that what occasionally looks like apathy with regard to these matters is in reality only apparent indifference due to want of information.

Returning once more to the particular case in question, it is obvious that our new knowledge points to the desirability of keeping the seed-beds and nurseries especially clean from groundsel and weeds of that description: on the one hand, such weeds are noxious in themselves, and on the other they harbour the *Coleosporium* form of the fungus *Peridermium* under the best conditions for infection. It may be added that it is known that the fungus can go on being reproduced by the *uredospores* on the groundsel-plants which live through the winter.

H. Marshall Ward.

(*To be continued.*)

EARTHQUAKES AND HOW TO MEASURE THEM.[1]

PROF. EWING explained that the study of earthquakes had two aspects, one geological and the other mechanical, and it was of the latter alone that his lecture was to treat. The mechanical student of earthquakes concerned himself with the character of the motion that was experienced at any point on the earth's crust, and with the means by which an earthquake spread from point to point by elastic vibration of rock and soil. The first problem in seismometry was to determine exactly how the ground moved during an earthquake, to find the amount and direction of every displacement, and the velocity and rate of acceleration at every instant while the shaking went on. He was to deal with the solution of that problem, and to describe some of the results which had been obtained in the measurement of earthquakes in Japan, where earthquakes happened with a frequency sufficient to satisfy the most enthusiastic seismologist. Most early attempts to reduce the observing of earthquakes to an exact science had failed because they were based on a false notion of what earthquake motion was. It had been supposed that an earthquake consisted of a single or at least a prominent jerk, or a few jerks, easily distinguishable from any minor oscillations that might occur at the same time. The old column seismometer, for instance, recommended in the Admiralty Manual of Scientific Inquiry, attempted to measure what was called the intensity of the shock by means of a number of circular columns of various diameters which were set to stand upright like ninepins on a level base. It was expected that the shock

[1] Abstract of a Lecture delivered at the Royal Institution on Friday evening, June 1, by Prof. J. A. Ewing, F.R.S.

would overthrow the narrower columns, the broadest that fell serving to measure its severity, and that the columns would fall in a direction which would point to the place of origin of the disturbance. In fact, however, such columns fell most capriciously when they fell at all, and it was impossible to learn anything positive from their behaviour in an earthquake. The reason was that there was no single outstanding impulse: an earthquake consisted of a confused multitudinous jumble of irregular oscillations, which shifted their direction with such rapidity that a point on the earth's surface wriggled through a path like the form a loose coil of string might take if it were ravelled into a state of the utmost confusion. The mechanical problem in seismometry was to find a steady-point—to suspend a body so that some point in it, at least, should not move while this complicated wriggling was going on. The steady-point would then serve as a datum with respect to which the movement of the ground might be recorded and measured. The simple pendulum had often been suggested as a steady-point seismometer, but in the protracted series of oscillations which made up an earthquake the bob of a pendulum might, and often did, acquire so much oscillation that, far from remaining at rest, it moved much more than the ground itself. The lecturer illustrated this by showing the cumulative effect of a succession of small impulses on a pendulum when these happened to agree in period with the pendulum's swing. The fault of the pendulum, from the seismometric point of view, was its too great stability, and its consequently short period of free oscillation. To prevent the body whose inertia was to furnish a steady-point from acquiring independent oscillation, the body must be suspended or supported astatically; in other words, its equilibrium must be very nearly neutral. Methods of astatic suspension which had been used in seismometry were described and illustrated by diagrams and models, in particular the ball and block seismometer of Dr. Verbeck, the horizontal pendulum, and a method of suspension by crossed cords based on the Tchebicheff straight-line link-work.

The complete analysis of the ground's motion was effected by a seismograph which resolved it into three components, two horizontal and one vertical, and recorded each of these separately, with respect to an appropriate steady-point, by means of a multiplying lever, on a sheet of smoked glass which was caused to revolve at a uniform rate by clock-work. The clock was started into motion by the action of the earliest tremors of the earthquake on a very delicate electric seismoscope, the construction of which was shown by a diagram. In this way a record was deposited upon the revolving plate which gave every possible particular regarding the character of the earth's motion at the observing-station. A complete set of the instruments as now manufactured by the Cambridge Scientific Instrument Company was shown in action. Prof. Ewing also described his duplex pendulum seismograph, which draws on a fixed plate of smoked glass a magnified picture of the horizontal motion of the ground during an earthquake. Apparatus was shown for testing the accuracy of the seismographs by means of imitation earthquakes, which shook the stand of the instrument, and drew two diagrams side by side upon the glass plate—one the record given by the seismograph itself, and the other the record derived from a fixed piece which was held fast in an independent support. The agreement of the two records with one another proved how very nearly motionless the "steady-point" of the seismograph remained during even a prolonged shaking resembling an earthquake. This test was applied to the instruments on the table, and the close agreement of the two diagrams was exhibited by projecting them on the lantern-screen. A large number of autographic records of Japanese earthquakes were thrown on the screen, including several which have been already reproduced in this journal (NATURE, vol. xxx. p. 174, vol. xxxi. p. 581, vol. xxxvi. p. 107); and particulars were given of the extent of the motion, and the velocity and rate of acceleration, in some representative examples. To determine the rate of acceleration was of special interest, because it measured the destructive tendency of the shock. The lecturer explained that some of the seismograms exhibited on the screen had been obtained since he had left Japan by his former assistant, Mr. Sekiya, who now held the unique position of Professor of Seismology in the Imperial Japanese University. Prof. Sekiya had recently taken the pains to construct a model representing, by means of a long coil of copper wire carefully bent into the proper form, the actual path pursued by a point on the earth's surface during a prolonged and rather severe shaking. This model of an earthquake had been made by combining the three components of each successive displacement as these were recorded by a set of seismographs like those upon the lecture-table. The appearance of Prof. Sekiya's model (a description of which will be found in NATURE, vol. xxxvii. p. 297) was shown to the audience by means of the lantern.

Prof. Ewing drew attention to the small tremors of high frequency which characterized the beginnings of earthquake motion, and which were apparent in a number of the diagrams he exhibited. These generally disappeared at a comparatively early stage in the disturbance. In the early portion they were generally found at first alone, preceding the larger and and slower principal motions; and then when the principal motions began, small tremors might still be seen for some time, superposed upon them. In all probability these quick-period tremors were normal vibrations, while the larger motions were transverse vibrations; and a reference to the theory of the transmission of vibrations in elastic solids served to explain why the quick-period tremors were the first to be felt. The whole disturbance went on for several minutes, with irregular fluctuations in the amplitude of the motion, and with a protracted dying out of the oscillations, the period of which usually lengthened towards the close. The record of a single earthquake comprised some hundreds of successive movements, to and fro, round fantastic loops. Each single movement usually occupied from half a second to two seconds. Earthquakes were quite perceptible in which the greatest extent of motion was no more than 1/100 of an inch. In one case, on the other hand, Prof. Sekiya had obtained a record in which the motion was as much as an inch and three-quarters. Even that was in an earthquake which did comparatively little damage, and there was therefore reason to expect that in a severely destructive shock (such as had not occurred since the present system of seismometry was developed) the motion might be considerably greater.

Prof. Ewing concluded his lecture by pointing out that seismographs might find practical application in measuring the stiffness of engineering structures. He exhibited, by the lantern, seismographic records he had recently taken on the new Tay Bridge, to examine the shaking of the bridge during the passage of trains. The instrument had been placed on one of the great girders, two-thirds of a mile from the Fife end, at a place where there was reason to expect the vibration would be a maximum. The extent of motion was remarkably small. It was less than an eighth of an inch, even while the train was passing the seismograph—a fact which spoke well for the stiffness of the structure. Nevertheless, by watching the index of the seismograph he had been able to tell whenever a train came on at the Dundee end of the bridge, a distance of $1\frac{1}{3}$ mile from the place where the instrument was standing. One could then detect a vibratory motion, the extent of which was probably not more than 1/500 of an inch. This began in the longitudinal direction, and for some time longitudinal vibration only could be seen. As the train came nearer, lateral vibration also began, and the amplitude of course increased. It reached a maximum when the train was close to the seismograph, and continued visible until the train had passed off the bridge at the other end.

DOES PRECIPITATION INFLUENCE THE MOVEMENT OF CYCLONES?

IN Prof. Elias Loomis's first "Contribution to Meteorology," in the *American Journal of Arts and Science*, he examined the distribution of rain around 152 storms (cyclones) in the United States, in order to determine whether there exists any relation between the velocity of a storm's progress and the extent of the accompanying rain area. He found that "the average extent of the rain area on the east side of the storm's centre is 500 miles; and when the rain area extends more than 500 miles, the storm advances with a velocity greater than the mean; but when the extent of the rain area is less than 500 miles, the storm advances with a velocity less than the mean." In his twelfth "Contribution" he examined 39 storms which moved with exceptional velocity (1000 miles or more per day) and found that "the rain area generally extended a great distance in advance of the storm centre, the average distance being 667 miles." Finally, Loomis examined 29 cases of those abnormal cyclones in the United States which moved toward the west. He says:—"In nearly every case we find a fall of rain or snow in the region toward which the low centre advanced, and in most of the cases the rainfall was unusually great. . . . It may be inferred from these comparisons that the fall of rain or snow is one of the most important causes which determine the abnormal movements of areas of low pressure" (ninth memoir, p. 44). Ley and Abercromby state that in Great Britain the relation of the weather to the cyclone centre is the same whatever the path of the cyclone; thus when storms advance toward the west the greatest cloud development and rainfall is to the west of the cyclone centre. In the Proceedings of the Royal Meteorological Society, vol. xliii., Abercromby gives a table showing the relation between the intensity of "trough phenomena" and the velocity of cyclones. This table indicates very clearly that the greater the velocity of the cyclone the more marked the "trough phenomena." Hence, according to Abercromby's definition of "trough phenomena" the heaviest rain and cloud areas are massed toward the front of rapidly advancing cyclones, while immediately after the passage of the line of minimum pressure the sky begins to show signs of clearing. This is especially well marked in cyclones passing off the north-east coast of the United States. When the cyclones are moving with unusual rapidity, not only all the rain, but almost all of the cloud area is confined to the front half of the cyclone.

Loomis suggested that the excess of rain in front of rapidly advancing cyclones was one of the causes of the rapid advance; but when investigating heavy rainfalls in the United States he concludes that "the forces which impart that movement to the air which is requisite to an abundant precipitation of vapour, instead of deriving increased strength from the great volume of rain, rapidly expend themselves and become exhausted;" and after examining certain cyclones which were accompanied by no rain he adds: "So that it seems safe to conclude that rainfall is not essential to the formation of areas of low barometer, and is not the principal cause of their formation or of their progressive movement." Hann arrives at similar conclusions from investigations in Europe. After investigating an especially heavy rainfall which occurred in Austria and vicinity in August 1880, he concludes thus:—"The appearance of a barometric minimum in Hungary occasioned abnormal and extended precipitation on the west and north-west side of this barometric depression. The reaction of this precipitation on the position of the centre of the depression is scarcely perceptible. . . . We find, therefore, through the investigation of the relative lowest barometer reading in its behaviour to rainfall, that our former conclusions are confirmed" (lxxxii. *Bunde d. Wiss.* ii. Ab., November 1880). This investigation does not necessarily prove that precipitation does not appreciably influence the movements of cyclones in general, but at least suggests that in the first cases mentioned above the unequal distribution of rain around rapidly moving cyclones was not the cause, but the result of the cyclone's advance. In cyclones which move very slowly, as do tropical cyclones, the air ascends almost uniformly around the centre; but when cyclones have a more rapid progressive motion, the air in the rear, which has not only to enter, but to follow the cyclone, is more retarded by friction than the air in front, and hence does not enter the cyclone so freely, so that the formation of cloud and rain in the rear is retarded; while, on the other hand, a larger volume of new air enters the progressing cyclone in front, and increases the amount of precipitation. Thus, between February 12 and 14, a cyclone passed across the American continent with the exceptionally high velocity of 58 miles per hour. During its passage the highest wind velocity reported on any of the United States Signal Service morning weather maps was 40 miles per hour, occurring immediately in the rear of the cyclone at Father Point, Can., on the morning of the 14th. At none of the other 130 stations did the maps show a wind velocity exceeding 30 miles per hour during the passage of the cyclone. This is an example of many similar cases which show that in rapidly moving cyclones the air in the rear near the earth's surface does not move as rapidly as the cyclone itself. Hence, it seems evident that the air near the surface immediately in the rear of these cyclones is not air which has followed the cyclone near the surface, but air which has descended from above. Espy showed many years ago that, on account of mechanical heating by compression, no descending air can be accompanied by precipitation; and an explanation is thus afforded why there is none, or but little cloud and precipitation in the rear of rapidly moving cyclones. On the other hand, in order that a cyclone may advance rapidly, there must be a rapid decrease in pressure, and consequently a rapid removal of the air, in front of the advancing depression. Since, according to the normal circulation of a cyclone, there is an inward movement near the earth's surface and an upward and outward movement near the top, this upward and outward movement is necessarily increased in unusually rapid-moving cyclones, and hence also the cloudiness and precipitation are increased.

Hourly observations of cloud movements made during the day hours for nearly two years at Blue Hill Observatory indicate that the velocity of storm movement, and especially the variability of the weather, are intimately connected with the velocity of movement of the general atmosphere.

The writer is hence led to believe that the main cause of rapid cyclone progression is an unusually rapid drifting of the atmosphere over large regions; and the unequal distribution of rain around the cyclone is due to the rapid progress of the cyclone.

H. HELM CLAYTON.

Blue Hill Observatory, Boston, June 18.

NOTES.

MR. JOHN WHITEHEAD returned to Labuan in safety from his second expedition to Kina Balu, and is daily expected in England. He ascended the mountain to its summit, and attained to an altitude of 13,500 feet. His collection will contain many novelties, the small portion sent by him in advance to Mr. Bowdler Sharpe exhibiting many curious features. The new species will be described by Mr. Sharpe in the forthcoming number of the *Ibis*, and four genera and twelve species appear to be quite new to science. Mr. Whitehead spent altogether eight months on the mountain of Kina Balu, and is at present known to have discovered thirty-one new species of birds. On his last expedition he met with fifteen different kinds of rodents, and his collections of reptiles and insects are also very large.

MR. ALFRED EVERETT, the well-known explorer of Borneo and the Philippine Islands, has had to return to England to recruit his health, sorely shattered by his nineteen years' residence in the tropics. He has brought with him a collection of birds and animals, amongst which are apparently many interesting species. He also discovered in the Brunei district the nest of *Machærhamphus alcinus*, the curious crepuscular Honey-Kite of the East, but unfortunately the tree in which it was placed proved to be inaccessible. This remarkable genus of Hawks occurs in the Malayan peninsula, Borneo, and again in New Guinea. It has an Ethiopian representative, *M. anderssoni*, which inhabits Damara Land and Madagascar.

THE summer meeting of the Institution of Mechanical Engineers will be held in Dublin on Tuesday, 31st inst., and the two following days, under the presidency of Mr. Edward H. Carbutt. An influential Committee has been formed for the reception of the Institution, under the chairmanship of Lord Rosse, F.R.S. On Friday, August 3, a visit will be paid to Belfast, on the invitation of a local Committee presided over by the Mayor, Sir James H. Haslett.

THE half-yearly general meeting of the Scottish Meteorological Society was held in the hall of the Royal Scottish Society of Arts, Edinburgh, on Monday, July 23, at two p.m. The following was the "business":—(1) Report from the Council of the Society; (2) the temperature of the air and surface-water of the North Atlantic, by H. N. Dickson; (3) the climate of the Isle of Man, by A. W. Moore; (4) note on earth currents on Ben Nevis, in connection with anticyclones, by R. T. Omond; (5) St. Elmo's fire observed at the Ben Nevis Observatory, by A. Rankin. Photographs of clouds, &c., from Ben Nevis were exhibited.

THE Berlin Academy has granted to Dr. R. von Lendenfeld the sum of 1000 marks to aid him in investigating the physiological functions, chiefly the digestion, of sponges.

A HUNGARIAN deputy, M. Hlavka, has given a sum of 200,000 florins towards the establishment of a Czeck Academy of Science at Prague.

THE death of Prof. H. Carvill Lewis, in the full vigour of manhood and of work, will be a painful surprise to many friends on both sides of the Atlantic. He died of typhoid fever at Manchester on July 21, a few days after landing in England, at the beginning of a journey undertaken in continuance of his investigations into the glacial deposits of Europe.

THE death is announced of Dr. Ludwig Julius Budge, the eminent physiologist and anatomist. He was born at Wetzlar, September 6, 1811, and died at Greifswald on July 14.

THE Geologists' Association have issued the programme of a long excursion to the Forest of Dean, Wye Valley, and South Wales, from August 6 to 11.

THE *Revue Internationale*, published at Rome, contains a description of the eighth centenary of the University of Bologna, and a dignified reply to the criticisms of the correspondent of the *Times*. The correspondent maintained that all delegates of foreign Universities, including American Colleges, ought to have received honorary degrees, without saying what the number and what the value of such distinctions would have been. The honorary degree was given "*agli scienziati saliti in altissima fama*," and this would hardly apply to all the chosen or self-constituted representatives of the world's Universities. We quote the folowing words from the article in the *Revue Internationale* for July:—"Il a paru dans le *Times* quelques correspondances très acerbes, sans grande portée cependant, étant donné le caractère du journal. En lisant le *Times*, la pensée du lecteur se reporte souvent instinctivement à ce Sir John Davenne, qui, au dire de Ruffini, était un parfait galant homme, un vrai gentilhomme, mais auquel il pouvait arriver—un peu par l'effet de son caractère individuel, un peu par l'effet du caractère national—de ne pas se montrer trop impartial, trop juste, ni trop tempéré dans ses jugements."

DESPATCHES have been received from Dr. Nansen announcing the safe departure of his expedition for Greenland from the Isa Fjord, in Iceland, on board the steam whaler *Jason*.

AN astronomical observatory is about to be erected within the walls of the Foreign College at Pekin.

A CORRESPONDENT of an English newspaper published in China furnishes the following account of the new foreign College being erected at Tientsin by the Viceroy Li Hung Chang:—"In coming up the Peiho to Tientsin, the first object of importance that will now strike the eye of a stranger is the new College building which is being erected just outside the mud rampart by the Viceroy, for the instruction of Chinese youth in the mysteries of the English language and of foreign science. This is a massive edifice, two stories high, built around the four sides of a square which forms a large interior court not less than one hundred feet on either side, around which, on the inner sides of the buildings, are spacious verandas. The construction of the building, under the careful supervision of a capable foreign engineer, is all that could be desired. If the educational results are equal to what has been accomplished by brick and mortar, the Viceroy will have great occasion to be proud that he has been privileged to start such an institution. It was hoped that the College would be ready for opening this autumn, but there seems little prospect that it can be opened before next spring."

IT is reported from China that Dr. Dudgeon, of Pekin, has published in Chinese a work on anatomy which he has had in preparation for some years; that a companion work on materia medica is in the press, with treatises on physiology and photography, in the latter of which the dry process is explained. Dr. Dudgeon is also preparing bi-lingual vocabularies of medical and anatomical terms.

WE are glad to learn that the Mikado of Japan has been pleased to bestow the Order of the Rising Sun on Prof. John Milne, of the Imperial University of Tokio, the well-known investigator of seismic phenomena.

VOL. II. Part I of the Journal of the College of Science of the Japanese Imperial University, contains an important summary by Prof. Sekiya of the results of seismometric observations in Tokio during two years, from September 1885 to September 1887, with special reference to the measurements of vertical motion. The observations recorded by Prof. Sekiya were for the most part made with Prof. Ewing's seismographs, some on the soft marshy ground of the lower part of the city, and some on the stiff soil of the upper parts. Particulars are recorded very fully for 119 earthquakes in a table setting forth the greatest horizontal and vertical motion, the period of the motion, the maximum velocity and rate of acceleration, the duration of the disturbance, and the approximate locality of the origin. At the end of the paper the results are collated, and averages are deduced, from which it appears that the greatest horizontal motion is about six times the greatest vertical motion in those earthquakes in which vertical motion was sensible. These, however, formed only 28 per cent. of the whole number recorded. The period of the vertical motion was little more than half that of the horizontal. In only 18 per cent. of the recorded shocks was the extent of motion greater than one millimetre. The paper forms the most extensive collection of data in absolute seismometry that has yet been published, and is a very valuable contribution to seismology.

ACCORDING to a telegram sent through Reuter's agency from Yokohama on July 18, a volcanic eruption had occurred at Makamats (? Takamatsu). Four hundred persons are reported to have been killed and 1000 injured.

IN our issue of the 6th October last (vol. xxxvi. p. 546) we drew attention to the useful work of Mr. Wragge, the Government meteorologist of Queensland, in issuing daily weather charts for Australasia. The entire meteorological observing-system of that colony is in course of reconstruction, upon the lines adopted by the Meteorological Office in London and other similar institutions abroad, and Mr. Wragge invites attention to the new series of weather charts now prepared at 9h. a.m. daily (except Sundays and holidays), files of which are kept at the Meteorological Office and at the office of the agent for Queensland, both in Victoria Street. The charts, which are on a large scale, contain observations received by wire from seventy-two selected observatories distributed over the Australian continent, Tasmania, and New Zealand, show very clearly the general atmospherical conditions, and contain besides collated information from about 300 smaller stations. A prominent feature in the new meteorological service is the preparation of a complete digest of the meteorological conditions of each colony, together with forecasts, which are issued about 5h. p.m. to the press. These publications have, of course, a special value to men of science generally, while to those interested in agricultural and shipping pursuits they have a practical bearing hitherto unequalled in Australia.

THE Pilot Chart of the North Atlantic Ocean for July shows that no severe cyclonic storms entirely crossed that ocean in June, but two or three depressions were formed in the mid-Atlantic, and caused gales off the Irish coast from the 8th to the 12th inclusive. Much fog was experienced off the American coast, north of Hatteras, and in the English Channel, and in the early part of the month fog-banks were frequently met with east of the 40th meridian. Icebergs and field ice have been encountered, principally off the eastern and southern coasts of Newfoundland. A few bergs, however, have been seen as far south as the 43rd parallel, in longitude 43° west. The chart also contains valuable information with reference to the West India hurricanes which are now likely to be encountered.

IN the Berlin *Meteorologische Zeitschrift* for June, Dr. Hann gives an interesting account of the winter temperature of Werchojansk (Siberia), deduced from several years' observations. The town, which lies in the valley of the Jana, about 9 feet above the level of the river, in latitude 67° 34′ N., longitude 133° 51′ E., and at a height of about 350 feet above the sea, has the greatest winter cold that is known to exist upon the globe. Monthly means of $-58°$ F. occur even in December, a mean temperature which has been observed nowhere else in the Polar regions; and minima of $-76°$ are usual for the three winter months (December–February). In the year 1886 March also had a minimum $-77°$, and during that year December and January never had a minimum above $-76°$, while in January, 1885, the temperature of $-89°$ was recorded. These extreme readings are hardly credible, yet the thermometers have been verified at the St. Petersburg Observatory. To add to the misery of the inhabitants, at some seasons the houses are inundated by the overflow of the river. The yearly range of cloud is characteristic of the climate; in the winter season the mean only amounts to about three-tenths in each month.

A NEW base has been discovered in tea by Dr. Kossel, of Berlin. It appears to be an isomer of theobromine, the well-known base present in cocoa-beans, possessing the same empirical formula, $C_7H_8N_4O_2$, but differing very materially in physical and certain chemical properties. The new base, to which has been assigned the name theophylline, was discovered during the investigation of large quantities of tea-extract, which, after treatment with sulphuric acid to remove foreign matters, was saturated with ammonia-gas and precipitated with ammoniacal silver solution. The silver precipitate was then digested with warm nitric acid, and, on cooling, the silver salt which separated out was filtered off and the filtrate rendered slightly alkaline with ammonia. On allowing this alkaline liquid to stand until the next day, a brownish deposit was noticed, which, on examination, proved to be the silver compound of a new base. The solution was therefore further concentrated, and a second and much larger yield of the silver salt obtained. This was next decomposed by sulphuretted hydrogen, the free base being thus obtained in solution. The liquid, after removal of the silver sulphide by filtration, deposited on standing a small quantity of xanthine, $C_5H_4N_4O_2$, a derivative of uric acid, whose presence in tea has previously been shown. The mother-liquors were afterwards treated with mercuric nitrate solution, which precipitated the theophylline in the form of a mercury compound, from which the base itself could readily be obtained by treatment with sulphuretted hydrogen as before. Analyses of the theophylline obtained after purification indicate the formula $C_7H_8N_4O_2$, which is that of theobromine. But the two substances are certainly not identical: their crystals are quite distinct, those of theophylline containing one molecule of crystal water which is expelled at 110°, while theobromine crystallizes anhydrous. The crystals also are totally unlike those of the other known isomer of theobromine, paraxanthine, from which theophylline differs most materially in its behaviour with soda. Again, the melting-points of the three isomers are considerably removed from each other, and their different solubilities in water are conclusive proofs of their different internal structures. Theophylline forms a well-crystallized series of salts with the mineral acids, and with platinum, gold, and mercury chlorides; and, like theobromine, yields with silver nitrate a silver substitution-compound, $C_7H_7AgN_4O_2$, which, as may be concluded from the above method of isolation, is readily soluble in nitric acid. Finally, to complete the proof of its isomerism with theobromine, which is the dimethyl derivative of xanthine, the silver compound was found to react with methyl iodide to form tri-methyl xanthine, better known as caffeïne or theïne, the remarkable base of the coffee and tea plants.

IN a letter lately submitted to the Elliott Society, and printed in its Proceedings, Mr. G. W. Alexander, of Charleston, S.C., tells a strange tale of a humming-bird. Mr. Alexander heard in his garden what he knew must be a cry of pain; and going to a vine, from which the cry seemed to proceed, he found a humming-bird "struggling violently, but unable to extricate itself." He took it in his hands, and, to his astonishment, saw that it was in the clutches of an insect, which he identified as a mantis, popularly known in those parts as "Johnny-cock-horse." "The bird," says Mr. Alexander, "was wounded under the wing, upon one side of the breast, which had evidently been lacerated with the powerful mandibles of its captor. The wound looked ugly enough to lead me to fear that it would prove fatal; nevertheless my children and I cared for it as tenderly as we knew how, but we found it difficult to administer nourishment to a humming-bird. So at night I placed it among the leaves of the vine—for it was a warm night—and in the morning the little sufferer lay dead on the ground beneath."

A SERIES of volumes to be entitled the "Fauna of British India," containing descriptions of the animals found in British India and its dependencies, including Ceylon and Burma, is about to be issued, under authority from the Government. For the present the work will be restricted to vertebrate animals. The editorship has been intrusted by the Secretary of State for India in Council to Mr. W. T. Blanford, formerly of the Geological Survey of India, and the printing and publication to Messrs. Taylor and Francis. The descriptions of vertebrates will occupy seven volumes, of which one will be devoted to mammals, three to birds, one to reptiles and Batrachians, and two to fishes. The mammals will be described by Mr. Blanford, the reptiles and Batrachians by Mr. G. A. Boulenger, of the British Museum, and the fishes by Mr. F. Day, Deputy Surgeon-General. The arrangements for the volumes on birds are nearly complete, and there is every probability of their being undertaken by a competent Indian ornithologist very soon. A half-volume of mammals will be issued immediately. It is expected that one or two volumes will be published each year. The work will be illustrated by cuts.

MESSRS. SAMPSON LOW will publish shortly the "Life and Correspondence of Abraham Sharp," the Yorkshire mathematician and astronomer, with memorials of his family, by William Cudworth. The work will be illustrated with numerous drawings specially prepared for it. Abraham Sharp, a member of an ancient family at Horton, near Bradford, was assistant in 1689 to Flamsteed, the first Astronomer-Royal, and designed and fixed the mural arc and other astronomical instruments with which the Astronomer-Royal made his observations at Greenwich Observatory. He also computed the places of many of the fixed stars in Flamsteed's famous "Catalogue," and was the principal means of completing and publishing the second and third volumes of the "Historia Celestis," published after Flamsteed's death. For many years after Abraham Sharp left the Observatory, a correspondence was kept up between him and Flamsteed, which gives much insight into many of the scientific events of the period, and especially refers to the difficulties experienced by Flamsteed in the publication of his great work, and to the doings of his contemporaries, Sir Isaac Newton, Dr. Halley, Sir Christopher Wren, and others. This correspondence will form the basis of Mr. Cudworth's work.

THE third number of vol. vi. of the Proceedings of the Bath Natural History and Antiquarian Field Club has been issued. Among the contents are the following papers: on some Ostracoda from the fullers' earth Oolite and Bradford clay, by Prof. T. Rupert Jones, F.R.S., and C. Davies Sherborn; landslips and subsidences, by W. Pumphery; remarks on some Hemiptera-Heteroptera taken in the neighbourhood of Bath, by Lieut.-Colonel Blathwayt; recent "finds" in the Victoria gravel pit, by the Rev. H. H. Winwood; note on *Webbina irregularis* (d'Orb.) from the Oxford clay at Weymouth, by C. Davies Sherborn.

MESSRS. WILLIAM WESLEY AND SON have issued No. 90 of their "Natural History and Scientific Book Circular." It contains lists of works relating to astronomy and mathematics.

THE heat in Norway this summer is most intense, the temperature exceeding any registered this century. At Christiania the thermometer has several times registered 30° to 32° C. in the shade, and at Nyborg, in the Varanger Fjord, near the White Sea, it was 35° C. at the end of June.

ON July 15 a remarkable mirage was seen, about 11 p.m., at Hudiksvall, on the Baltic. It represented a ship going down in a terribly agitated sea, a boat being on the point of putting off from the vessel. The mirage lasted five minutes.

A CURIOUS ornithological phenomenon is witnessed at Oddernæs, in the south of Norway, this season, the ring throstle (*Turdus torquatus*) nesting there. Generally, the bird only breeds in the extreme north. Prof. Esmark is of opinion that the present unusual occurrence is due to the severity of the spring.

DURING the spring of the present year some 200 eider-fowl were caught in fishermen's nets on the south coast of Sweden.

THE remains of several prehistoric canoes have been found at the bottom of some lakes drained off in uplands in Central Sweden. They were made by the hollowing out of trunks of trees by fire. One had evidently been sunk on purpose, being full of large stones.

AN unusually large skull of the *Rhinoceros tichorrhinus* was lately discovered in a well-preserved condition at Rixdorf, near Berlin. It has been sent to the Natural History Museum of Berlin.

THE additions to the Zoological Society's Gardens during the past week include a Green Monkey (*Cercopithecus callitrichus* ♀) from West Africa, presented by Mrs. Holden; a Rhesus Monkey (*Macacus rhesus* ♂) from India, presented by Mr. Herbert C. Oates; two Californian Quails (*Callipepla californica* ♂ ♂) from California, presented by Mrs. Fanny Lloyd; a Lesser Kestrel (*Tinnunculus cenchris*) European, presented by Mr. Harold Hanauer, F.Z.S.; two Æsculapian Snakes (*Coluber æsculapii*) from Germany, presented by Mr. P. L. Sclater, F.R.S.; seven Slender-fingered Frogs (*Leptodactylus pentodactylus*) from Dominica, W.I., presented by Dr. H. A. A. Nicholls; two American Black Bears (*Ursus americanus* ♂ ♀) from North America, a Grey Parrot (*Psittacus erithacus*, white var.) from West Africa, an Æsculapian Snake (*Coluber æsculapii*) from Germany, a Tabuan Parrakeet (*Pyrrhulopsis tabuensis*) from the Fiji Islands, deposited.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JULY* 29—*AUGUST* 4.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on July 29

Sun rises, 4h. 22m.; souths, 12h. 6m. 11·1s.; sets, 19h. 51m.: right asc. on meridian, 8h. 36·5m.; decl. 18° 36′ N. Sidereal Time at Sunset, 16h. 23m.

Moon (at Last Quarter July 30, 20h.) rises, 22h. 31m.*; souths, 4h. 46m.; sets, 11h. 13m.: right asc. on meridian, 1h. 14·8m.; decl. 2° 21′ N.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	2 48 ...	10 44 ..	18 40 ...	7 13·7 ...	20 26 N.
Venus ...	4 44 ...	12 28 ...	20 12 ...	8 57·9 ...	18 31 N.
Mars ...	12 43 ...	17 34 ...	22 25 ...	14 5·1 ...	13 55 S.
Jupiter ...	14 42 ...	19 6 ...	23 30 ...	15 37·8 ...	18 39 S.
Saturn ...	4 36 ...	12 19 ...	20 2 ...	8 49·1 ...	18 29 N.
Uranus ...	10 42 ...	16 21 ...	22 0 ...	12 51·7 ...	4 52 S.
Neptune..	23 44* ...	7 31 ...	15 18 ...	4 0·8 ...	18 57 N.

* Indicates that the rising is that of the preceding evening.

Comet Sawerthal.

		Right Ascension.	Declination.
July.	h.	h. m.	° ′
29 ...	0 ...	1 6·4 ...	53 4 N.
Aug.			
2 ...	0 ...	1 4·6 ...	53 37

July. h.
29 ... 21 ... Mercury at greatest elongation from the Sun 19° west.

Aug.
2 ... 1 ... Saturn in conjunction with the Sun.

Occultations of Stars by the Moon (visible at Greenwich).

July.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.	
			h. m.	h. m.	°	°
31	*f* Tauri	4	23 44	0 22†	25	297
Aug.						
2	B.A.C. 1351	6½	2 20	3 6	112	205
2	63 Tauri	6	2 44	near approach	158	—
4	χ^3 Orionis	6	1 52	2 4	346	319
4	χ^4 Orionis	5	1 58	2 36	109	195

† Occurs on the following morning.

Variable Stars.

Star.	R.A.	Decl.		h. m.	
	h. m.	° ′			
U Cephei	0 52·4	81 16 N.	July 30,	20 30	*m*
			Aug. 4,	20 9	*m*
Algol	3 0·9	40 31 N.	July 31,	2 24	*m*
			Aug. 2,	23 13	*m*
U Monocerotis	7 25·5	9 33 S.	,, 1,		*M*
U Canis Minoris	7 35·3	8 39 N.	July 31,		*m*
U Virginis	12 45·4	6 10 N.	Aug. 1,		*M*
R Hydræ	13 23·6	22 42 S.	,, 1,		*m*
δ Libræ	14 55·0	8 4 S.	,, 2,	23 52	*m*
U Coronæ	15 13·6	32 3 N.	,, 2,	4 0	*m*
U Ophiuchi	17 10·9	1 20 N.	July 29,	3 36	*m*
			and at intervals of	20 8	
W Sagittarii	17 57·9	29 35 S.	Aug. 1,	1 0	*m*
Z Sagittarii	18 14·8	18 55 S.	,, 4,	1 0	*M*
U Sagittarii	18 25·3	19 12 S.	July 30,	0 0	*M*
S Vulpeculæ	19 43·8	27 1 N.	Aug. 4,		*m*
η Aquilæ	19 46·8	0 43 N.	,, 2,	3 0	*M*
R Sagittæ	20 9·0	16 23 N.	,, 2,		*m*
X Cygni	20 39·0	35 11 N.	,, 4,	1 0	*m*
T Vulpeculæ	20 46·7	27 50 N.	,, 2,	2 0	*M*
			,, 3,	3 0	*m*
δ Cephei	22 25·0	57 51 N.	July 30,	2 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
	°	°	
Near δ Andromedæ	7	31 N.	Swift; streaks.
The *Perseids*	33	55 N.	Swift; streaks.
Near β Persei	48	42 N.	Very swift; streaks.
	350	52 N.	Very swift.

GEOGRAPHICAL NOTES.

THE *Mittheilungen* of the Vienna Geographical Society for June has a paper by Dr. Hans Meyer on the German East African possessions which is likely to attract some attention at the present juncture. No attempt is made to give either the area or the population of this ill-defined region, which, however, is stated to comprise the central section of the East African coastlands, terraces, and plateaux for a distance north and south of about 550 geographical miles, and 150 east and west between the Swaheli coast and the water-parting towards the Congo basin. It is conterminous towards the north with the new British East African protectorate, from which it is separated by a conventional line passing from Lake Victoria Nyanza in an oblique direction along the north foot of Mount Kilima-Njaro to the coast at about 5° S. lat. below Mombasa. Southwards the frontier is marked by the Rovuma River, and another conventional line running thence west to Lake Nyassa, while on the east side it is made to reach the Indian Ocean, thus apparently absorbing the ten mile zone of coastlands reserved to the Sultan of Zanzibar by the Anglo-German Convention of October 29, 1886. It is described as orographically and hydrographically the most diversified region in the whole of Africa, including within its limits the highest summit (Kilima-Njaro) as well as the head-waters of streams flowing north to the Nile, west to the Congo, and south to the Zambesi basin. Hence it presents a great variety of climate and vegetation, but nevertheless, except in a few favoured spots, it is not to be compared in productiveness with the rich tropical lands of the Eastern Archipelago. Its prospects as a future field of German colonial enterprise are spoken of in depressing terms. Both servile and free labour in the interior are stated to be alike impracticable, and for the present at least it will be impossible to develop any great commercial activity except on the fertile and more thickly-peopled, but also mostly fever-stricken coastlands. Hence a foundation for the future development of the colony is stated to have been laid by the recently-accomplished transfer of the administration of the seaboard from the Sultan of Zanzibar to the German East African Company's agents. But it is added that even here, without State aid, it will be difficult successfully to compete with their English rivals, who have been longer in possession of the field, and who have at their disposal more capital and resources of all kinds.

ELECTRICAL NOTES.

KUNDT (*Phil. Mag.*, July 1888) has determined experimentally that there exists a proportionality between the velocity of light, electric conductivity, and conduction of heat in metals. The velocity of red light is proportionately as follows—

Silver	100	Iron	14·9
Gold	71	Nickel	12·4
Copper (impure)	60	Bismuth (crystallized)	10·3
Platinum	15·3		

The order is the same for heat and electricity. These figures were obtained in each instance by determining the index of refraction of each metal, which is the ratio of the velocity of light *in vacuo* to its velocity in the metal. The actual indices obtained were, for red light—

Silver	0·27	Iron	1·81
Gold	0·38	Nickel	2·17
Copper	0·45	Bismuth	2·61
Platinum	1·76		

Thus the velocity of light in silver is ten times that in bismuth. How is the velocity of light affected by temperature? and how is it changed by a magnetic field? Kundt proposes to examine these points.

PROF. ELIHU THOMSON (U.S.A.) states that he has observed as many as six lightning-flashes very quickly following each other along the same path. He kept his head rapidly wagging during a thunderstorm, and his eyes fixed in one direction. Most people have experienced a peculiar throbbing during a flash of lightning; and a succession of rapid currents, sometimes forming letters, are observed on telegraphs. A lightning discharge may therefore have the same oscillatory character as the discharge of a Leyden jar. But no trace of such an effect is visible in the photographs of lightning-flashes unless it be the mysterious dark flashes that have been recorded.

CHAPERON AND MERCADIER (*Comptes rendus*, cvii., June 4, 1888) have shown that the periodic incidence of rays of light upon a cell of silver sulphide, H_2SO_4, and bright silver produces sounds in a telephone by the corresponding variations of E.M.F. They call the effect electro-chemical radiophony. The cell copper-oxide, sodium chloride, copper also forms an electro-chemical radiophone.

E. G. ACHESON (*Electrical World*, N.Y., July 7, 1888) has made some very useful measurements on the sparking distance in air of alternate currents used in electric light working. He finds that it varies with the capacity of the circuit and with the *cube of the E.M.F.* It is expressed by

$$d = \frac{E^3K}{a},$$

d being the sparking distance in inches, E and K being in B.A. units, and *a* a constant = 135. Two thousand volts, with 0·0032 microfarad in circuit, sparked about 0·2 inch, and 1000 volts about 0·02 inch. These results are very different from those obtained by Warren De la Rue with his great battery, who found that with direct currents 1200 volts sparked across 0·012 inch and 2400 volts across 0·021 inch, but the capacity present is not given.

ANOTHER of Mr. H. Tomlinson's remarkable papers appears in the *Phil. Mag.* for July. The chief remarkability of these papers consists in their diffuseness. It is almost impossible to extract the new facts out of them. His terms are peculiar. What is "the specific heat of electricity" which changes sign at varying temperatures? The conclusion of this long paper appears to be that the temperature at which permanent magnetism begins to suddenly disappear is not the temperature at which permanent torsion begins to suddenly disappear. We find the mechanical qualities, viz. hardness, elasticity, linear expansion, internal friction, tensile strength, molecular structure, torsion, &c., of iron, steel, and nickel inextricably mixed up with magnetic susceptibility and retentiveness, electric resistance and thermo-electric conditions, specific and latent heat, and varying temperatures.

THE PROGRESS OF THE HENRY DRAPER MEMORIAL.[1]

THE additional facilities provided by Mrs. Draper have permitted a considerable extension of this research during the past year. The 11-inch refractor belonging to Dr. Draper, and the 8-inch photographic telescope provided by the Bache Fund, have been kept at work throughout every clear night. The 28-inch and 15-inch reflectors constructed by Dr. Draper have been moved to Cambridge, and the first of these instruments is placed in a building surmounted by a dome constructed for the purpose. Experiments are now in progress with it, and it will probably soon be employed regularly. Four assistants take part in making the photographs, one of whom comes to the Observatory every clear night about midnight, and keeps the 8-inch and 11-inch telescopes in use until interrupted by the morning twilight. Five ladies have been employed in the measurements and reductions.

The various investigations now in progress are described in detail below. The first three of these, including the photographic work of the 8-inch and 11-inch telescopes, will be finished in about a year. It is accordingly proposed in the autumn of 1889 to send an expedition to the southern hemisphere, probably to Peru, and there complete the work to the South Pole. As only about one quarter part of the sky is too far south to be conveniently observed at Cambridge, it is expected that the photographs needed to cover this portion of the sky could be obtained in two years. Each investigation could thus be extended to all parts of the sky upon the same system.

An important advance has been made by the recent improvements in the manufacture of dry plates. The M. A. Seed Company of St. Louis have endeavoured to comply with our request for more sensitive plates, and have gradually increased their sensitiveness, so that they now furnish us with plates measuring 27 on their scale, while a year ago the most sensitive plates were only numbered 21. As a result, stars nearly a magnitude fainter can be photographed, and the number of objects which can be examined is nearly doubled. A careful study will shortly be made, by the help of the instruments described below, of the most sensitive plates obtainable. It is hoped that makers of very sensitive plates will send specimens to Cambridge for trial. The demand for increased sensitiveness is so great not only here, but at all other observatories where stellar photography is carried on, that a real improvement would be widely appreciated.

Various improvements have been made in the methods of detecting defects in the photographic processes. Each plate, when it is taken from its box, is exposed to a standard light for exactly one second. The portion of the flame of an oil lamp shining through a small circular aperture constitutes the standard light. The exposure is made for a second by means of a pendulum, which allows the light to shine on the plate for this interval through a small square aperture. When the plate is developed, a dark square appears near its edge, whose intensity measures the sensitiveness of the plate, and also serves to detect any defect in its development. Passing clouds, or a variation in the clearness of the sky, are detected by an instrument called the Pole-Star recorder. It consists of a telescope with a focal length of about 3 feet, placed parallel to the earth's axis. An image of the Pole-Star is formed by it, and allowed to fall upon a sensitive plate, describing an arc of a circle, which is interrupted whenever clouds pass. The plate is changed every day, and the instrument is closed automatically by an alarm-clock every morning before the twilight begins. Much trouble is experienced from the deposition of moisture on the objectives of the photographic telescopes, on account of their exposure to a large portion of the sky. The failure of some of the earlier plates may be due to this cause. Moisture is now carefully looked for, and, if detected, removed by gently heating the objectives. Another test of the quality of the plates consists in occasionally exposing a plate in the 8-inch telescope to the circumpolar sky, first with and then without the prism. The trails of the stars near the Pole and the spectra of the brighter stars are thus photographed. A comparison of the intensity of these images tests the condition of the air, the instrument, and the plates.

[1] Extracted from the "Second Annual Report of the Photographic Study of Stellar Spectra conducted at the Harvard College Observatory," Edward C. Pickering, Director. With 2 Plates. (Cambridge: John Wilson and Son, University Press, 1888.)

The various investigations will now be described in order, as in the last Report.

1. *Catalogue of Spectra of Bright Stars.*—The spectra of all the brighter stars have been photographed with the 8-inch telescope, giving an exposure of at least five minutes to each. Each plate contains from two to four regions 10° square. The plates representing the region north of −25° were divided into three series, which may be distinguished as polar, zenith, and equatorial. Each region is contained on two plates, and the work has been repeated in two successive years, so that at least four photographs should be obtained of all the brighter stars. If a plate proved poor, it was repeated, so that the very bright stars will appear in several plates. The photographic portion of this work was finished last November. If no plates had been repeated, 36 polar, 72 zenith, and 72 equatorial plates would have been required each year, or 360 in all. The actual numbers of plates taken and measured were 46, 120, and 93, total 259, the first year; and 61, 209, and 104, total 374, the second year; or 633 in all. In the later work the number of zenith plates was doubled, to avoid the confusion arising when several exposures were made on a single plate. The numbers of spectra measured on these plates were 2381, 3314, and 2618, total 8313, the first year; and 7199, 8217, and 4074, total 19,490, the second year. Two plates covering the immediate vicinity of the North Pole contain 150 spectra. The whole number of spectra is therefore 27,953. The measurement and identification of this large number of spectra has occupied the greater portion of the time of the corps of computers. Each plate to be measured was placed on a stand, and the light of the sky was reflected through it by means of a mirror. The approximate co-ordinates of each spectrum in turn were then read off, and a careful description of the spectrum was given. Besides the usual division into types, each additional line visible was recorded both as regards its position and intensity. The photographic intensity of the brightest portion of each spectrum was also measured by means of a photographic plate, dark at one end and light at the other, like a wedge of shade glass. When the spectra show sudden changes in brightness, additional measurements are made. This portion of the work is complete only for the polar plates and about 62 of the other plates, including 12,574 spectra. The identification of the spectra is effected either by computation from its co-ordinates, or by laying the plate upon the maps of the "Durchmusterung," the scale being the same for both. All the plates have, however, been checked by the latter method. The names of the stars are then taken from the "Harvard Photometry," "Uranometria Argentina," or "Durchmusterung," according to their brightness and declination. Their places are next brought forward to 1900, the epoch of the final catalogue. As the intensity of the photograph of a given spectrum will vary greatly with the sensitiveness of the plate, the clearness of the air, and the rate of the driving-clock, all must be reduced to the same system. The scale of the "Harvard Photometry" is adopted for this purpose. The most prevalent spectra are those of the first type, in which the K line is too faint to be visible. After applying a correction for the declination of the stars, the brightness of all such spectra on each plate is compared with the photometric magnitudes. A correction is thus derived for each plate, which is applied to all the spectra upon it. The effect of colour, so far as it varies with the type of spectrum, is thus eliminated. It is possible that, owing to variations in temperature, or other causes, some stars may be redder or bluer than others having the same type of spectrum.

2. *Catalogue of Spectra of Faint Stars.*—Until the photographs required for the research mentioned above were completed, the time of the 8-inch telescope was mainly devoted to them. Since then it has been used principally in photographing the fainter stars. An exposure of one hour is given to each portion of the sky, a region 10° square being included upon each plate. Stars as far south as −25° can be advantageously photographed at Cambridge, and the plan proposed covers this region. The plates overlap, so that the region north of −20° will appear on at least two plates. The southern stars are only photographed when the sky is unusually clear. Each plate is examined, and, if unsatisfactory, the work is repeated. If all were good, 650 plates would be required. Thus far, 606 plates have been taken, covering 339 of the desired regions. As the time of the computers has been mainly devoted to the first investigation mentioned above, the greater portion of these plates have not been measured or reduced. The total number measured is 105 plates, containing 6931 spectra, of which 94 plates and 6293 spectra

have been reduced. The form of reduction and publication will be similar to the catalogue of bright stars, except that it will be convenient to retain the "Durchmusterung" numbers and places, arranging the stars in the order of the zones in that catalogue. It is hoped that the photographs for this investigation will be nearly all taken by the autumn of 1888, and the remainder during the following year. To provide for a possible increase in sensitiveness of the plates, precedence is given to those completely covering the sky once, the alternate plates, covering the sky the second time, being taken later. The actual improvement in the plates shows itself by an increase in the number of spectra in this second series of plates. In some cases over three hundred stellar spectra appear on a single plate.

3. *Detailed Study of the Spectra of the Brighter Stars.*—These spectra are obtained by placing four prisms, having an angle of about 15°, and each nearly a foot square, over the object-glass of the 11-inch telescope, as described in the last Report. The increased sensitiveness of the plates has greatly increased the number of stars bright enough to produce a satisfactory image in this way. The white stars of the first type give good images when no brighter than the fourth magnitude. These spectra are about 4 inches in length. An improvement has been made in the method of enlargement with a cylindrical lens described in the last Report. When such a lens was used with an enlarging lens having a small aperture, the width of the spectrum was greatly reduced; with a large aperture, the best definition could not be attained. A slit perpendicular to the axis of the cylindrical lens is accordingly placed over it. This reduces the aperture in one direction so that the definition of the lines is good, without affecting the width of the spectrum. Slow plates are also used in the enlargements to increase the contrast. Much more brilliant spectra are thus obtained.

4. *Faint Stellar Spectra.*—As stated above, the 28-inch reflector constructed by Dr. Draper is now ready for use. The difficulties commonly encountered in the use of a large reflector have been met, and it is hoped successfully overcome. A spectroscope has been devised for this instrument which will give a dispersion about equal to that employed in the first and second of the researches described above. As the area of the aperture of this telescope is about eleven times that of the 8-inch telescope, it is hoped that much fainter stars can be photographed with it. A study will be made of the spectra of the variable stars of long period, of the banded stars, and of other objects having peculiar spectra.

But little progress has been made with the other investigations proposed, including the reduction to wave-lengths, and the study of the approach and recession of the stars. It seemed best to concentrate our work on the researches described above, undertaking the other investigations as soon as time permitted.

The investigations described above are illustrated by a plate. A special study was made of the spectrum of the variable star β Persei. A variation in this spectrum would have an important bearing on the theory that the diminution in light is due to an interposed dark satellite. Spectra of this star at minimum were first obtained with one prism. With the increased sensitiveness of the plates more prisms were tried, until finally good spectra were obtained with all four prisms even when the star was at its minimum. At first it was thought that a variation was detected in the spectrum, but this change was not confirmed under more favourable circumstances. The spectrum of this star on February 6, 1888, when at its full brightness, is contrasted in the plate with the spectrum on February 9, 1888, when the star was at its minimum. A careful inspection of the original negatives failed to show any differences in the spectra. Twenty lines are visible at minimum, all of which are seen at maximum. The spectrum of α Orionis is also given. Before the recent increase in the sensitiveness of the photographic plates, satisfactory photographs could not be obtained of the spectrum of this star, on account of its red colour.

INFLUENCE MACHINES.[1]

I HAVE the honour this evening of addressing a few remarks to you upon the subject of influence machines; and the manner in which I propose to treat the subject is to state as shortly as possible, first, the historical portion, and afterwards to point out the prominent characteristics of the later and the more commonly known machines.

[1] Lecture delivered at the Royal Institution, by Mr. J. Wimshurst, on April 27, 1888.

In 1762, Wilcke described a simple apparatus which produced electrical charges by influence, or induction, and following this the great Italian man of science, Alexander Volta, in 1775 gave the electrophorus the form which it retains to the present day. This apparatus may be viewed as containing the germ of the principle of all influence machines yet constructed.

Another step in the development was the invention of the doubler by Bennet in 1786. He constructed metal plates which were thickly varnished, and were supported by insulating handles, and which were manipulated so as to increase a small initial charge. It may be better for me to here explain the process of building up an increased charge by electrical influence, for the same principle holds in all of the many forms of influence machines.

This Volta electrophorus, and these three blackboards, will serve for the purpose. I first excite the electrophorus in the usual manner, and you see that it then influences a charge in its top plate; the charge in the resinous compound is known as negative, while the charge induced in its top plate is known as positive. I now show you by this electroscope, that these charges are unlike in character. Both charges are, however, small, and Bennet used the following system to increase them.

Let these three boards represent Bennet's three plates. To plate No. 1 he imparted a positive charge, and with it he induced a negative charge in plate No. 2. Then with plate No. 2 he induced a positive charge in plate No. 3. He then placed the plates Nos. 1 and 3 together, by which combination he had two positive charges within practically the same space, and with these two charges he induced a double charge in plate No. 2. This process was continued until the desired degree of increase was obtained. I will not go through the process of actually building up a charge by such means, for it would take more time than I can spare.

In 1787, Carvallo discovered the very important fact that metal plates when insulated always acquire slight charges of electricity; following up those two important discoveries of Bennet and Carvallo, Nicholson in 1788 constructed an apparatus, having two disks of metal insulated and fixed in the same plane. Then, by means of a spindle and handle, a third disk, also insulated, was made to revolve near to the two fixed disks, metallic touches being fixed in suitable positions. With this apparatus he found that small residual charges might readily be increased. It is in this simple apparatus that we have the parent of influence machines, and as it is now a hundred years since Nicholson described this machine in the Phil. Trans., I think it well worth showing a large-sized Nicholson machine at work to-night.

In 1823, Ronalds described a machine in which the moving disk was attached to and worked by the pendulum of a clock. It was a modification of Nicholson's doubler, and he used it to supply electricity for telegraph working. For some years after these machines were invented no important advance appears to have been made, and I think this may be attributed to the great discoveries in galvanic electricity which were made about the commencement of this century by Galvani and Volta, followed in 1831 to 1857 by the magnificent discoveries of Faraday in electro-magnetism, electro-chemistry, and electro-optics, and no real improvement was made in influence machines till 1860, in which year Varley patented a new form of machine.

In 1865 the subject was taken up with vigour in Germany by Toepler, Holtz, and other eminent men. In 1866, Bertsch invented a machine, but not of the multiplying type; and in 1867, Sir William Thomson invented a form of machine, which, for the purpose of maintaining a constant potential in a Leyden jar, is exceedingly useful.

The Carré machine was invented in 1868, and in 1880 the Voss machine was introduced, since which time the latter has found a place in many laboratories. It closely resembles the Varley machine in appearance, and the Toepler machine in construction.

In condensing this part of my subject, I have had to omit many prominent names and much interesting subject-matter, but I must state that, in placing what I have before you, many of my scientific friends have been ready to help and to contribute; and, as an instance of this, I may mention that Prof. Silvanus P. Thompson at once placed all his literature and even his private notes of reference at my service.

I will now endeavour to point out the more prominent features of the influence machines which I have present, and, in doing so, I must ask a moment's leave from the subject of my lecture to show you a small machine made by that eminent worker, Faraday, which, apart from its value as his handiwork, so closely brings us face to face with the imperfect apparatus with which he and others of his day made their valuable researches.

The next machine which I take is a Holtz. It has one plate revolving, the second plate being fixed. The fixed plate, as you see, is so much cut away that it is very liable to breakage. Paper inductors are fixed upon the back of it, while opposite the inductors, and in front of the revolving plate, are combs. To work the machine (1) a specially dry atmosphere is required; (2) an initial charge is necessary; (3) when at work the amount of electricity passing through the terminals is great; (4) the direction of the current is apt to reverse; (5) when the terminals are opened beyond the sparking distance the excitement rapidly dies away; (6) it does not part with free electricity from either of the terminals singly.

It has no metal on the revolving plates, nor any metal contacts; the electricity is collected by combs which take the place of brushes, and it is the break in the connection of this circuit which supplies a current for external use. On this point I cannot do better than quote an extract from p. 339 of Sir William Thomson's "Papers on Electro-statics and Magnetism," which runs:—"Holtz's now celebrated electric machine, which is closely analogous in principle to Varley's of 1860, is, I believe, a descendant of Nicholson's. Its great power depends upon the abolition by Holtz of metallic carriers and metallic make-and-break contacts. It differs from Varley's and mine by leaving the inductors to themselves, and using the current in the connecting arc."

In respect to the second form of Holtz machine I have very little information, for since it was brought to my notice nearly six years ago I have not been able to find either one of the machines or any person who had seen one. It has two disks revolving in opposite directions; it has no metal sectors and no metal contacts. The "connecting arc circuit" is used for the terminal circuit. Altogether I can very well understand and fully appreciate the statement made by Prof. Holtz in *Uppenborn's Journal* of May 1881, wherein he writes that "for the purpose of demonstration I would rather be without such machines."

The first type of Holtz machine has now in many instances been made up in multiple form, within suitably constructed glass cases, but when so made up great difficulty has been found in keeping each of the many plates to a like excitement. When differently excited, the one set of plates furnished positive electricity to the comb, while the next set of plates gave negative electricity: as a consequence no electricity passed the terminals.

To overcome this objection, to dispense with the dangerously cut plates, and also to better neutralize the revolving plate throughout its whole diameter, I made a large machine having twelve disks 2 feet 7 inches in diameter, and in it I inserted plain rectangular slips of glass between the disks, which might readily be removed; these slips carried the paper inductors. To keep all the paper inductors on one side of the machine to a like excitement, I connected them together by a metal wire. The machine so made worked splendidly, and your late President, Mr. Spottiswoode, sent on two occasions to take note of my successful modifications. The machine is now ten years old, but still works splendidly. I will show you a smaller-sized one at work.

The next machine on which I make observations, is the Carré. It consists essentially of a disk of glass which is free to revolve without touch or friction. At one end of a dicmeter it moves near to the excited plate of a frictional machine, while at the opposite end of the dicmeter is a strip of insulating material, opposite which, and also opposite the excited amalgam plate, are combs for conducting the induced charges, and to which the terminals are metallically connected; the machine works well in ordinary atmosphere, and certainly is in many ways to be preferred to the simple frictional machine. In my experiments with it I found that the quantity of electricity might be more than doubled by adding a segment of glass between the amalgam cushions and the revolving plate. The current in this type of machine is constant.

The Voss machine has one fixed plate and one revolving plate. Upon the fixed plate are two inductors, while on the revolving plate are six circular carriers. Two brushes receive the first portions of the induced charges from the carriers, which portions are conveyed to the inductors. The combs collect the remaining portion of the induced charge for use as an outer circuit, while the metal rod with its two brushes neutralizes the plate surface in a line of its diagonal diameter. When at work it supplies a considerable amount of electricity. It is self-exciting in ordinary dry atmosphere. It freely parts with its electricity from either terminal, but when so used the current frequently changes its direction, hence there is no certainty that a full charge has been obtained, nor whether the charge is of positive or negative electricity.

I next come to the type of machine with which I am more closely associated, and I may preface my remarks by adding that the invention sprang solely from my experience gained by constantly using and experimenting with the many electrical machines which I possessed. It was from these I formed a working hypothesis which led me to make the small machine now before you. The machine is unaltered. It excited itself when new with the first revolution. It so fully satisfied me with its performance that I had four others made, the first of which I presented to this Institution. Its construction is of the simplest character. The two disks of glass revolve near to each other, and in opposite directions. Each disk carries metallic sectors; each disk has its two brushes supported by metal rods, the rods to the two plates forming an angle of 90° with each other. The external circuit is independent of the brushes, and is formed by the combs and terminals.

The machine is self-exciting under all conditions of atmosphere, owing probably to each plate being influenced by, and influencing in turn its neighbour, hence there is the minimum surface for leakage. When excited, the direction of the current never changes; this circumstance is due probably to the circuit of the metallic sectors and the make-and-break contacts always being closed, while the combs and the external circuit are supplemental, and for external use only. The quantity of electricity is very large and the potential high. When suitably arranged, the length of spark produced is equal to nearly the radius of the disk. I have made them from 2 inches to 7 feet in diameter, with equally satisfactory results.

I have also experimented with the cylindrical form of the machine; the first of these I made in 1882, and it is before you. The cylinder gives inferior results to the simple disks, and is more complicated to adjust. You notice I neither use nor recommend vulcanite, and it is perhaps well to caution my hearers against the use of that material for the purpose, for it warps with age, and when left in the daylight it changes and becomes useless.

I have now only to speak of these larger machines. They are in all respects made up with the same plates, sectors, and brushes as were used by me in the first experimental machines, but for convenience sake they are fitted in numbers within a glass case.

This machine has eight plates of 2 feet 4 inches diameter; it has been in the possession of the Institution for about three years.

This large machine, which has been made for this lecture, has twelve disks, each 2 feet 6 inches in diameter. The length of spark from it is $13\frac{5}{8}$ inches.

During the construction of the machine every care was taken to avoid electrical excitement in any of its parts, and after its completion several friends were present to witness the fitting of the brushes and the first start. When all was ready the terminals were connected to an electroscope, and the handle was moved, so slowly that it occupied thirty seconds in moving one half revolution, and at that point violent excitement appeared.

The machine has now been standing with its handle secured for about eight hours; no excitement is apparent, but still it may not be absolutely inert; of this each one present may judge, but I will connect it with this electroscope, and then move the handle slowly, so that you may see when the excitement commences and judge of its absolutely trustworthy behaviour as an instrument for public demonstration. I may say that I have never under any condition found this type of machine to fail in its performance.

I now propose to show you the beautiful appearances of the discharge, and then in order that you may judge of the relative capabilities of each of these three machines, we will work them all at the same time.

The large frictional machine which is in use for this comparison is so well known to you that a better standard could not be desired.

In conclusion I may be permitted to say that it is fortunate I had not read the opinion of Sir William Thomson and Prof.

Holtz, as quoted in the earlier part of my lecture, previous to my own practical experiments. For had I read such opinions from such authorities I should probably have accepted them without putting them to practical test. As the matter stands I have done those things which they said I ought not to have done, and I have left undone those which they said I ought to have done, and by so doing I think you must freely admit that I have produced an electric generating machine of great power, and have placed in the hands of the physicist, for the purposes of public demonstration, or original research, an instrument more trustworthy than anything hitherto produced.

NOTE ON THE TARPON OR SILVER KING (MEGALOPS THRISSOIDES).

THE genus Megalops belongs to the family Clupeidæ, and, amongst other features, is characterized, according to Dr. Günther,[1] by an oblong compressed body, the presence of a narrow osseous lamella attached to the mandibular symphysis and lying between the halves of the mandible. Further, the latter is prominent, the intermaxillary short, the maxillary forming the lateral part of the mouth. There are bands of villiform teeth on the jaws, vomer, palatines, pterygoid, tongue, and base of skull.

The interest in the species above-mentioned has been considerably increased of late by the fact that the huge fish (between 5 and 6 feet in length, and weighing from 90 to 150 pounds) can be caught by rod and line, and I am much indebted to Lady Playfair for giving me all the information she had obtained on the subject through her father and Mr. W. G. Russell of Boston, United States.

The tarpon (*Megalops thrissoides*) frequents the Atlantic shores of North America, and is especially found "on the western or Gulf coast of Southern Florida, haunting the shallow bays and creeks inside the bars and keys which stretch along that coast; and the fishes are supposed to enter by the passes from the outer Gulf.[2]

"In shape the tarpon somewhat resembles the salmon, but, as becomes one of the herring tribe, it is deeper and less rounded, and the head is larger, the scales (cycloid) are thick and large, more than an inch in diameter" (a fine scale sent by Lady Playfair measures 2¼ inches both in antero-posterior and transverse diameter), "and the exposed portion is of a bright silvery hue, indeed it looks as if it had been dipped in silver and burnished: hence the name 'silver king.' I have seen specimens weighing from 50 to 137 pounds, and have heard of none above 150 pounds.

"The tarpon has always been upon the Gulf coast, but was formerly captured, as the sword-fish is, by the harpoon. In 1885, however, a Mr. Wood undertook successfully to secure the fish by rod and reel. . . . About 150 have been caught in this manner during the seasons 1885 and 1886, the time being in March and April, perhaps a little earlier in a warm season: after April it is too hot for fishing.

"The fish is caught on the edge of the channels in 15 to 25 feet of water with a bait of (half a) mullet. The rod should be very stiff, not more than 9 feet in length, such as is used for large sea-bass, and the line strong, but fine enough to carry 200 to 250 yards on the reel, which must therefore be large and heavy. A snood or gauging of about 3 feet of cod-line, copper-wire, or chain, should be fixed to the hook[3] as the dental apparatus of the fish efficiently combines a file and shears, with which even a double cod-line may be frayed or worn off, or severed without a sensible strain.

"The tarpon takes the bait lying on the bottom, and moves off, swallowing it, until he is struck, and the moment he feels the hook he is out of the water, perhaps 3 or 6 feet in the air, shaking his head fiercely—as does the black bass—to disengage the hook, and then begins such a fight as, I believe, no other game fish ever shows. It frequently leaps with a clean breach twenty times before the game is over, and so close that it occasionally sends a douche over the boatmen; while in one instance a large one made a run of 100 yards, the whole of which was a succession of frantic leaps and plunges, leaving a wake like that of a steamer. The same fish towed my boat, with three men in it, about two miles, and, after more than an hour's hard fight, ended by three huge leaps out of the water amongst some mangrove-trees, the oysters on the roots of which cut my line, so that we parted company after a close and protracted intimacy."

[1] "Introduction to Fishes," pp 661–62.

[2] Extracted from a description (from personal observation) by Mr. W. G. Russell, of Boston.

[3] Described elsewhere as "an O'Shaughnessy knobbed 10–0 hook."

There is little doubt, from the foregoing remarks, that the splendid sport of tarpon-fishing must make it most fascinating. In April 1887, indeed, a single rod caught nine fish in eleven days, two of them weighing respectively 151 and 149 pounds, and in length 6 feet 4 inches, and 6 feet 5 inches. These were taken at Punta Rassa on the western coast of Florida, the total weight of the catch being 1042 pounds, or an average of about 116 pounds for each. The tarpon, like others of its tribe, has the advantage also of being good food. W. C. McINTOSH.

SCIENTIFIC SERIALS.

Bulletins de la Société D'Anthropologie de Paris, tome dixième, 4e fascicule, 1887.—This closing number for the last year enumerates the various presentations made to the Society since the previous publication of the Bulletins. Among the recent communications attention is due to M. Boban's report of the interesting collection of North American flint instruments presented to the Society by the Smithsonian Institution. They appear to be almost identical with those existing in Europe, and belonging to the Stone Age.—M. Verneau, on presenting various stone instruments from the Canary Isles, drew attention to their rude forms, due, he believes, to the relatively brittle character of the basalt and obsidian from which they were cut. The few specimens of polished stone belong only to Gomère and Canary Proper, and are, therefore, conjectured to have been introduced by some of the numerous North African invaders who landed on those islands.—M. André Sanson's paper on experimental craniology in reference specially to domestic animals, which he considers under two cephalic types only, viz. the dolicocephalic and the brachycephalic, is directed against the systems of craniometry and anthropometry at present in vogue. M. Fauvelle took a leading part in the discussion to which the paper gave rise, and gave his views in regard to the value of the cephalic index, which he considered to have been greatly overestimated by Broca and his followers. These remarks, and the refutation of Broca by M. Topinard, form, with M. Sanson's paper, a complete exposition of the various views maintained in different provinces of anthropological science in France.—Report on the various papers presented by competitors for the Godard Prize in 1887, by M. Moudière. — On aphasia and its history since the original observations of Broca, by M. M. Duval.—On the distinctive characteristics of the human brain considered from a morphological point of view, by M. le Dr. S. Pozzi.—On a case of supernumerary digits on the cubital margin of each hand, by Dr. Béranger.—On the morphological variability of the muscles under the influence of functional variations, by Mme. Clémence Royer.—On the abnormal elongation of the cuboides, accompanied by the pressure of a round pronator in a horse, by M. E. Cuyer.—On the tumulus of Kerlescan at Carnac, by M. Gaillard. The remains of this interesting monument, with its double dolmens similar to the covered allées, known as "Hunebeds" in Holland, were first described in 1860, since which time they have suffered so much from neglect and wanton injury that M. Gaillard is making a strong appeal to the Government for their protection.—Note on the tumuli of a covered gallery, examined in 1887, near Montigny l'Engrain (Aisne), by M. Vauville, and report of the crania found there, and referred to the Furfooz men of the dolmen age, by Dr. Verneau. The preponderating character of these crania was their length and straightness. Several bore marks of cicatrised wounds.—On a Quaternary equidean, similar to the *kertag* of Kirghis, described by M. Poliakoff under the the name of *Equus Przewalskii*. The description of the kertag with its short and straight mane, its relatively large head and inferior height, corresponds remarkably well with the numerous representations of the Quaternary equidean found in different parts of Western Europe among the varied debris that mark the site of primæval settlements. In the Magdelian carvings found in the cavern at Arudy among mammoth bones, special prominence is given to the thin, rat-like character of the tail of the animal, a feature that is very marked in the kertag, which appears to be the nearest living representative of the horse of the Quaternary age.

Bulletin de l'Académie Royale de Belgique, May.—On the new elements of the orbit of Eucharis, by L. de Ball. Continuing his researches on the elements of this planet (181), the author here establishes two new normal positions by means of the observations made in 1886 and 1887. He also revises the positions of the comparison stars, and resumes the calculation of the perturbations caused by Jupiter, Saturn, and Mars, utilizing for the last named the results of Asaph Hall's observations on the satellites.—Contribution to the study of pulsation in the lower animal organisms, by Dr. De Bruyne. The results are given of the author's studies on the pulsating function of an encysted Protozoon obtained in abundance by culture, but of not yet determined family. From his minute observations on the formation, development, and action of the vesicle endowed with rhythmical motion, he concludes that this organ has no communication with the periphery, and has nothing to do with the digestive function, as is commonly supposed, but is a true organ of respiration and circulation, a heart and lung combined.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 31.—"The Conditions of the Evolution of Gases from Homogeneous Liquids." By V. H. Veley, M.A., University College, Oxford.

In Part I. an account is given of the effect of finely divided particles on the rate of evolution of gases resulting from chemical changes; in Part II. the phenomenon of initial acceleration, as also the effect of variation of pressure on the evolution of gases, is discussed; in Part III. the case of the decomposition of formic acid into carbonic oxide and water is investigated under constant conditions, other than those of the mass of reacting substances and of temperature.

Part I.—It is found that the addition of finely divided chemically inert particles increases the rate of evolution of gases from liquids in which they are being formed. The effect of these particles on the following chemical changes is investigated: (i.) the decomposition of formic acid yielding carbonic oxide; (ii.) the decomposition of ammonium nitrite in aqueous solution yielding nitrogen; (iii.) the reduction of nitric acid into nitric oxide by means of ferrous sulphate; (iv.) the decomposition of ammonium nitrate in a state of fusion producing nitrous oxide; and (v.) the decomposition of potassium chlorate in a state of fusion producing oxygen. The finely divided substances used are pumice, silica, graphite, precipitated barium sulphate and glass-dust.

Part II.—It is observed that, conditions of temperature remaining the same, the rate of evolution of a gas from a liquid is at first slow, then gradually increases until it reaches a maximum, and for some time constant, rate. From this point the rate decreases proportionally to the diminution of mass. This is observed in the cases of decomposition of formic acid, potassium ferrocyanide, and of oxalic acid by concentrated sulphuric acid, and in that of ammonium nitrate. It has previously been observed in the case of the decomposition of ammonium nitrite in aqueous solution. The same phenomenon repeats itself when the temperature is temporarily lowered and then raised to its former point, and also to a more marked degree when, temperature remaining the same, the superincumbent pressure is suddenly increased.

The reduction of pressure from one to a fraction of an atmosphere produces no *permanent* effect on the rate of evolution of a gas from a liquid; a decrease of pressure, however, produces *temporarily* an increase in the rate, and an increase of pressure conversely produces *temporarily* a decrease in the rate.

Part III.—The case of the decomposition of formic acid into carbonic oxide and water by diluted sulphuric acid is studied with the aid of an apparatus by means of which the temperature is kept constant within one-twentieth of a degree. It is shown that the rate of evolution of carbonic oxide is expressible by the following equation—

$$\log (\tau + t) + \log r = \log c,$$

in which τ is the time from the commencement of the observations; t is the interval of time from the moment of commencement, and that at which, conditions remaining the same, the interval of time required for unit change would have been *nil*; r is the mass at the end of each observation, and c is a constant. The results calculated by this hypothesis agree with those observed, whether the interval of time required for unit change is 30 or 960 minutes. The curve expressing the rate of chemical change in terms of mass is thus hyperbolic and illustrative of the law—

$$\frac{dr}{d\tau} = -\frac{r^2}{c},$$

which expresses the rate at which equivalent masses act upon one another; $1/c$ in each experiment is the amount of each unit mass which reacts with the other per unit of time, when a unit mass of each substance is present. Since, then, equivalent masses take part in the change, it is reasonable to suppose that at first an anhydride of formic acid is produced, which is subsequently decomposed into carbonic oxide and water.

The change may thus be compared to the production of ethyl formate from formic acid and alcohol, with which it shows several points of analogy.

June 14.—"The Electric Organ of the Skate. Structure and Development of the Electric Organ of *Raia radiata*." By J. C. Ewart, M.D., Regius Professor of Natural History, University of Edinburgh. Communicated by Prof. J. Burdon Sanderson, F.R.S.

The first part of this paper is chiefly devoted to a comparison of the electric organs of *Raia radiata*, *Raia batis*, and *Raia circularis*. It is shown that the organ in the species *radiata* differs in many respects from the organ in the two other species, and that an exhaustive study of its structure and development is likely to throw considerable light on the nature of electric organs generally, and also on the structure of the motor plates of muscles. While *Raia batis* may reach a length of over 180 cm., *Raia radiata* seldom measures more than 45 cm. from tip to tip, and is thus only about half the size of a large *Raia circularis*. In *Raia radiata* the electric organ is absolutely and relatively extremely small. In *Raia batis* the electric organ may be 60 cm. in length and 7 cm. in circumference at the centre, and extend from the skin to the vertebral column, but in an adult *Raia radiata* the organ is seldom over 13 cm. in length and 8 mm. in circumference, and the posterior two-thirds is confined to a narrow cleft between the skin and the great lateral muscles of the tail. Further, the organ of *Raia radiata* consists of minute shallow cups, which only remotely resemble the large well-formed electric cups of *Raia circularis*. In the latter species the various layers of the electric cup are readily comparable to the more important layers of the electric disk of *Raia batis*, but in *Raia radiata* the electric cup is little more than a muscular fibre, with one end expanded and slightly excavated to support a greatly enlarged motor plate, in which terminate numerous nerve-fibres. The striated layer of *Raia batis* and *circularis*, which consists of characteristic lamellæ, having an extremely complex arrangement, is entirely absent from *Raia radiata*, the electric layer is indistinct, and instead of a thick richly nucleated cortex, the cup is merely invested by a slightly thickened sarcolemma. Further, the tissue forming the shallow, thick-walled cup, both in its appearance and consistency, closely resembles an ordinary muscular fibre, while the long stem usually remains distinctly striated to its termination.

In the second part of the paper an account is given of the development of the electric cups of *Raia radiata*. It is shown that the rate of development compared with *Raia circularis*, but more especially with *Raia batis*, is extremely slow. The young *radiata* is nearly double the size of the *batis* embryo before the muscular fibres reach the "club" stage, and the long nearly uniform clubs, instead of at once developing into rudimentary cups as is the case in *batis*, assume the form of large Indian clubs. When the young skate reaches a length of about 35 cm., the long secondary clubs begin to expand anteriorly, and this expansion continues until a fairly well-moulded cup mounted on a long delicate stem is produced. But the process of conversion is scarcely completed when the skate has reached a length of 40 cm., *i.e.* when it has nearly reached its full size, for in the species *radiata* a length of 50 cm. is seldom if ever attained.

The cup-stage having been eventually reached, the stem, which for a time may still increase in length, is often compressed by two or more cups being closely applied together, and part of the rim of the cup may be slightly everted or projected forwards, but even in the largest specimens of *Raia radiata* examined there was never any indication of retrogressive changes.

The small size of the electric organ, together with the shallowness of the minute cups of which it consists, seems at first to indicate that in *Raia radiata* we have an electric organ in the act of disappearing. But when the organ of the species *radiata* is carefully compared with the organ of the species *batis* and *circularis*, the evidence seems to point in an opposite direction, and the view that the cups of *Raia radiata* are in process of being elaborated into more complex structures, such as already exist in *Raia circularis*, is apparently confirmed by the developmental record. Were the electrical organ of *Raia radiata* a mere vestige of a larger structure which formerly existed, we should expect to find the motor (electric) plate incomplete, or only occupying a portion of the electric cup; and the nerves proceeding to it, either few in number or undergoing degenerative changes. But instead of this we have a relatively large bunch of extremely well-developed nerves proceeding to the motor plate, which is not only complete, but extends some distance over the rim of the cup. Further, there is no indication of the walls of the cup having ever consisted of extremely complex lamellæ, such as we have in *Raia circularis*. They consist of a nearly solid mass of muscular tissue, scarcely to be distinguished rom the unaltered adjacent muscular fibres. The electric cup of *Raia radiata* may, in fact, when its structure alone is considered, be said to be a muscular fibre which has been enlarged at one end to support a greatly overgrown motor plate. But the development of the electric cups is even more suggestive than their structure. Had the muscular fibres in *Raia radiata* assumed the form of clubs before the young skate escaped from the egg capsule; had the clubs been rapidly transformed into electric cups; and had the cups soon after reaching completion begun to disappear, the evidence in favour of degeneration would have been complete. But, as has been indicated, the conversion of the muscular fibres into an electric organ is late in beginning, and the clubs having appeared, pass slowly through a long series of intermediate stages before they eventually assume the cup form. Further, as has already been mentioned, in the largest specimens of *Raia radiata* examined no evidence was found of retrogressive changes, either in the cup proper, or the numerous nerves passing to its electric plate. Hence it may be inferred that the electric organ of *Raia radiata*, notwithstanding its apparent uselessness and its extremely small size, is in a state of progressive development.

EDINBURGH.

Royal Society, June 18.—In the report of this meeting the title of a paper on the development and life-histories of the food and other fishes, communicated by Prof. W. C. McIntosh and Mr. E. E. Prince, was inadvertently omitted.

July 2.—Prof. Chrystal, Vice-President, in the chair.—Dr. Ramsay Traquair read a paper on fossil fishes from the Pumpherston oil-shale, and exhibited specimens.—Dr. W. Peddie read a paper on the effects of electromotive force and current-density on the total opposition (due to resistance of the conductors, reverse electromotive force, &c.) to the passage of an electric current through a liquid.—Mr. George Brook described a lucifer-like crustacean larva from the West Coast, and also communicated, in conjunction with Mr. W. E. Hoyle, a paper on the metamorphosis of the British *Euphausiidæ*.—Prof. Haycraft and Dr. E. W. Carlier read a paper on morphological changes which take place in blood during coagulation.—Prof. Tait submitted a paper on Laplace's theory of the internal pressure in liquids.

July 9.—A special meeting was held, Sir Douglas Maclagan, Vice-President, in the chair.—Dr. Berry Hart read a paper on the mechanism of the separation of the placenta and membranes during labour.—Dr. Woodhead communicated a paper, by Dr. J. W. Martin, on the pathology of cystic ovary; and also a paper, by Mr. T. A. Helme, on histological observations on the muscle, fibre, and connective tissue of the uterus during pregnancy and the puerperium.—Dr. T. G. Nasmyth read a paper on the air in coal-mines.

PARIS.

Academy of Sciences, July 16.—M. Janssen, President, in the chair.—Experiments with a new hydraulic machine, by M. Anatole de Caligny. This apparatus is of less simple structure than the valved machine with oscillating tube already described and exhibited by the inventor. But it has the advantage, under certain conditions, of giving relatively better results.—On the planet Mars, by M. Perrotin. These remarks are made in connection with the four sketches referred to in a previous communication, which are here reproduced, and which give the appearance of the planet on May 8, 1888, June 12, 1888, May 21–22, 1886, and June 4, 1888. The two first show the new canal A and that of the north polar ice-cap, the second also giving the smaller canal B seen for the first time on June 12. The fourth shows four simple and three double canals, all clearly defined. Two of the latter stretch from near the equator along the meridians 330° and 5° of Schiaparelli's chart to the vicinity of the north polar ice-cap. The difference is very striking, especially in the region of Libya, between the first and second of this year, and the corresponding No. 3 for the year 1886.—On the explanation of an experiment by Joule according to the kinetic theory of gases, by M. Ladislas Natanson. The experiment in question occurs in vol. i. p. 183 of Joule's "Scientific Papers." From the considerations here advanced, M. Natanson concludes that, so far from being opposed to the kinetic theory of gases, this experiment might be regarded as a practical confirmation of the law determining the distribution of molecular velocities discovered by Clerk Maxwell, and generalized by Boltzmann.—M. Natanson's paper was accompanied by a note from M. G. A. Hirn, who still maintains that not one of his nine fundamental objections to the kinetic theory itself has yet been answered, and consequently that this theory is already out of date.—On the thermic conductibility of mercury above 100° C., by M. Alphonse Berget. In continuation of a previous note (*Comptes rendus*, April 16, 1888), the author here gives the results of his studies on the variation in the thermic conductibility of mercury between 100° and 300° C. For 1° he finds the variation in the coefficient of thermic conductibility to be −0·00045.—Measurement of the velocities of etherification by means of electric conductibilities, by M. Negreano. The author has already shown that the velocity of etherification for a mixture with equal equivalents of alcohol and acetic acid may be measured by determining the electric resistance of the liquid by Lippmann's electrometric method. In the present communication he extends the same process to masses of alcoholic reagents or acetic acid differing in the number of their equivalents.—Observations respecting some recent communications from M. Sabatier on the chlorhydrate of cupric chloride, and the chlorhydrate of cobalt chloride, by M. Engel. While insisting on his admitted claim to priority, the author points out that there are two distinct chlorhydrates of the chloride of copper. He also shows that the pale blue powdery precipitate observed by M. Sabatier is not a chlorhydrate of chloride, but a hydrate of cobalt chloride—On the elementary composition of crystallized strophan thine, by M. Arnaud. This is an extract from *Strophanthus Kombé*, much used by the Fans of West Equatorial Africa for poisoning their spear- and arrow-heads. The formula is here shown to be $C_{31}H_{48}O_{12}$, its elementary composition thus showing it to be a close homologue of the wabaïn ($C_{30}H_{46}O_{12}$), the active principle of the wabaio plant used for similar purposes by the Somali people.—Influence of the temperature of fermentation on the production of the higher alcohols, by M. L. Lindet. The experiments here described seem to show that the yield of the higher alcohols is little affected by varying the temperatures of fermentation.—On *Fascicularia radicans*, C. Vig., a new type of Anthozoa, by M. Viguier. This little specimen of an Alcyonium was lately obtained during some dredgings in the port of Algiers. From the description here given it appears to be most closely related to the Paralcyonia, although sufficiently distinct to form an independent group or sub-family of the Fasciculariæ.—M. A. d'Arsonval describes and illustrates a new metal self-regulating stove, which is intended to maintain invariable temperatures by the exclusive use of gas and water. It is specially adapted for physiological and microbiological researches, and is constructed essentially on the same principle as that submitted to the Academy on March 5, 1877.

BERLIN.

Physical Society June 29.—Prof. von Helmholtz, President, in the chair.—Dr. R. von Helmholtz exhibited a new form of bolometer differing from that used by Langley. In Langley's instrument the alterations of electrical resistance produced by radiation are measured by introducing the exposed bolometer into one arm of a Wheatstone bridge a similar one protected from the light being introduced into the second arm of the bridge, while the other two arms contain a corresponding resistance. In the new bolometer as constructed by Siemens and Halske all four arms of the bridge are composed of equal wires rolled up into a coil and of these coils 1 and 3 are illuminated, while 2 and 4 are kept dark, and then coils 2 and 4 are illuminated, and 1 and 3 kept dark. By this means a fourfold sensitiveness of the bolometer is theoretically obtained. All four coils lie inside a brass tube, and by turning a screw at one time coils 1 and 3, at another coils 2 and 4 are brought opposite the opening. In comparing the speaker's experiments with those of Langley it appeared that the latter's measurements were five times more delicate than those of the speaker, a result which must however be entirely attributed to the fact that Langley's galvanometer was twenty times more sensitive. The speaker then expounded the theoretical efficiency and conditions of perfect sensitiveness of the bolometer, and compared with these the capabilities of a thermopile. Dr. Fritz Kötter discussed some new instances for the application of the Helmholtz-Kirchoff theory of stationary motion of fluids. Prof. Gad gave some explanations in connection with his demonstration of the phosphorescent moss. Prof. Neesen spoke on an ether calorimeter which he has succeeded in constructing in such a form, after many experiments, that it presents many advantages, when compared with an ice-calorimeter. It consists of a tube for the reception of the object whose heat is to be measured; this tube is surrounded with a layer of lamp-wick which dips into ether at its lower end. From the side of the outer vessel a tube passes with appropriate bending to a horizontal capillary tube containing as index some ether, and by a parallel capillary tube to a second and similar calorimeter. After the index has been adjusted, its movement, as resulting from the vaporisation of ether due to the warm object, indicates how much heat has been given up to the wick saturated with ether. The sensitiveness of this calorimeter is 2000 greater than that of an ice-calorimeter. The speaker has determined with this instrument the specific heat of platinum, palladium and copper, and also the heat produced by the passage of an electric spark between a metallic point and a mass of mercury in the tube of the calorimeter. The results were very satisfactory. The special advantage of this instrument consists in the fact that extremely small masses of any substance can be examined calorimetrically. The extreme sensitiveness of the apparatus makes it also suitable for the measurement of radiant heat. The speaker has additionally examined other fluids as to their suitability for a vapour-calorimeter, especially alcohol.

Physiological Society, July 6.—Prof. Munk, President, in the chair.—Prof. Zuntz described a simplified method of measuring the gaseous interchange during respiration, intended to make it possible to introduce such measurements into the limits of clinical observations to the same extent that urinary analysis is now carried out. In this method breathing is carried on, the nose being closed, through a mouth-piece which is connected by very mobile valves with gasometers, which thus measure the volume of the inspired as well as of the expired air. Samples of the expired air can be collected at any desired intervals of time and the amount of O and CO_2 which they contain determined by Hempel's method. The burette into which the gas is drawn off by means of an aspirating apparatus connected with the gas meter, is connected by a gutta-percha pipe with a vertical tube which is partly filled with water: the latter not only permits of the measurement, at atmospheric pressure, of the volume of air drawn off, but also provides a means of forcing it out of the burette into a pipette filled with a solution of caustic potash, in which the absorption of the CO_2 speedily takes place. By lowering the tube the gas is allowed to pass into the burette again, and the reduction in its volume gives the amount of CO_2 in the expired air. After this the gas is forced into a pipette which contains lumps of phosphorus which absorb all the oxygen it contains in five or six minutes. On passing the gas again back into the burette, the further diminution in its volume gives the amount of O in the expired air. A new sample can now be taken, and thus the expired air may be tested as often as may be desired for its contents of O and CO_2. Dr. Loewy has carried out some experiments with the above apparatus on five intelligent persons in order to determine the influence of digesting activity on the respiratory interchange. The respiratory interchange of the patients was determined in the morning while fasting and in a perfectly quiescent condition; as soon as this was found to be constant they received doses of 5, 10 or 15 grains of Glaubersalt, and as soon as the action of the salt had manifested itself painfully, and increased peristaltic action had set in, the respiratory interchange was again determined up to the time of defæcation. In all cases the gaseous interchange was increased, more oxygen being used up and more carbonic acid given out, the increase being between 7 and 30 per cent. of the normal. The several persons behaved very differently in this respect and the same person showed marked differences in the increase of respiratory interchange at different times, after equal doses of the salt. As a rule the increase was proportional to the amount of discomfort experienced by the patient in the lower parts of the body. Dr. Loewy is inclined to attribute the increased oxydational interchange to the greater activity of the unstriated muscles of the alimentary canal; the increased activity of its mucous membrane, resulting from the presence of the purgative, appeared to have no influence.—Prof. Munk gave an account of his experience last year while using catgut as a ligature. After having used catgut for some time as a substitute for silk, with excellent results, suddenly bad results began to follow its use, so that each ligature was accompanied by suppuration. A series of control experiments showed that the wounds healed well when silk was used, but never did so with catgut, and inasmuch as the above change was first observed after obtaining the catgut from a new source he proceeded to obtain the article again from the original source, and at once found it worked successfully again. No matter how long the second sample of catgut was disinfected its use was always attended with suppuration. Prof. Munk has hence reverted to the use of silk ligatures, and urges great caution in the use of catgut in surgery.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Parish Patches: A. N. Simpson (Buncle, Arbroath).—The Senses and the Will: W. Preyer, translated by H. W. Brown (Whittaker).—A New Theory of Necessary Truths: Leonard Hall (Williams and Norgate).—Camping Out, or Holidays under Canvas: Gyp; second edition (Simpkin).—A Bibliography of Chemistry for the year 1887: H. C. Bolton (Washington).—Bericht über die Thätigkeit der Botanischen Section der Schlesischen Gesellschaft im Jahre 1887: Dr. F. Cohn.—On the Structure, Development, and Affinities of *Trapella*, Oliv., a New Genus of Pedalinæe: F. W. Oliver.—Bulletins of the Philosophical Society of Washington, vols. ix. x. (Washington).—Proceedings of the American Philosophical Society, vol. xxv. No. 127 (Philadelphia).—Botanische Jahrbücher für Systematik, Pflanzengeschichte, und Pflanzengeographie, Zehnter Band, 1 and 2 Heft (Leipzig).

CONTENTS.

THURSDAY, AUGUST 2, 1888.

LORD ARMSTRONG ON TECHNICAL EDUCATION.

LORD ARMSTRONG, in his article in the July number of the *Nineteenth Century*, brings forward ideas which, he tells us, have long been incubating in his mind, and which he believes to be in accord with those of many employers of labour who, like himself, are engaged in manufacturing pursuits affording scope for the application of technical knowledge. A more unfortunate exposition could not have been addressed to the public at a time when so many are earnestly striving to impress upon the nation the importance of scientific training to the well-being of the people. It is not that we do not cordially agree with Lord Armstrong in many of his remarks; what we object to is the indefinite and vague character of his judgments generally, and the want of logic which characterizes many of his criticisms and recommendations: in every paragraph almost we recognize that we are reading the words of a true representative of that remarkable genus, the "practical" Englishman, who undoubtedly has been the glory of his race in the past, but threatens to be its destruction in the near future. But so outspoken a refusal to recognize the altered conditions of the times, by one who occupies the highest position among engineers, unfortunately affords clear evidence that we are making but little progress towards "organizing victory" in that great industrial war of which Huxley spoke in his memorable and incisive letter to the *Times* early in 1887, in words of deepest import, which unquestionably should serve to guide us *pace* Lord Armstrong's avowal: "As to whether our commerce is to be saved from the effects of foreign competition by a wide diffusion of technical knowledge, I have no faith in any such safeguard." In contrast with this is Huxley's emphatic warning:—"I do not think I am far wrong in assuming that we are entering, indeed have already entered, upon the most serious struggle for existence to which this country has ever been committed; and the latter years of the century promise to see us embarked in an industrial war of far more serious import than the military wars of its opening years. On the east, the most systematically instructed and best informed people in Europe are our competitors; on the west, an energetic offshoot of our own stock, grown bigger than its parent, enters upon the struggle possessed of natural resources to which we can make no pretension, and with every prospect of soon possessing that cheap labour by which they may be effectually utilized." Surely we shall elect to follow Huxley's advice offered to us in the sentence, "Many circumstances tend to justify the hope that we may hold our own if we are careful to organize victory," and we shall not be content to rely on a sufficient number of self-educated men of genius being spontaneously forthcoming to supply the nation's needs: indeed there can be no doubt that in the course of a generation or two—if we can maintain our existence unimpaired so long—every effort will be made to develop the faculties of each member of the community as fully as circumstances will permit; but unless some grievous reverse of fortune should lead the nation suddenly to realize its position, we sadly fear that the cause of educational progress has too many lukewarm adherents, holding views similar to those expressed by Lord Armstrong, for it to make much immediate progress.

Lord Armstrong says very truly that, although there is at the present time a great outcry for technical education, very few people have any distinct idea of what they mean when they use that term, or any definite opinion either as to the class of persons who will be chiefly benefited by it, or as to the time of life at which it ought to be acquired. Speaking of the meeting recently held at the Mansion House respecting the scheme for establishing Polytechnic Institutes in London, he remarks also that the speeches then delivered were rather vague and indefinite as speeches on technical education generally are; and he points out that, by using the more comprehensive phrase secondary instead of technical education, Lord Salisbury avoided the troublesome but not unnecessary task of framing a correct definition. But it may with equal truth be said of Lord Armstrong that he, like most writers on technical education, is indefinite and vague; and he also makes no attempt to give a definition of technical education. In fact, his article is nothing more than a discursive essay on the subject of popular education generally, excluding moral and religious questions.

The vagueness which characterizes the utterances of most speakers and writers on technical education is undoubtedly the outcome of the peculiarly English practice which permits men to speak with authority who have no claim whatever to be heard on the subject, and which leads us to put aside those who really are experts as of no account. The work has fallen almost entirely into the hands of philanthropists and politicians, and inquiries into the subject have been handed over to men whose qualifications for the work in too many cases would have been regarded in any other country but England as lamentably insufficient. At the recent meetings at the Society of Arts and the Mansion House there was a conspicuous absence of nearly all those who are known to have been most active in carrying on the real work of technical education and who are able to speak from experience. Yet, if the public are to be properly informed and guided, and if the politicians are to be instructed in their duties, it is imperative that others besides the ornamental and amateur members of the body of technical educators should be summoned to assist in the movement.

The *Times*, in a recent article on Lord Hartington's speech at the meeting of the Association for the Promotion of Technical Education, has very properly called attention to the importance of an accurate definition of the term technical education, pointing out that if it means that kind of education which bests fits a man both mentally and bodily for technical pursuits requiring skill and intelligence the proposition that technical education is a good thing is self-evident; but that if it means a particular method of imparting knowledge on technical subjects then it is open to many of the criticisms passed on it by Lord Armstrong. Probably the majority of the public are at present of opinion that to technically educate a youth is to teach him his business—that technical education is the modern equivalent of the now effete apprenticeship system. This came out very clearly in the late discussion with reference to the introduction of manual training into schools, to which objection was made by many artisans, who urged, among other things, that if such instruction were given it should be imparted by skilled artisans and not by the teachers—entirely failing to realize that it was sought to introduce manual training with an educational object, for the purpose of cultivating a faculty hitherto left untrained, and not for the purpose of teaching a trade. Authorities, however, we believe, are mostly of opinion that to technically educate a youth is to teach him to understand and scientifically follow his business, and they consider that only so much of the actual practice should be learnt by the student who is being technically educated as will suffice to afford the necessary insight into the principles on which the practice is founded. Thus, medical men have long been technically educated: they have not only learnt the practice of their profession, but have also devoted a large amount of time to the study of the facts and scientific principles on which medical practice is based, and the demands upon them in this latter direction have been much increased within recent years. Engineers and architects, on the other hand, hitherto have generally not been technically educated: entering the workshop or office, they have been left to acquire as they best might a knowledge of the scientific principles underlying their professions, their attention having been almost entirely devoted to acquiring manipulative skill and a knowledge of constructive details.

It is difficult to understand what meaning Lord Armstrong attaches to the term technical education. He tells us that the question "What is the use of useful knowledge?" appears to him to present in a quaint form a theme of a very debatable nature! He then proceeds to argue that success in the world depends on the possession of genius; knowledge—well, is of no particular consequence! "Many people imagine that genius is kept down from want of knowledge, and that in many cases it is thus lost to the world. This I entirely dispute. Genius is irrepressible, and revels in overcoming difficulties." But even the genius must find his opportunity, and—nowadays at least—must be possessed of sufficient knowledge to be able to take advantage of the opportunity when found. Moreover, as the world progresses, opportunities are not found to be increasingly numerous in proportion to the growth of the population, nor do the problems diminish in difficulty; and no reliance can be placed upon the supply of genius keeping pace with the demand.

Lord Armstrong thinks that the well-known dictum that if the Romans had had to learn Latin they never would have conquered the world, is suggestive of what our loss might have been if self-made engineers such as Watt, George Stephenson, Smeaton, Brindley and Telford, had frittered away their energies upon inappropriate studies forced upon them at school; and that generals such as Wellington and Marlborough, or naval commanders such as Nelson and Blake, would not have directed the armies and navies of England with more effect if book knowledge had been crammed into them at school. But to argue in this manner is to entirely pervert the theme of technical education: the whole object of its advocates being so to improve the entire educational machine that all inappropriate studies may be eliminated from the school course, and every provision made for developing and strengthening the faculties generally; and even Lord Armstrong admits that as "cheapness of production and superiority of quality will decide the victory in the race of competition, we shall improve our chance of maintaining a foremost place if by early training we develop the mental and bodily faculties of our people." His subsequent words, however, "but not, I think, by any forced or indiscriminate system of imparting knowledge," are simply incomprehensible, as no one has suggested the introduction of any "forced and indiscriminate system"; in fact, this is only one of the many cases in which Lord Armstrong sets up an image of his own creation, and at once hastily destroys it. When he tells us that he does not "undervalue technical knowledge voluntarily acquired as a means to an end, but it is the brain-workers and not the hand-workers who will seek to attain it and benefit by it," he entirely overlooks the fact that one great object of technical education is to associate brains with hands and hands with brains.

We have no space left to discuss Lord Armstrong's extraordinary views with reference to existing facilities for the acquisition of technical knowledge and their sufficiency. But we must call attention to his contention "that when Colleges can be established by public subscription or private munificence, they are worthy of approval and commendation; but where the State or local governing bodies have to furnish money for education in relation to national industry, they must look to attaining the required results at the least possible expense, and I am inclined to look upon Colleges as luxuries in education rather than necessaries." In marked contrast to this is a statement made by Sir Henry Roscoe in the discussion on Mr. Swire Smith's paper on the Technical Education Bill, read at the Society of Arts in February last. Speaking of what the Swiss were doing, Sir Henry related how, a few years ago, when it was proposed to spend £24,000 on the erection of a new chemical department of the Zurich Polytechnicum, some of the Bundesrath were a little startled and rather objected to paying so large a sum, and there was accordingly in Berne some opposition; but the Minister of Education pointed out that the amount of money which had already been received by Switzerland from the men who had studied in the Polytechnic School at Zurich had amounted to ten times over the sum he was asking for, and he was sure that the money would be well spent, and in a short time recouped. The Swiss, at all events—let alone the Germans—therefore do not look upon Colleges as luxuries rather than necessaries; and we are assured that if comparison were made of the work done by chemists in Swiss laboratories with that done by English chemists, the result would not be to the credit of our country. We should like Lord Armstrong to tell us—is he, or is he not, content to see this country remain on a lower intellectual footing than Switzerland?

Great as is Lord Armstrong's reputation as a mechanical engineer, we trust that few will regard him as an "unimpeachable authority" in the matter of technical education: if the majority remain much longer of his opinion, then is the fate of our nation sealed.

EXPLORATIONS AND ADVENTURES IN NEW GUINEA.

Explorations and Adventures in New Guinea. By Captain John Strachan, F.R.G.S., F.R.C.I. (London: Sampson Low, Marston, Searle, and Rivington, 1888.)

THE *terræ incognitæ* of the world are year by year growing less; but of these the vast island continent of New Guinea remains as to much of its coasts, and almost all of its mountain regions, still scantily known and explored. The elegantly bound volume under review is the latest contribution to our knowledge of the shores of this tripartite country. To the explorer who adventures himself into this most insalubrious territory, even if he bring back with him but small additions, we are under a debt of gratitude, if so be, however, that his record be trustworthy, and an honest attempt to add to science geographical or biological.

Captain Strachan is a master mariner, who appears to have spent several years on the New Guinea coasts, in command of small trading vessels, engaged in the collection of such commercial products as are to be obtained from the natives, and has made a bid for fame by combining with his ordinary pursuits the *rôle* of explorer. The narrative before us it would be unfair to submit to too rigid a criticism as a literary production, especially as the author disclaims the intention of aspiring to "literary renown," but relates his experiences in the "homely language of a British sailor." Deprived of the expectation of a literary delicacy, the reader has a right to hope for a more or less satisfying portion of new facts and observations, as the *raison d'être* of the work.

The book divides itself into two portions: explorations in the Papuan Gulf within the British Protectorate; and in Macluer Inlet (or Gulf, as Mr. Strachan not inappropriately calls it) in the Dutch territory in the north-west.

In the Papuan Gulf, Mr. Strachan claims to have ascended the Mia Kasa river, and to have discovered and explored Strachan Island and Strachan Country, a region lying to the immediate west of the Fly River. He has discovered also a large arm of the Mia Kasa, christened by him the Prince Leopold River, which incloses, and is the western boundary of, Strachan Island. The name, Prince Leopold River, he has applied also to the Mia Kasa above its junction. Beyond the mere statement, "the Mia Kasa itself was discovered by Dr. Samuel Macfarlane as far back as 1877, and was named by him the Baxter," Mr. Strachan makes no reference to the previous exploration of the river made, not in 1877 but in 1875, by that missionary, who ascended it for sixty miles in the *Ellengowan* steamer, and for thirty miles farther in one of his ship's boats. This is as far as, if not farther than, the point attained by Mr. Strachan. If therefore a new name had to be applied, only the western arm, now first brought to our knowledge, ought to bear the name Prince Leopold, while the river explored by Mr. Macfarlane should be known as the Mia Kasa or Baxter. Even the Prince Leopold River is indicated in Macfarlane's map. Mr. Strachan has indicated a number of diverticula extending right and left from both rivers, but he adds little, beyond stating it, to the opinion, long held, though yet without absolute proof, that the Mia Kasa and all its affluents are merely canals of the vast delta system of the Fly River. If Mr. Strachan had taken the trouble to examine the work of his predecessors, he could scarcely have deluded himself on entering the mouth of its estuary with such fancies as these: "During the whole day I could not help thinking that we were not sailing on a river at all; but were on an arm of the sea, which would, in all probability, extend across the whole island from south-east to north-west, opening into the Arafura Sea at that part known to the Dutch as the Utanata River; and I built a good many castles in the air in consequence, hoping we had found a new channel to China and the East"! It is sufficient to state that the Utanata River rises in the gorges of the Charles Lewis Range, so that the water-way surmised by Mr. Strachan to exist must cross the spurs of that range. Nor has he any better basis for many of his beliefs, none of which appears more unfounded than that given on p. 278, where a river "debouching into the Arafura Sea opposite Providential Bank, will, I believe, be found connected with the Fly River at its junction with the Alice River, discovered by D'Albertis"! This new river would necessarily bisect his new channel to China! We have unfortunately no means of testing the accuracy of the author's positions. He does not tell us on what base his survey is constructed; or whether it is established by astronomical observations, or from assumed points on the Admiralty chart fixed by sextant angles or prismatic bearings, so as to gain his reader's confidence in his discoveries. On p. 41 he refers to a large tributary as being "ninety miles inland which I named the Wallace"; while on p. 128, he says, "at a distance of some *eighty* miles the Prince Leopold again divides into two branches, the eastern of which is the Wallace," "which we followed [p. 42] for a distance of seventy miles through the same class of country." If we test this distance by his map, we find that a chain thirty-five miles in length, would extend from the mouth of the Wallace River to *beyond* the Fly River. These discrepancies do not increase our confidence in the accuracy of Mr. Strachan's explorations. He describes the country in this region in the most glowing terms, "splendid agricultural country," "well watered," "high land." Other travellers have reported it as "low and swampy," while D'Albertis in ascending the Fly, found the whole country for some hundreds of miles low and little elevated above the sea. Such glowing advertisements are to be gravely deprecated, of a region so malarious that few Europeans can ever be able to settle in it as their home; it is doubtful whether they could even find it habitable during the wet season. While abundance of unoccupied territory exists in Australia, richer in soil and easier of access, and in a far less unhealthy climate, no wise man will risk his capital and his life in the great delta of the Fly River. The natives at the mouth of the Mia Kasa seem to have so threatened the little party, that they had to abandon their lugger, and make for the coast overland, experiencing some hardships by the way, and eventually the loss of one of their companions by drowning. We fear few will be able to appreciate Mr. Strachan's delicacy in forbidding, "in order to prevent raising a hostile spirit among the natives," his "weary, worn and starving people," from cutting down a cocoa nut tree, during their retreat, shortly after they had been firing on its owners with their Winchesters, discharging rockets in their midst, and exploding among them a tin case containing twenty-five pounds of gunpowder. One would think that if their hostility had not been excited by these gentle tactics, they could have borne also with equanimity the appropriating of a few cocoanuts.

The second part into which this record of exploration divides itself, is really little more than the log of a trading cruise. Except the claim to the differentiation of a few insignificant islands, no piece of exploration worthy of the name abides in the recollection after laying down the volume. Macluer Inlet has long been a *rendezvous* for trading vessels, and Mr. Strachan's time seems to have been chiefly devoted to collecting nutmegs, massoi bark, tortoise- and pearl-shell from the natives. He reached the top of the Gulf, and he lays evidently great store by another geographical surmise related in the following words :—

"In three days we arrived at the head of the Gulf and anchored opposite what afterwards proved to be an island. Here two channels, one to the north and the other to the south, debouch into the inlet. The latter we entered and followed until we reached a bend, at a distance of not more than three miles from Gleevink [Geelvink] Bay, where we anchored.

"Here the channel is between two and three miles in width, and the depth of water seven fathoms. My charts showed the opposite shore to be entirely unsurveyed and faced by many islands; the inhabitants of which I had reason to believe were hostile.

"These considerations decided me to return, although well convinced that by continuing another two or three miles I should enter the broad waters of Gleevink [Geelvink] Bay."

We should have felt more confidence in this conviction, if the author had given the data on which he grounds his surmise, if only to allay our suspicions that this is not a happier guess than that which flashed on him at the mouth of the Mia Kasa River. He makes no reference to the explorations in 1873, in the same region, of Dr. Meyer, who, entering the Wapari River on the eastern side, in Geelvink Bay, and ascending mountains over 1200 feet in height, descended the western slope till he struck the Jakati river by which he reached the shores of Macluer Inlet—a route which must have led him across the wide channel supposed by Mr. Strachan to exist, but of which no mention is made by Dr. Meyer. Is Mr. Strachan *quite sure* about his position—especially the longitude of his turning point?

In the selection of his crews Mr. Strachan was most unfortunate. They appear to have been very typical beachcombers, against whom he brings charges of threatening the natives, and of wantonly shooting their dogs—deeds which are very characteristic of that baneful type of humanity.

In his natural history determinations Mr. Strachan is very often considerably afield; but he makes several interesting observations on the customs of the people. One or two illustrations of the natives are given, which appear to be faithful representations of the tribes of the delta.

The book, we regret to say, does not leave a very satisfactory impression on the reader; there are numerous inaccuracies and too many discrepancies between the text and the maps; while the goody-goodyism and buccaneering brag with which it is interlarded are insufferably nauseous, with the result that the reader loses what confidence he might otherwise have had in statements of the author that may be quite accurate.

In noticing this volume we cannot omit to draw attention to a subject much more serious than its poverty of fare. Mr. Strachan tells us he was denounced in New South Wales as a "red-handed murderer, who had tramped through New Guinea knee-deep in blood." The accusations against him were the outcome of the "outrageous lying" of one of his own party, which he rebutted by a letter to the Secretary of State for the Colonies, who caused his (Mr. Strachan's) letter to be published in Sydney for general information. We may probably accept the statements made against himself in this volume, under his own hand, as at least not "outrageous lying." Mr. Strachan knew fully the conditions under which he and his party had permission to cruise in the waters of the Protectorate or of the Dutch Crown. No spirits, firearms, gunpowder, dynamite, or any explosives can be landed under any circumstances, so as to be given or sold to the natives; no acquisition of land on any account is permitted; and above all a just treatment of the natives is a *sine quâ non,* since it was the overacts of her subjects that compelled Her Majesty to take under her gracious Protection the inhabitants of that portion of Papua, now generally known as British New Guinea, and for which the name of Torresia has been suggested. On p. 80 is recorded this little episode: "The men who were so fortunate as to possess muskets were very eager to obtain ammunition; but this the law distinctly forbids the white man either to give or to sell to the natives under a penalty of three months imprisonment. . . . . Being anxious to accommodate those whose kindness to me had been so uniform, I was placed on the horns of a dilemma, but having confidence in their integrity, and being anxious to serve them while keeping within the strict letter of the law [! ! !] . . I at last decided to place the required ammunition on my cabin table. Having done this I lit my pipe, and went on deck to give some orders to my officers. On my return the natives had all left my cabin. . . . I missed a twenty-eight pound bag of No. 4 shot, half-a-dozen half-pound flasks of powder, and a box of caps." This is not the only occasion, recorded in his book, on which he distributed warlike material. In several places he confesses to having dispensed gin to the natives, and presented it as gifts to chiefs. The edict as to the purchase of land was also disregarded in the same open way. He purchased Strachan Island, containing [only] seven hundred and fifty square miles, by a very simple transaction. "'Are you willing that I come and possess this island?' . . . . They all signified their willingness. My trade was opened and parcelled out to each chief according to the number of people in his tribe. I told them the name was Strachan Island, and by this name the natives know the island at present." The latter amazing statement we may take for what it is worth; but it would have been very instructive to have had details of the items of the trade paid for this little estate. The document would probably have formed a companion to the valuable inventory given in the late Sir Peter Scratchley's journals of the price paid by certain Australian pioneers for a tract of land the size of a large English county. There is no evidence that these "chiefs" owned the land they were selling, nor that they were made aware that they were parting for ever with their most cherished possessions, of which Mr. Strachan attempted to claim ownership (Her Majesty's edict notwithstanding) in right of exploration and purchase. While in Macluer Inlet, the author resided among a people who spoke to some extent the Malay language. The quotations with which we are favoured in his book, not to mention his own admission of the fact, show clearly how imperfect his knowledge of that language (as of the true language of the region) is. Yet from a conversation he overhears, half of which only, he admits, he understood, he accuses certain chiefs of Macluer Inlet of slave-hunting, and in the most high-handed and unauthorized manner, carries them off prisoners to Gessir, to give them in charge to the Dutch authorities, yet does not do so (owing to stress of weather), which under the grave circumstances he ought to have done when the weather moderated. Eventually, after severe cogitations whether he should not himself inflict punishment on them, he returns them to their homes, when he feels "much lighter of heart." Shortly after this, he sees a canoe "dodging backwards and forwards among the islands within gun shot of the ship," and is seized with a panic (as he often was), and without the flimsiest evidence of a hostile intention on the part of its occupants, he seized a "long range rifle" and fired into it; "they then began paddling rapidly, and although I fired many shots I could not round them to." Nor are these again the solitary instances of most illegal acts performed by Mr. Strachan as recorded by himself. It is doubtful, also, whether the removal of the little lad whom he brought from his country to England (and whom he appears to have treated with the greatest possible kindness) was not an act of kidnapping. Altogether, it is perhaps not surprising that the natives, as Mr. Strachan bemoans, "cannot recognize nor appreciate the principles of honesty and honour," so exemplified.

Her Majesty's Special Commissioner comes in for a most violent and unwarrantable attack. No one who reads Mr. Strachan's own admissions will wonder that his explorations were not regarded by the authorities with all the favour he could desire. If Mr. Douglas had had the facts here recorded before him, he must, we fear, instead of renewing the author's permit, have excluded him from again approaching the island. The Commissioners administering the Government in New Guinea have had experience enough of the woes that flow not to the natives themselves only, but to unsuspecting Europeans who have the misfortune to follow behind (and have paid, too often, the penalty of the overacts of) such explorers as "Captain" John Strachan.

MINE-SURVEYING.

A Treatise on Mine-Surveying. By Bennett H. Brough, F.G.S., F.I.C., 8vo., pp. 282 with 101 woodcuts, two appendices and index. (London: Charles Griffin and Co., 1888.)

MR. BROUGH, who for many years has been giving instruction in surveying at the Royal School of Mines, has placed the mining world under a debt of gratitude to him by the issue of his compact manual. It is the kind of book which has long been wanted, and often asked for, not only by mining students, but also by mine-agents desirous of obtaining more knowledge concerning a material branch of their profession.

The book is divided into nineteen chapters. In the first the author dwells upon the importance of mine-surveying and certainly does not exaggerate it. Instances could be multiplied showing the danger to life and the loss of valuable mineral from the want of accurate plans. A blot in British legislation does not escape the author's notice, and he very properly regrets that the agents of ordinary ore-mines are not required to qualify themselves by examination in the same way as their brethren at collieries. Considering that the tin miners of Cornwall have a rather higher death-rate from accidents than colliers, and a very much higher death-rate from diseases induced by their occupation, it does seem strange that the test of ability imposed in one case should be entirely dispensed with in the other. When the Metalliferous Mines Regulation Act is amended we may hope to see this anomaly swept away Many agents of ore-mines would welcome the introduction of certificates of competency, because a Government diploma would raise their status at home and constitute a valuable passport for them abroad.

Four chapters are devoted to surveying with the ordinary miner's dial, of which various forms are described; and very useful hints are given concerning sources of error with the magnetic needle, which would not strike tyros, and some of which are probably unknown to many practised surveyors. The important question of the diurnal and secular variation of the magnetic needle is next fully dealt with, and we hope that due heed will be paid to Mr. Brough's remarks, for few ordinary diallers are aware that the needle may vary 10′ from 8 a.m. to 1 p.m.

The theodolite is properly recommended for cases where great accuracy is required, and much useful information is afforded upon various matters, such as plotting, calculation of areas, levelling, connection of underground and surface surveys and methods of rapid surveying with the tacheometer. Faults and subsidences are discussed at length, and careful directions are given concerning the construction and copying of mine plans. Mr. Brough insists upon neat lettering, but curiously enough omits all mention of stencil plates for this purpose.

The last chapter, dealing with the application of the magnetic needle in mining, is full of interesting matter. We have good descriptions of the Swedish and American dip-compasses, and the improved methods of Brooks, Thalén and Tiberg, for exploring for iron ore; and the author exposes the clever devices of unscrupulous mine-sharks for misleading intending purchasers. Between the years 1868 and 1875 eighty-five iron mines were discovered in the State of New Jersey solely by the magnetic needle, and in many cases where there was no visible indication of ore at the surface.

Mr. Macgeorge's ingenious appliances for ascertaining the true direction taken by bore-holes, which frequently deviate very considerably from the vertical, attracted much attention at the Inventions Exhibition, where they received a gold medal. Now that Mr. Macgeorge's method is described in a text-book, its advantages will become more generally known.

Mr. Brough deserves much praise for the care with which he has searched European and American publications so as to bring his work up to date, and there is little call for censure save upon minor points which do not affect the general value of the text-book.

It is time that some one should enter a protest against two of the technical terms defined by the author, and frequently met with in the reports of mining experts, viz. "country rock" and "gangue." To say "country rock" is tautology. The word "country" alone, as used in Cornwall, means "surrounding rock" or "enclosing rock," and, if the provincialism is to be adopted, there is no necessity to add the word "rock." The word "gangue" is objectionable, because it has come to us through Frenchmen, who apparently did not thoroughly understand the meaning of the German word "Gang." "Matrix," "lodestuff," and "veinstuff" are better words than "gangue," which might well be allowed to drop out of mining books, especially as it is rarely heard at mines.

To cite the china clay deposits of Cornwall as examples of *stockworks* is unfortunate, because the occurrence in them of veins bearing workable quantities of tin ore is the exception, not the rule.

In Chapter VIII. Mr. Brough says: "In 1798 Breithaupt, of Cassel, invented a mine-surveying instrument, which he called an *astrolabium*." This remark is not correct, for, as the author well knows, the astrolabe was invented by the ancients. The statement should have been that H. C. W. Breithaupt was one of the first to put an astrolabe upon a stand and use it for surveying underground. According to Mr. Brough the theodolite has been employed more or less for mine surveys since 1836. This date is probably correct as far as Germany is concerned; but as a matter of fact a mining theodolite was supplied to the Imperial Brazilian Mining Association four years earlier.

The description of Prof. Borcher's method of using magnets for ascertaining the precise line in which one should continue to work in order to connect two drivages in opposite directions which are approaching each other, is not so clear as it ought to be. Mr. Brough omits to explain, in reference to Fig. 101, that by construction the points A, B, and C are situated upon the circumference of a circle, the centre of which is E; and the confusion is increased by the statement that the triangle A E C is "equilateral," whereas it is really only isosceles. The consequence is that the reader is very much puzzled.

However, these and a few other errors can easily be corrected in a second edition, which is likely to be required before many years are past; because, as soon as the book becomes known, no English-speaking mine-agent or mining student will consider his technical library complete without it. C. Le Neve Foster.

OUR BOOK SHELF.

Charles A. Gillig's Tours and Excursions in Great Britain. By Stephen F. Smart. (London: United States Exchange, 1888.)

This book is intended in the first instance for Americans, but it may also be of some service to English tourists. Taking London as a central point—"not only because it is the most notable city of the world, but because it is the Mecca, if not the El Medina, of trans-Atlantic tourists, at least"—the author describes a series of excursions, any one of which will well repay the trouble of those who may elect to follow his guidance. He also describes various tours in Wales and Scotland. Mr. Smart has been at pains to make himself familiar with the ground over which he undertakes to lead others, and the information he presents, so far as we have been able to test it, is thoroughly trustworthy. Of course, no one who wishes to obtain a full account of any particular town or district will think of consulting this little book. But as a general sketch, it has considerable merits; and it will doubtless help many American visitors to make the most of a brief visit to Great Britain.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

The Supply of Bait for Sea-Fishermen.

One of the first questions of practical importance with which the Marine Biological Association has to deal is that of supplying the long-line fishermen with a continuous supply of bait at a cheap rate. Great distress is often occasioned through fishermen being unable to get the necessary bait for their long lines. Mr. Robert Bayly, of Plymouth, a governor of the Marine Biological Association, has generously given a sum of £500 to be spent on investigations on the bait question, and the Council have instructed me, as Director of the Association, to consider the best means of spending this sum. I shall therefore be glad to receive any suggestions from gentlemen who may interest themselves in this question, or to consider the work of any investigator already in the field, with the view of employing a suitable person to carry out a series of observations and experiments.

Two methods appear to offer a solution to the question. Either the animals used commonly as bait, such as whelks, mussels, and squid, may be reared artificially and kept in confinement till required, or some artificial bait may be invented which will lure the more valuable kinds of fish to the hook.

The former of these methods has been successfully practised in France, but such is the operation of the English laws on shore fisheries that there is very little prospect of its being possible in England, unless those laws are altered.

The second method, though more apparently difficult, is the more likely to attain success. Fish are undoubtedly guided by smell and taste in the selection of their food. Some are known to be very nice about the kind of food offered to them, and will only take certain kinds of bait. The whelk is a very favourite morsel, and has a distinct smell and taste: it may be possible to determine by analysis the essential oil or whatever it may be that gives this odour, and to imitate it sufficiently well to deceive the fish. The trade is able to imitate successfully the bouquet of wines: cannot chemistry produce an imitation of the bouquet of the whelk? G. C. Bourne.

The Laboratory, Citadel Hill, Plymouth, July 31.

Geometric Meaning of Differential Equations.

In the Proceedings of the Royal Asiatic Society of Bengal, 1888, p. 76, Prof. Asutosh Mukhopadhyay has proposed a really excellent mode of geometric interpretation of differential equations in general: viz. writing the equation in form $F = 0$, the geometric meaning of the symbol F considered as a magnitude (angle, line, area, &c.), *in any curve whatever* (wherein F is of course not zero), is, if possible, to be formed; then the geometric meaning of that equation obviously is that the quantity F vanishes right round every curve of the family represented. This is the most direct geometrical interpretation yet proposed.

Three examples have been given by him, all very neat. Writing for shortness the differential equations thus—

Circle, R = 0; *Parabola*, S = 0; *Conic*, T = 0,

he has proved (in Journ. As. Soc. Bengal, vol. lvi. p. 144, and NATURE, vol. xxxviii. p. 173) that in general *in any curve* whatever,

(1) Tan. ∠ of aberrancy = q_1. R;
(2) Index of aberrancy = q_2. S;
(3) Radius of curvature of aberrancy curve = q_3. T;

where q_1, q_2, q_3 are certain functions in general finite. Hence the geometric meaning of the differential equations of the three curves is at once

(1) *Circle.*—Angle of aberrancy = 0
(2) *Parabola.*—Index of aberrancy = 0
(3) *Conic.*—Radius of curvature of aberrancy curve = 0
} right round all curves of each family.

The verbal neatness of these interpretations can hardly be excelled.

A writer (R. B. H.) in NATURE, vol. xxxviii. p. 197, objects to the last that it really only means that *a conic is a conic* (because its aberrancy curve shrinks into the centre)! Now, this is precisely what was to be expected: the differential equation of a curve expresses *exactly* that the curve of some family which osculates it in the highest degree is *the curve itself*. But the new interpretation puts this in a neat form, viz. in assigning a *meaning* to the magnitude F, which differs from zero in general, and whose vanishing at all points of every curve of a certain family (say conic) indicates a property of high generality of those curves.

But the Professor makes, what I conceive to be, the mistaken claim (Proc. As. Soc. Bengal, 1888, p. 75, *et seq.*), that this mode of interpretation is the *only true one;* and further that, accepting this mode of interpretation, *only one meaning* can be attached to it (p. 76, l. 29, *op. cit.*).

Now it must be observed that the equation F = 0 implies *directly*, not only that some one geometric magnitude F vanishes, but also that every geometric magnitude vanishing with F (such as aF, aF^m. *sin*F, &c.) vanishes right round every curve of the family. All of these are equally good geometric interpretations *of the same kind* as proposed.

But the equation F = 0 also implies, more or less directly, countless theorems of position, osculation, &c. All of these may be fairly considered geometric meanings of that equation. Thus, attending to the meaning of "aberrancy," the results quoted involve directly—

(1) *Circle.*—Normal coincides with diameters.

(2) *Parabola.*—Diameters are axes of aberrancy, and meet at infinity.

(3) *Conic.*—Diameters are axes of aberrancy, and are concurrent (in the centre).

Surely these are also *true* geometric interpretations.

Lastly, let the equation F = 0 be multiplied by any of its integrating factors μ, and write for shortness $\int \mu F dx = \phi$. It follows that ϕ = constant. Hence, since the number of integrating factors is infinite, another (indirect) geometric interpretation arises, viz. that all the geometric magnitudes ϕ are constant right round every curve of the family.

These latter *general* modes of interpretation, viz. theorems of position, osculation, and of first integrals ($\phi = c$), I had given eleven years ago (in *Quart. Journ. Math.*, vol. xiv. p. 226).

To the last of these the Professor has objected (p. 76 of his paper quoted), that it is not an interpretation of the equation F = 0 at all, but only of its first integrals $\phi = c$. This is, of course, admitted. But it is worth noting that the connection between the two, F = 0, $\phi = c$, is so very close, that many will accept an interpretation of the latter as a fair (indirect) interpretation of the former also.

In fact, since F = 0 is equivalent to $D_x\phi = 0$, the former is now seen to mean *directly* that there is no variation of any of the magnitudes ϕ right round every curve of the family; and this is a strict *direct* interpretation of the equation F = 0 itself. But many will probably prefer the shorter phrase ϕ = constant, even though it interprets F = 0 only indirectly.

There is, moreover, a *slight* disadvantage in the former mode of interpretation, viz. that the meaning of the magnitude F must necessarily be sought in curves other than, and *usually more complex* than, the curves denoted by F = 0; whereas the interpretation of $\phi = c$ only requires the finding a meaning for ϕ, which is explained in my paper quoted to be any fundamental geometric magnitude *of the curve itself*.

ALLAN CUNNINGHAM, *Lt.-Col.*, *R.E.*

British Earthworms.

THE occurrence of any new animal in England is a point of some interest, however humble that animal may be; and, in order to work out the species of British earthworms, I sent a letter to the *Field* some time back, requesting readers of that journal to forward me specimens. In reply I received a large number of worms from various people, amongst them being Mr. F. O. Pickard Cambridge, of Hyde, who has very kindly sent me several parcels of worms. One of these parcels contained some very fine gravel taken from the bed of a stream, together with a number of small worms about 1½ to 2 inches in length. These turned out to be a species of *Allurus*, a genus formed by Eisen for a worm in which the male pores are on the thirteenth segment instead of on the fifteenth, as in the other genera of the family Lumbricidæ. Only one species is at present known, viz. *A. tetraëdrus;* it is of a beautiful sienna colour, with a dull orange clitellum.

I wish to record, for the first time, its occurrence in England, and also to draw attention to the fact that it lives below water, at any rate for some part of the year. Mr. Cambridge has been most obliging in giving me the facts as to the place in which he found the worms: they occur in the gravelly bed of a stream which at certain times of the year runs down so low as to leave small gravelly islands 2 or 3 inches high. In these islands he found *Allurus;* but he finds none in the banks of the stream. We already know of *Criodrilus* as being a thoroughly aquatic earthworm, living in the muddy beds of rivers and lakes; and although this worm has not yet been recorded in Great Britain, I see no reason to doubt that it exists here.

I should add that Mr. Beddard has informed me that he received a specimen of *Allurus* from Lea, Kent, some time after I received these from Hyde. It has been recorded also from Sweden, Italy, and Tenerife. WM. B. BENHAM.

University College.

THE SUN MOTOR.

INDIA, South America, and other countries interested in the employment of sun power for mechanical purposes, have watched with great attention the result of recent experiments in France, conducted by M. Tellier, whose plan of actuating motive engines by the *direct* application of solar heat has been supposed to be more advantageous than the plan adopted by the writer of increasing the intensity of the solar rays by a series of reflecting mirrors. The published statements that "the heat-absorbing surface" of the French apparatus presents an area of 215 square feet to the action of the sun's rays, and that "the work done has been only 43,360 foot-pounds per hour," furnish data proving that Tellier's invention possesses no practical value.

The results of protracted experiments with my sun motors, provided with reflecting mirrors as stated, have established the fact that a surface of 100 square feet presented at right angles to the sun, at noon, in the latitude of New York, during summer, develops a mechanical energy reaching 1,850,000 foot-pounds per hour. The advocates of the French system of dispensing with the "cumbrous mirrors" will do well to compare the said amount with the insignificant mechanical energy represented by 43,360 foot-pounds per hour developed by 215 square feet of surface exposed to the sun by Tellier, during his experiments in Paris referred to.

The following brief description will give a clear idea of the nature and arrangement of the reflecting mirrors adopted by the writer for increasing the intensity of the solar heat which imparts expansive force to the medium propelling the working piston of the motive engine. Fig. 1 represents a perspective view of a cylindrical heater, and a frame supporting a series of reflecting mirrors composed of narrow strips of window-glass coated with silver on the under side. The frame consists of a light structure of wrought iron or steel, provided with transverse ribs as shown by the illustration, each rib being accurately bent to a parabolic curvature whose focus coincides with the axis of the cylindrical heater. It needs hardly be stated that the mirrors supported by the said transverse ribs continue from side to side of the frame, which accordingly resembles a parabolic trough whose bottom is composed of mirrors. It will be readily understood that this trough with its bent ribs and flat mirrors forms a perfect parabolic reflector, to which a cylindrical heater, as stated, may be attached for generating steam or expanding the gases intended to actuate the piston of the motive engine. Regarding the mechanism for turning the reflector towards the sun, engineers are aware that various combinations based on the principle of the "universal joint" may be employed.

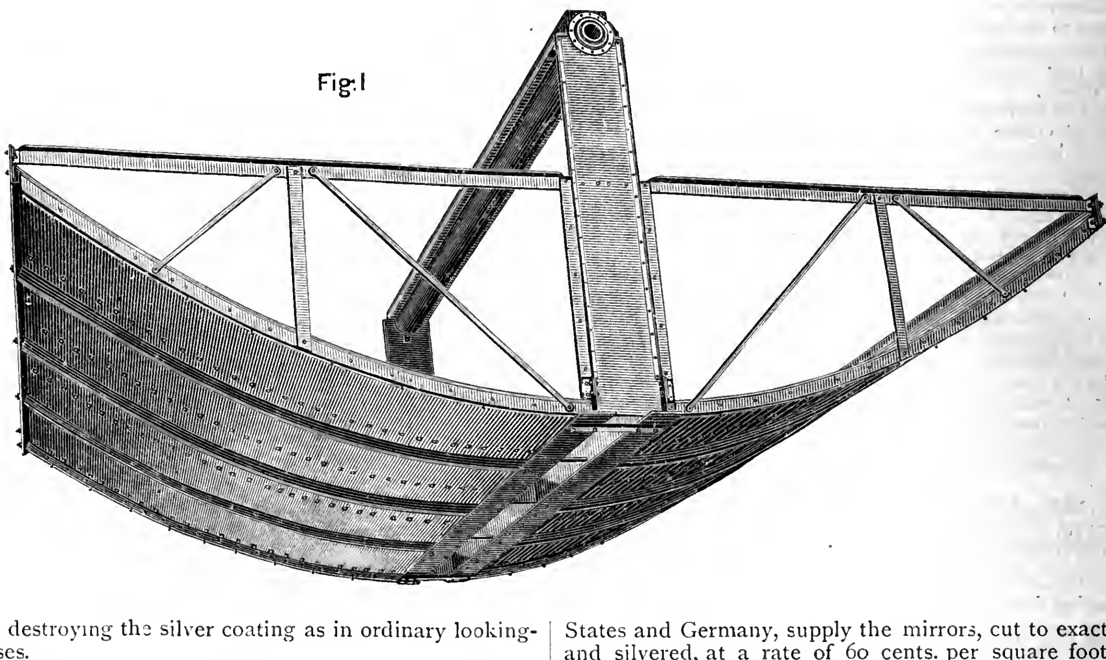
Concerning previous attempts made in France to utilize solar energy for mechanical purposes, it is well known that practical engineers, having critically examined Mouchot's solar engine, which M. Tellier proposes to supersede, find that it is incapable of developing sufficient power for any domestic purpose. Again, the investigations carried out by order of the French Government to ascertain the merits of Mouchot's invention show that irrespective of the great expense of silver-lined curved metallic reflectors for increasing the insufficient energy of direct solar radiation, these reflectors cannot be made on a sufficient scale for motors having adequate power to meet the demands of commerce; nor is it possible to overcome the difficulty of rapid wear of the delicate silver lining of the metallic reflectors consequent on atmospheric influence, which after a few hours of exposure renders their surfaces tarnished and ineffective unless continually polished. A glance at the accompanying illustration (Fig. 1) shows that the reflector constructed for my sun motor differs altogether from that originated by Mouchot, which Tellier's apparatus, tested at Paris, was intended to displace.

Description of the Illustrated Reflector.

(1) The mirrors which reflect the solar rays are devoid of curvature, being flat narrow strips of ordinary window-glass, cut to uniform width and length, perfectly straight.

(2) The under sides of said strips are coated with silver by a process which prevents the action of the sun's rays

Fig. 1



from destroying the silver coating as in ordinary looking-glasses.

(3) The mirrors supported by the bent metallic ribs extending from side to side of the parabolic trough, are held down by the heads of small screws tapped into the ribs. Thin slats of wood may be introduced between the mirrors and the ribs—an expedient of some importance in localities where the reflector is exposed to high winds.

(4) It needs no explanation that the *reflecting surface* of the mirrors cannot become tarnished by atmospheric influence, since the bright side of the silver coating is permanently protected by the glass; hence it will be only necessary to remove *dust* from the mirrors, an operation readily performed by feather brushes secured to light handles of suitable length.

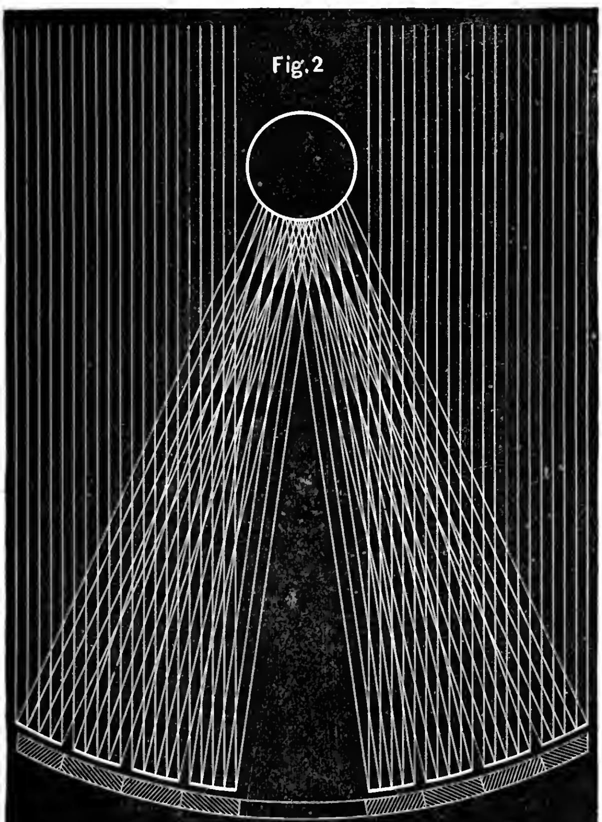
(5) The frame of the reflector, being composed of rolled bars of iron or steel, requires no finish, excepting the top of the transverse ribs, which must correspond accurately with a given parabolic curvature. It should be observed that the needed accuracy is readily attained by a cutting tool guided by a bar of proper form.

(6) Regarding cost of construction, it will suffice to state that manufacturers of glass, both in the United States and Germany, supply the mirrors, cut to exact size and silvered, at a rate of 60 cents. per square foot, the weight being 106 pounds per 100 square feet. Consequently the cost of the reflector and heater for the sun motor will not much exceed that of a steam boiler and appurtenances, including chimney. The cost of the engine apart from the reflector, will not be greater than that of an ordinary steam-engine.

(7) With reference to durability, it will be evident that the light metallic frame with its mirrors, and a heater acted upon only by reflected solar heat, will last much longer than steam boilers subjected to the action of fire, soot, and corrosion.

Let us now briefly consider the distinguishing feature of the sun motor—namely, the increase of the intensity of the sun's radiant energy by *parallel* rays and *flat* reflecting surfaces permanently protected against atmospheric influence. It has been supposed that the lens and the curved reflecting surface, by converging the sun's rays, could alone increase the intensity of radiant heat. But Newton's demonstration, showing that the temperature produced by solar radiation is "as the density of the rays," taught me to adopt in place of curved surfaces and converging rays, flat surfaces and parallel rays, as shown by Fig. 2, which represents a transverse section of part of the reflector. The direct vertical solar rays, it will be seen, act on the mirrors; while the reflected rays, divided into diagonal clusters of parallel rays, act on the heater, the surface of which will thus be exposed to a dense mass of reflected rays, and consequently raised to a temperature exceeding 600° F. at noon during ordinary sunshine.

The cost, durability, and mechanical energy of the sun motor being thus disposed of, it remains to be shown whether the developed energy is continuous, or whether the power of the engine changes with the increase and diminution of zenith distance and consequent variation of atmospheric absorption. Evidently an accurate knowledge of the diathermancy of the terrestrial atmosphere



is indispensable to determine whether the variation of the radiant energy is so great that the development of constant power becomes impracticable. Of course, manufacture and commerce demand a motor developing *full power* during a modern working day of *eight* hours. Observations relating to atmospheric diathermancy continued during a series of years, enable me to assert that the augmentation of solar intensity during the middle of the day is so moderate that by adopting the simple expedient of wasting a certain amount of the superabundant heat generated while the sun is near the meridian (as the steam engineer relieves the excess of pressure by opening the safety-valve) a uniform working power will be developed during the stipulated eight hours. The opening of the safety-valve, however, means waste of coal raised from a great depth at great cost, and possibly transported a long distance, while the radiant heat wasted automatically by the sun motor is produced by fuel obtained from an inexhaustible storehouse free of cost and transportation.

It will be proper to mention that the successful trial of the sun motor described and illustrated in NATURE, vol. xxxi. p. 217, attracted the special attention of landowners on the Pacific coast then in search of power for actuating the machinery needed for irrigating their sun-burnt lands. But the mechanical detail connected with the concentration at a single point of the power developed by a series of reflectors was not perfected at the time; nor was the investigation relating to atmospheric diathermancy sufficiently advanced to determine with precision the retardation of the radiant heat caused by increased zenith distance. Consequently no contracts for building sun motors could then be entered into, a circumstance which greatly discouraged the enterprising Californian agriculturists prepared to carry out forthwith an extensive system of irrigation. In the meantime a simple methcd of concentrating the power of many reflectors at a given point has been perfected, while the retardation of solar energy caused by increased zenith distance has been accurately determined, and found to be so inconsiderable that it does not interfere with the development of constant solar power during the eight hours called for.

The new motor being thus perfected, and first-class manufacturing establishments ready to manufacture such machines, owners of the sun-burnt lands on the Pacific coast may now with propriety reconsider their grand scheme of irrigation by means of sun power.

JOHN ERICSSON.

THE WHITE RACE OF PALESTINE.

ON the occasion of my first visit to Palestine I was struck by the number of blue-eyed, fair-haired children whom I met with in the towns and villages, more especially in the mountainous parts of the country. At the t me I supposed them to be the descendants of the Crusaders or of the other natives of Northern Europe who found their way to the Holy Land during the Middle Ages. But a new light has recently been thrown on the matter by the ethnological observations made by Mr. Flinders Petrie in Egypt.

The winter before last Mr. Petrie was commissioned by the British Association to take casts and photographs of the ethnological types represented on the Egyptian monuments, and to note, wherever it was possible, the colour of the skin, eyes, and hair. It was not the first time, however, that notes of the kind had been taken. Some years ago, Osburn, a careful observer, had noticed that in the sculptures of Ramses II. at Abu-Simbel "the Shasu of Kanana" were depicted with blue eyes, and red hair, eyebrows, and beard, and the Amaur with "the eyes blue, the eyebrows and beard red." As "the Shasu of Kanana" lived a little to the south of Hebron, while the Amaur are the Amorites of the Old Testament, it was clear that a population existed in Palestine in the fourteenth century before our era which had all the characteristics of the white race.

Mr. Petrie's observations have abundantly verified this conclusion. He finds that, on the walls of a Theban tomb, the chief of Kadesh on the Orontes is painted with a white skin, and light red-brown hair. Kadesh was the southern capital of the Hittites, after their invasion of Syria, but the Egyptian inscriptions describe it as being "in the land of Amaur"; and that its chief must have been an Amorite is shown by the fact that the Hittites are depicted with yellow or orange skins, their hair being black, and their eyes dark.

The physiognomy of the Hittites and Amorites, moreover, differed widely. The Egyptian artists agree with the native Hittite monuments in representing the former with ugly protrusive profile, and Mongoloid features, the hair being arranged at the back of the head in a sort of "pig-tail." The Amaur or Amorites, on the other hand, are a handsome people, tall, and dolichocephalic, with large sub-aquiline noses, and a short pointed beard at the end of the chin. The defenders of "the fort of Amaur" are represented as having been burnt a light pink-red by the action of the sun. Otherwise the skin is white or "sallow."

We learn, then, from the ancient monuments of Egypt that a portion of Palestine was occupied by a white race before its conquest by the Israelites. And they further inform us that this white race continued to exist in the country after the conquest. The physical characteristics of the captives taken by Shishak in the time of Rehoboam from the cities of Judah have Amorite and not Jewish features. There is nothing in common between them and the tribute-bearers of Jehu, who are depicted on the black obelisk from Nimroud, now in the British Museum, with faces of a most typically Jewish cast. In the tenth century before our era, consequently, the bulk of the population in the southern part of Judæa must have been of Amorite origin.

It is not wonderful, therefore, if we find traces of the same population still surviving in Palestine. There is no need of explaining their existence by a theory of their descent from the Crusaders. The survival of the ancient white race of Palestine is parallel to the survival of the ancient white race of Northern Africa, now generally known among French writers under the name of Kabyles. The Kabyles were at one time imagined to be the descendants of the Vandals, but we now know that they have inhabited the southern coast of the Mediterranean since the later Neolithic age. They are the Libyans of antiquity, represented on the Egyptian monuments, like the Amorites, with white skins, blue eyes, and dolichocephalic skulls, and similarly described by classical writers. They extended into Teneriffe and the Canary Islands, and their long-headed skulls have been disinterred from the dolmens of Northern Africa.

To the traveller who sees them for the first time the Kabyles offer a striking appearance. Their clear white skins, covered with freckles, their blue eyes and light hair, remind him of the so-called "Red Kelts" he has met with in an Irish village. They bear a high reputation for physical courage and love of independence, though at the same time they seem to be an orderly people. But they have two characteristics which they share with the white race of Northern Europe. They are mountaineers, the climate of the African plains being apparently too hot for them, and they are distinguished by their tall stature.

These were equally the characteristics of the Amorites of ancient Palestine. The Jews declared that their "height was like the height of the cedar," the Semitic tribes by the side of them seeming to be but "grasshoppers," and the iron couch of Og, the Amorite king of Bashan, preserved at Rabbath, afterwards the capital of Ammon, excited the wonder of later generations on account of its size.

The Amorites also occupied the whole of the mountainous district of Syria and Palestine from the neighbourhood of Kadesh in the north to the desert southward of Judah, and on the eastern side of the Jordan they founded the two kingdoms of Bashan and Heshbon. In the mountains of Moab and Seir they formed the aboriginal population, partially dispossessed by the Semitic tribes of Moab, Ammon, and Edom, and the name of Horite under which they went in Edom is best explained as meaning "white," in contradistinction to the Semitic Edomite or "red-man." A passage in the Pentateuch (Numbers xiii. 29) expressly states that along with the Hittites and Jebusites they inhabited the mountainous region, while the Canaanites dwelt on the coast and the valley of the Jordan. That Jebusite simply means a cross between Hittite and Amorite is clear from the statement of Ezekiel (xvi. 3, 4, 5) that Jerusalem, whose old name of Jebus gave rise to that of Jebusite, was born of a Hittite mother and an Amorite father. The Egyptian monuments bear witness to the same "interlocking" of Hittite and Amorite.

There is yet a third characteristic which has been ascribed to the white race of Northern Europe. It has been brought into close connection with the dolmens which cover so large a part of its territory. Faidherbe and others have traced a continuous line of dolmens of similar construction along the northern coast of Africa, through Spain, Portugal, and France, into the British Isles. No one, indeed, who has examined the famous dolmens of Roknia, in Algeria, can fail to be struck by their resemblance to the sepulchral cromlechs of our own country. If they are really due to the genius and influence of a single race, it would seem that the race moved from north to south, since the objects found in the dolmens of the south of France betray a more advanced stage of culture than those found in the north.

The chief objection hitherto raised against ascribing these dolmens to the white race with whom they are associated has been that similar megalithic monuments exist in Palestine. Over 700 have been discovered in Moab on the eastern side of the Jordan. Major Conder has drawn attention to others in the basaltic region in the neighbourhood of the ancient Dan, and though none have as yet been observed in Judah, this is probably due to the fact that the attention of travellers has not been called to them. I have myself come across a fine specimen on a hill to the south of Jenîn which had been overlooked by the Palestine Survey, and that megalithic structures once existed in Judah is evident from the occurrence in the Old Testament of names like Gilgal or "Stone-circle," and Ai or "cairn" (Joshua viii. 29). It will be noticed that they are especially plentiful on the eastern side of the Jordan, where the two chief Amorite kingdoms once flourished. Just as the dolmens of Northern Africa were the burial-places of the ancestors of the Kabyles, so tradition affirmed that the Amorite king of Ai had been buried beneath a cairn of stones.

The discovery that the Amorites of Palestine were racially allied to the ancient Libyans opens up ethnological and archæological questions of considerable interest. These cannot be touched upon here, but must be reserved for a future occasion. It is sufficient for the present to have drawn attention to a new and curious ethnological fact.

A. H. SAYCE.

ENGINEERING SCHOOLS.

AT a time when so much is being said about the need for technical education, especially in engineering, the following letter will be read with interest:—

Engineering School, Trinity College, Dublin, June 1888.

DEAR LORD ASHBOURNE,—As you have requested me to draw up a statement of the claims of engineering schools to be recognized by the Civil Service Commissioners as affording part at least of the technical training required of candidates for engineering Civil Service appointments, I send you the following account.

Allow me, in the first place, to state that I am not advocating the claims of our Engineering School here as in any way distinct from that of many other excellent engineering schools that exist. For instance, the Indian Government is so fully convinced of the absolute necessity for a proper technical school training for engineers that it requires all candidates for Indian engineering appointments to go through Cooper's Hill Engineering School; and yet the Home Civil Service do not in any way even recognize the very same technical training given to other students who stay at home as of any value at all.

The instruction given in engineering schools is of two kinds:—

I. Lectures and demonstrations in mathematics, mechanics, physics, chemistry, geology, &c.; and in the theory and practice of engineering, surveying, &c., &c.

II. Practical training—

(*a*) Practical work in laboratories and workshops in mechanics, machines, physics, chemistry, and field-work in geology.

(*b*) Drawing and office work, including designing, making out specifications, taking out quantities, &c., &c.

(*c*) Practical surveying, and all manner of field work.

(*d*) Inspection of works in progress.

It will be observed what a large and important part of the training given in a school cannot be obtained in an office at all. All the instruction in mathematics, mechanics, physics, chemistry, geology, &c., and in the theory of engineering, and all the important practical laboratory training in these subjects, can only be obtained in a school; and unless an engineer has been properly and *practically* taught these things before entering on his profession, it is almost certain that he will never learn them. In the other more especially engineering parts of the course there are several great advantages in the school course over the office course. In the school, in the first place, the student is under the constant instruction of teachers whose time is devoted to instructing the student, and explaining to him the principles upon which his work depends; and, in the second place, the course of instruction covers as wide a range of subjects as is consistent with teaching each properly. In the office, in the first place, the apprentice has to pick up what instruction he can, and is generally content with a rule-of-thumb knowledge, that may desert him at any really critical juncture; and, in the second place, in any one office the work is yearly becoming more specialized, so that an apprentice will have experience of only a small range of subjects, and, not being acquainted with the theory of even these, will be incompetent to engage in other work.

There are, of course, certain things, such as facility in numerical calculation, and perhaps in the use of field-instruments, acquaintance with the details of specifications in a particular class of work, familiarity with prices at a particular time, and an opportunity of seeing designs carried into execution, which cannot be as well obtained in school as on works The object of a school being to teach, and of works being to pay, neither can completely supply the place of the other. As a course of technical training for a young engineer, the school course is out of all proportion the more important. What can be learnt from the office course will certainly be acquired, while what can be learnt from the school course will hardly ever be acquired, unless learnt before beginning the practice of his profession. In this age of technical education it is practically certain that in a few years no engineer will be recognized as such unless he has had a proper technical school education, just as in the medical profession it has long ago been recognized that, without a proper medical school education, it is impossible for a doctor to learn the many sciences upon which the successful practice of his profession necessarily depends.

Eminent engineers who have had experience of students taught in engineering schools hold opinions similar to those here enunciated. Our late Professor of Engineering, Mr. Crawford, whose engineering experience is world-wide, is of this opinion. Mr. Bindon B. Stoney, Engineer to the Dublin Port and Docks Board, is of the same opinion. Both these have had experience of school-trained students, and think that the proper course for a young engineer to pursue is to go through a course of instruction in a properly-equipped school, and then to go for a year on works. They consider that a year on works is required to complete the education of an engineer, and they think that a short time on works is quite sufficient for a student who has already gone through an engineering school. Mr. Stoney, for instance, takes students who have been through an engineering school as apprentices for one year, although he will not take untrained apprentices for so short a term.

Foreign Governments in general require all who profess to practise as engineers to go through a proper technical school training, and it is a serious difficulty in the way of English engineers who endeavour to obtain employment on the Continent that, even though they may have been trained in an excellent school, yet this is not recognized by foreign Governments, because our engineering schools are in no way recognized by our own Government.

The Civil Service Commissioners should endeavour to encourage the proper scientific training of the engineers they receive into the public service, and they can do so by recognizing the years spent in an engineering school as equivalent to the same number of years of the technical training that is now required. In the more important appointments, which at present require five years' technical training, the candidate would have to supplement his school course by an office course of at least two years; and this, in the opinion of eminent engineers, as quoted above, would be amply sufficient. In the case of the less important appointments, the school training is probably much better than what satisfies the Commissioners at present; but if it is thought that the special qualifications of an office-trained apprentice are essential, they can be easily secured by requiring in every case at least one year's office experience.

The Civil Service Commissioners should, before recognizing any engineering school as giving the instruction qualifying a candidate to compete for an appointment, inspect the school, and see that it is properly equipped, and has the means and teachers required to teach what it professes. For instance, in some schools there is no special instruction in architecture, and this special teaching should be required of any school that was recognized as qualifying candidates for specially architectural appointments. Similarly, in the case of mechanical engineering, some schools have not the means of teaching it properly, and these schools should not be recognized as qualifying candidates for specially mechanical engineering appointments. A school that teaches civil engineering should be recognized as such, and only as such; and similarly, one that only teaches mechanical engineering should be recognized only as such In the case of medical appointments, the State recognition of schools is already fully carried out, so that there can be no insuperable difficulty in doing the same in the case of the engineering appointments.

If the Civil Service Commissioners require further information as to the instruction imparted in engineering schools, it would be well for them to inspect University College, London, the City and Guilds of London Institute, and Cooper's Hill, all of which are easy of access from London; and if they require further information they had better appoint some competent Committee to inspect and report to them generally as to the training given in engineering schools, and as to whether they give a technical training that the Civil Service Commissioners would recognize as equivalent to some years spent in an office; and, if not, how the schools should modify their courses so as to give this instruction. Statements as to the nature and value of instruction made by those interested in it and responsible for it are not so valuable as independent testimony.

In conclusion, I would earnestly press upon the Civil Service Commissioners the very great desirability of their encouraging scientifically-trained candidates to apply for appointments in the Civil Service. The application of scientific principles to engineering is the special feature of our age, and instruction in these principles, and practical training in their application, should be part of the training of every engineer; and this can only be acquired in a properly-equipped school. A want of familiarity with details will surely be remedied, but a want of scientific knowledge will be a lasting cause of danger to the public.

Yours very truly,

GEORGE FRANCIS FITZGERALD.

THE GAPE WORM OF FOWLS (SYNGAMUS TRACHEALIS).

IN the Bulletin of the Buffalo Society of Natural Sciences, vol. v. No. 2, 1886-7, is a paper by Dr. H. D. Walker, which does not appear to have been noticed in this country, on "The Gape Worm of Fowls (*Syngamus trachealis*)." The writer claims to have discovered that the common earthworm (*Lumbricus terrestris*) is the intermediate host of this well-known parasite, and to have observed it in all stages of its development. He further suggests the use of common salt on infected poultry runs to secure the extermination of these noxious pests by destroying the worms which harbour and distribute them.

The series of experiments by which he has arrived at his conclusions are interesting, and afford strong presumtive evidence of their correctness. The earthworms were carefully dissected and examined, the embryonic form of *Syngamus* being found in them, "differing but slightly in structure, so far as can be discovered from the embryo which has passed through one moult after the egg has hatched in water."

The question may be asked: Why should it differ at all if it is the same? It may be suggested that earthworms are themselves subject to various intestinal parasites and that the embryonic forms of many species and even genera are scarcely distinguishable from each other; but with a view to obtaining corroborative evidence Dr. Walker fed some chickens with worms obtained from a place where *Syngamus* had not been noticed. These chickens did not develop the gapes. An examination of worms from this spot showed them to be free from embryos such as were found in others. The double observation certainly points to the probability that in the first instance the embryo of *Syngamus* had been rightly recognized.

Embryos were also found in the œsophagus and in the lungs of birds to which earthworms taken from an infected locality, but carefully washed and cleansed externally, had been given.

The only link apparently wanting to complete the chain of evidence is to determine the manner in which the parasite (if it be truly the embryo of *Syngamus*) makes its way into the intestinal canal of the earthworm.

Dr. Walker concludes that it is taken in with its food. His evidence upon this point is chiefly negative. Eggs of *Syngamus* were placed on damp earth in a dish to which living earthworms were added a fortnight later. After ten days chickens were fed with these worms, but were not attacked. This experiment would have been more complete and perhaps conclusive if the worms had been supplied at the same time with vegetable food. Unless the worms were fed, the only means of entry for the embryos of the parasite must have been by boring through the outer integument of their bodies, which is not suggested.

Dr. Walker notices and examines somewhat critically a paper by Dr. Pierre Mégnin, published under the auspices of the Entomological Society of London in 1883, in which the author, after a minute inquiry into the history, habits, and development of *Syngamus trachealis*, came to the conclusion that the epidemic of gapes is spread, first by "food or drink which has become infested with eggs or embryos; secondly, (by) the diseased birds themselves, which are constantly disseminating the eggs of the parasite; and therefore all other living agents, perfect insects, larvæ, or mollusks (for example, the larvæ of ants, which are the habitual food of young pheasants, have been suspected, with some appearance of reason) may be acquitted of any share in spreading the disease." The American author disputes these conclusions. Admitting that the eggs will hatch in water, and that the embryos may be taken in by birds drinking infected water, he finds no instance, after repeated experiments, in which eggs swallowed by a bird have produced the disease, and although he thinks that exceptional cases might occur, he concludes that the instrumentality of the intermediate host is not ordinarily dispensed with. This is the only material point in which Walker differs from Mégnin, and there is nothing in Walker's discoveries to impair the accuracy of Mégnin's observations, so far as they go. Dr. Walker's observations on the structure and development of the parasite from the egg through its embryonic stages agree substantially in all other respects with those of Dr. Mégnin, except that he believes "the egg of *Syngamus* within the perfect worm just arrived at maturity does not contain a developed embryo," whereas Mégnin found "embryos quite perfect and living in eggs not yet freed from the decomposing bodies of female *Syngami* attached to the tracheal mucous of pheasants that had died of gapes."

The discovery of the distribution of these parasites through the instrumentality of earthworms, which are undoubtedly a favourite food of all young game birds, as well as of domestic fowls, is especially interesting to game preservers, and the theory is strongly supported by their experience.

First, if, as Dr. Mégnin believed, the eggs could be hatched only in water, a gamekeeper could have counted upon reducing to a minimum the risk to his artificially-reared birds by depriving them of water and feeding them upon food carefully moistened with pure spring water only, or more conveniently, upon water that had been first boiled. Many have followed this rule habitually and with good results, but certainly without securing any immunity from occasional outbreaks of the "gapes disease." Secondly, all who have had any experience in rearing pheasants or partridges, or have observed the growth and health of broods of the young of these birds in a wild state, must have noticed that very dry summers are much more favourable to the maturing of full broods and coveys than those in which a greater degree of moisture prevails, but if after very dry weather copious showers or very heavy dews moisten the surface of the ground when the birds have not yet attained their full growth, an outbreak of gapes is almost certain to follow, and is very rapid in its effects. So long as the ground is hard and dry earthworms do not come to the surface, but whenever it becomes sufficiently moistened to permit them to throw up their casts and to reach the surface, all species of birds of which they form a natural or favourite food are eager to seek and to devour them. The birds named by Dr. Walker as those in which *Syngamus* has been found are, with the single exception of the swift, all worm-eating birds. He does not mention on what authority the swift is included in the list, but it is difficult to understand, if water is to be regarded as the only medium of conveyance for this parasitic disease, why many other birds should not also have been found to be affected by it. We believe Dr. Walker's discovery has been received in America with some incredulity, but apart from the careful observations and experiments on which he relies, the accuracy of which there seems to be no good reason to dispute, the field experience of those who have had the best opportunities of forming an opinion on the subject would tend to support the probability that his conclusions are in the main correct.

WALSINGHAM.

NOTES.

MEN of science will be glad to learn that, at a meeting recently held at Dr. George Johnson's house, it was proposed to make Sir William Bowman some acknowledgment of the appreciation in which he is held on account of his high character, and professional and scientific attainments. A portrait of himself was suggested, and also, possibly, a reprint of some of his publications. Dr. George Johnson, Mr. J. W. Hulke, and Prof. Burdon Sanderson undertook to see Sir William Bowman, and ask his acceptance of the proposal. This consent having been received, a Provisional Committee was at once constituted, at whose invitation a number of eminent men of science formed themselves into the first list of the "Committee of the Bowman Testimonial Fund." As this body is already large and widely scattered, the practical carrying out of the scheme has been relegated to a Sub-Committee, consisting of the Treasurer (Dr. George Johnson), the Secretaries (Dr. W. A. Brailey and Dr. W. H. Jessop), Mr. Power, and Prof. Klein. It is not proposed to place any limit in either direction to the amounts of individual subscriptions, though the Committee are generally of opinion that large subscriptions will be found unnecessary, and that the compliment is a greater one when paid by a longer list of comparatively small subscriptions. They also hope that the funds will allow the distribution of a good reproduction of the portrait to subscribers of at least two guineas. Mr. Frank Holl, whose sudden death is deeply deplored by all who interest themselves in English art, had undertaken to paint the portrait.

IN the House of Commons on Tuesday Sir H. Roscoe asked the Chancellor of the Exchequer whether the astronomical instruments for the international photographic survey of the heavens, recommended by the Royal Societies of London and Edinburgh and the Board of Visitors of the Greenwich Observatory, the estimates for which had been forwarded from the Admiralty some months since to the Treasury, were yet ordered; and, if not, whether, in view of the fact that all the thirteen other sets of instruments were ordered by foreign and colonial Governments last year, and consequently the British Observatories would be placed at a serious disadvantage, Her Majesty's Government would be prepared to put the necessary amount on the Estimates in order to avoid further delay. To these questions the Chancellor of the Exchequer returned the following answer:—"The astronomical instruments required for the international photographic survey of the heavens have not yet been ordered, and the House will soon be asked to vote the necessary funds. It is, I believe, the case that thirteen instruments have been already ordered by different Powers and public bodies, but the hon. member is mistaken in supposing that all the Powers whose co-operation is contemplated have as yet ordered their instruments. On the contrary, two of the Great Powers, so far from ordering their instruments, have not yet definitely declared their intention to take part in the work. I do not think there is any cause to fear that Great Britain will be behindhand in the matter."

AMONG the Civil List pensions granted during the year ended June 20, 1888, were the following:—To the Rev. F. O. Morris, in recognition of his merits as a naturalist, £100; to Mr. William Kitchen Parker, F.R.S., in recognition of his services to science as an investigator, £100; to Mrs. Balfour Stewart, in recognition of the services rendered to science by her late husband, £50.

THE summer meeting of the Institution of Mechanical Engineers was opened at Dublin on Tuesday. In his Presidential address, Mr. Carbutt did not confine his remarks to purely mechanical subjects, but drew the attention of the members to some statistics relating to the population of Ireland and to Irish agriculture and industries. Mr. Carbutt expressed a decided opinion to the effect that more money should be spent in Ireland on education, and especially on technical education. "What I mean by technical training," he said, "is teaching children to use their hands and eyes, and also giving them such practical acquaintance with the applied sciences as may bear upon the industrial employments in their district. I hope the valuable speech on the need of technical education, made by the Marquess of Hartington at our annual dinner in May, will be widely read. I may refer to the work done in the agricultural school at Glasnevin, three miles out of Dublin, of which Mr. Carrol is the head. To this school is attached a farm of 180 acres for teaching practical farming. The Munster dairy school, started in 1880 with a farm of 126 acres, is quite full, and frequently has to refuse pupils. The Government grant to these two schools is £2671. The Baltimore industrial school, the Public Works Commissioners state, will practically be a technical school of fishing. The Belfast technical school is very successful in training pupils in flax cultivation and spinning. Dairy schools have been established twenty years in Denmark, Sweden, Germany, and Normandy. Let me give an example of what the result has been in Denmark. A Report on agricultural dairy schools has been lately presented to Parliament from a Departmental Commission presided over by Sir R. H. Paget, M.P., which states that in 1860 the British Vice-Consul at Copenhagen reported that the butter made in that country was execrably bad. What has happened? Denmark has now ten State-aided dairy schools, with the result that her exports of butter to the United Kingdom have increased as follows:—

1867	80,000 cwts.,	value £422,479
1877	210,322 ,,	,, 1,347,791
1887	487,603 ,,	,, 2,669,123

In France theoretical and practical lessons in agriculture are now given every week in the primary schools; and a circular has been issued inviting the municipalities to provide for every district a demonstration plot of not less than half an acre for the purpose of applying the principles taught in the school."

TWO rather striking speeches on education were delivered at the Sorbonne on Monday at the distribution of prizes to the successful students of the great secondary schools of Paris. M. Blanchet, Professor of History at the Lycée Charlemagne, while expressing a high opinion of the value of the ancient classics in education, urged that methods of instruction should be adapted to the actual wants of the present day. He quoted the following passage written by Fleury at the end of the seventeenth century: "It seems to me that we ought to accommodate our studies to the present state of our manners, and to study those things which are of use in the world, as we cannot change this use so as to accommodate it to the order of our studies." "Truly," said M. Blanchet, "these old pedagogues were great revolutionists. What is new in the history of French pedagogism is not the spirit of innovation and progress but that of routine." M. Lockroy, the Minister of Public Instruction, spoke in a similar tone. It was essential, M. Lockroy pointed out, that Frenchmen should know what was said and written beyond their frontiers. Science was progressing everywhere, and they should be able to follow its progress abroad, especially in Germany and England. That was one reason why the modern languages had such a strong claim on the young of this generation. M. Lockroy protested against the notion that anyone thought of destroying Greek and Latin studies. But these studies were not the only solution of the very complicated problem of modern education. Accordingly, he had thought it right to take an opportunity of stating that the problem was receiving close attention. The University was anxious to study it, and would bring to the work its high sentiment of duty, and its passion for the public good.

ON Monday Mr. Howorth asked the Under-Secretary of State for Foreign Affairs whether, in view of the continuous and deplorable destruction of the ancient monuments of Egypt by travellers and others, and of their incomparable value and interest, it would be possible to appoint some Engineer officer to make a survey of those monuments and to have custody of them in future. Sir J. Fergusson replied that it rested with the Egyptian Government to take the necessary measures. A Special Committee had been appointed to consider what ought to be done in the matter, and it had been decided to levy a small fee for seeing the antiquities. This would to some extent increase the sum which it was possible to devote to the preservation of ancient monuments.

THERE is no difference of opinion as to the great variety of uses to which aluminium might be applied if it could be produced in sufficient quantities at a reasonable cost. Hitherto it has been produced, almost entirely in France, by the Deville process; and this process involves so considerable an expenditure that the results have been by no means satisfactory. About seven years ago, Mr. H. Y. Castner, of New York, began experiments in that city with a view to improve the Deville process and cheapen the cost of aluminium by reducing the cost of producing the sodium from which it is obtained. Two years since, Mr. Castner erected experimental works at Lambeth, where he succeeded, after nearly eighteen months of further experimentation, in satisfying a number of men of science and others that he could produce sodium at one-fifth and aluminium at one-third of the cost previously incurred. A company was thereupon formed in order to take up and work the Castner patents. In October last the foundation-stone was laid of new works at Oldbury, near Birmingham, for the production of both sodium and aluminium on a large commercial scale; the works were virtually completed, and the successful manufacture of these products was begun about a fortnight ago; and a large number of gentlemen were invited to visit the works on Saturday last, and witness the processes in actual operation. Among those who accepted the invitation to be present were the Right Hon. A. J. Balfour, M.P., a trustee for the debenture-holders; Sir Frederick Abel, C.B., F.R.S.; Sir Henry Roscoe, M.P., F.R.S.; Lieut.-General Sir Andrew Clarke, G.C.M.G., C.B.; Prof. C. Roberts-Austen, F.R.S., of the Mint; Prof. Dewar, F.R.S.; Dr. Crookes, F.R.S.; Dr. Hugo Müller, F.R.S.; Lord Rayleigh, F.R.S.; Prof. Huntingdon, and others. According to the *Times*, only one opinion was expressed by the gentlemen who visited the works—some of them among the highest authorities on the subject—as to the practical success of all the operations witnessed, and the admirable arrangement of the plant employed. Mr. Castner was freely complimented on the skill and success with which he had developed his system.

DR. HANS REUSCH, of the Norwegian Meteorological Institute, who is engaged in collecting particulars of the earthquakes which occur in Norway yearly, has issued his report for 1887, from which it appears that earthquakes are far more frequent in Norway than has hitherto been imagined. Reports were received of twenty-three, all of which were faint, except three. One occurred on the night of May 7 in the Bömmel Islands, on the west coast, and was accompanied by subterranean detonations, another in the Islands of Værö and Röst, at the extreme point of the Lofodden Group, where doors and windows clattered and the slates on the roofs were pitched off. Again, on November 5, a severe shock of earthquake was felt at Bodö, on the north-west coast. Of the minor shocks those which frequently occurred on the Ytterö are particularly remarkable, as this island lies far out in the ocean, off the coast of Söndfjord.

THE International Meteorological Committee will hold its fourth meeting at Zürich on September 3. This will be the final meeting of the Committee as so constituted. For various reasons it has been found impracticable to organize an International Meteorological Congress, more than one Government having declined to take part in such an assemblage. It is probable that, in future, occasional meetings will be held of a body to be composed of the chiefs of the various existing meteorological services, to whose meetings nothing of a diplomatic character will attach. The arrangements connected with such Conferences have yet to be made.

IN the *American Meteorological Journal* for June, Mr. A. L. Rotch describes the meteorological organization of Austria and the independent observatories in connection with the Central Institute (not including those of the Hungarian service). There is a regular telegraphic weather service, but no storm warnings are issued; an agricultural service, however, exists in the summer season. The pressure at the high mountain stations is reduced to the level of 2500 metres. Mr. G. E. Curtis contributes an article on the trans-Mississippi rainfall, with reference to the popular belief that the rainfall is increasing in the Middle and Western States, the increase being attributed to the building of railroads and the extension of cultivation. Whether the amount of rainfall has actually increased or not does not appear to be proved; the author points out, however, that the breaking-up and tillage of the soil have increased its moisture, and with the growth of vegetation there have come an increased humidity of the atmosphere and a more general diffusion of rainfall. As an evidence of this result it is stated that the streams have a much more even flow than formerly. Dr. A. Woeikof offers an explanation of the different views of Mr. A. Hazen and Dr. Hann as to the general "inversion of temperature" in areas of high and low pressure. Mr. Hazen objects that the statement that, during the passage of anticyclones, the temperatures on high mountains are high in winter, is not applicable to Mount Washington, and thus no law at all. Dr. Woeikof supports Dr. Hann's views, and explains that the exception pointed out by Mr. Hazen may be due to the different type of weather in the Eastern States and in Europe, and to the greater rapidity of the passage of anticyclones in the former locality.

ANOTHER contribution to the chemistry of the rare earths, by Drs. Krüss and Kiesewetter, will be found in the current number of the *Berichte*. The somewhat startling results published a year ago by Drs. Krüss and Nilson, involving as they did the announcement of the existence of a large number of new chemical elements, appear to receive additional confirmation by this subsequent work undertaken by the two former chemists. They are not yet in a position to announce the complete isolation of any one of these new elements, but so much progress has been made in this direction that a mixture containing only two of them in any quantity has been arrived at. The task of separating these elementary constituents from the minerals which have hitherto been examined appears, in the face of the fact that their properties are so similar—their known salts being almost equally soluble, and the basicities of their oxides so nearly alike—well-nigh impossible. But the results of the examination of a large number of Scandinavian minerals show that Nature herself, with her infinite resource of time and circumstance, has partially, possibly in some yet unknown instance completely, performed this long and laborious operation for us. Different minerals from the same place, and even the same mineral from different localities, are shown by the absorption-spectra of their nitrates to consist of different constituents in varying quantities. Hence, by extending the observations over a large number of specimens it is possible to find a few which contain only a small number—one, two, or three—of these new elements in any considerable quantity. Working upon this principle, Drs. Krüss and Kiesewetter have been fortunate in discovering a mineral, yttro-titanite of Arendal, the absorption-spectrum of whose nitrates indicates the presence in large quantity of only two elements, viz. that constituent of didymium termed Diδ, and the constituent Xζ of holmium. The bands of these elements are very intense, and are of wave-lengths 521·5 and 452·6 respectively. Samarium is entirely absent, but there are small quantities of constituents of erbium and thulium present. However, the Diδ and Xζ so largely preponderate, that their fractionation is being undertaken. This happy discovery goes very far to prove the accuracy of the deductions made by Krüss and Nilson, which have caused so much discussion in chemical circles; for of the elements composing the mixture called didymium we have here only one of them, and of the constituents of holmium we have likewise but one representative. Therefore the compound nature of didymium and holmium may now be taken as proved.

At the meeting of the Scientific Committee of the Royal Horticultural Society, on the 24th ult., Dr. Masters showed ripe fruits of the Plymouth strawberry, grown from plants presented to him by Mr. G. F. Wilson. This curious monstrosity is an alpine strawberry, in which all the parts of the flower are more or less represented by leaves. The plant was mentioned by old botanical writers, but afterwards disappeared, or was so completely overlooked that its very existence was assumed to be a myth. Of late years, however, the plant has reappeared in several gardens, and the correctness of the old writers has been vindicated.

Plaster-of-Paris models of the bed of the Atlantic Ocean and of that of the Carribean Sea have been sent by the United States Hydrographic Office to the Cincinnatti Exhibition. They were made by Mr. E. E. Coart, of the Hydrographic Office; and the charts from which they were constructed were carefully revised by Commander J. R. Bartlett and Lieut. J. L. Dyer, respectively former and present Hydrographer. *Science* suggests that duplicates or even photographs of these models would be of very great value in the teaching of physical geography. That of the bottom of the Atlantic would, says our American contemporary, give a pupil more actual instruct ion in a quarter of an hour than could be obtained by a week's study of descriptive text. This model, it seems, shows many things that will be surprising to almost everybody except the expert hydrographer. One of these is the great height of many of the small islands from the ocean's bed, when compared with their area either above the surface of the water or where they rest upon the bottom of the sea. This height is exaggerated in the model by the perpendicular scale being made fifty times as great as the horizontal scale; but, even allowing for that, these islands stand up like tall, narrow, truncated cones, many of them not being more than twice as far across at the base as at the top.

The United States Fish Commission lately sent off to California 600 live lobsters, 350 of which arrived safely at Sacramento. Several attempts had previously been made to send live lobsters across the North American Continent, but had failed. In the present instance, as we learn from *Science*, Colonel McDonald, Fish Commissioner, personally superintended the packing of the lobsters. A crate or box devised by the late Captain Chester was used. This was placed within another larger box, the intervening space being filled with pounded ice. In the inner box the lobsters were placed between layers of rock-weed, which at times was moistened with sea-water. Each box had an independent drain, so that the fresh water from the melting ice could not enter the lobster-box. The temperature of the latter was kept at 45° F. A Fish Commission car was used, the boxes along the side of it serving as the outer box of the combination described above; one hundred crates, each containing six lobsters, being placed in them, and surrounded with ice. Each morning before sunrise a careful inspection of the lobsters was made, and those that had died were removed. The first day 45 died; the second day, 55. After that the mortality was much less. All of those that died were in an advanced state of shedding, and were in poor condition when they started. One half of the 350 lobsters that arrived safely on the Pacific coast were placed in the ocean north of San Francisco, and the other half south. The condition of the water in that region is similar to that of the Atlantic off the Massachusetts coast. The temperature is about the same, but is more constant. The lobster on the Massachusetts coast crawls out into deep water in the summer, where the temperature is low, but it is thought that the equable temperature of the Pacific will enable the lobster in those waters to spend the whole year in one spot.

An account of two interesting old globes in the library of the Middle Temple will be presented in the next volume of the Hakluyt Society's series. These globes, one terrestrial, the other celestial, were made by E. Molyneux in 1593, and were the first ever produced in England. The geography on the terrestrial globe was afterwards brought down to 1603. A description of the globes was written in Latin in 1593 by Robert Hughes, a mathematician of the period. This description was rendered into English by Chilmead, of Oxford, in 1623; and Chilmead's translation, which has been prepared for publication by Mr. Coote, of the Map Department of the British Museum, will form the substance of the forthcoming volume. The editor of the volume is Mr. Clements Markham.

The Report of the Council of the North-Eastern Sanitary Inspection Association for 1887–88—the fifth financial year of the Association—has been issued at Newcastle. Excellent work is evidently being done by the Association. One of its good deeds has been the formation at Newcastle of a permanent exhibition of sanitary appliances. This exhibition was fitted up at considerable outlay by the Association as well as by exhibitors, and is open daily, free to the public, to whom it has proved of great value. "To see the best appliances in each department properly fitted," says the Report, "and to have any explanation desired freely given, where there is nothing on sale, are advantages that must be the better appreciated the more widely they are known. So far as known, there is no better permanent collection in the Kingdom."

In the *Entomologist's Monthly Magazine* for August, Dr. R. C. R. Jordan presents a list of species of Lepidoptera taken by him during a short visit to Jersey. In this list there are several species which have not hitherto been known to occur in the Channel Islands. Dr. Jordan proposes that a Committee of working entomologists should be formed for the thorough investigation of all orders of insects inhabiting these islands.

We have received the second supplement of Mr. John Wheldon's Botanical Catalogue. It includes, besides a large number of books relating to botanical subjects, many important works on agriculture.

The Calendar of the Heriot-Watt College, Edinburgh, for the session 1888–89, has been issued; and it is satisfactory to find that in this well-known institution provision is made for that higher commercial and technical education about which so much has lately been said. It is claimed that the College possesses, in its lecture theatres, laboratories, and workshops, every facility for preparing young men for work as merchants, manufacturers, or engineers, and for supplying in the evening such instruction as is required by those already employed in such occupations.

AT a recent meeting of the Wellington Philosophical Society, Mr. J. W. Fortescue spoke of the rapid increase of deer that have been acclimatized in the New Zealand mountains. Having had special facilities for observing these creatures, he proceeded to state some interesting facts as to their habits. At the close of his address Sir James Hector asked Mr. Fortescue, as an expert on the subject, whether the chief use of the antlers was not so much for fighting as for facilitating the progress of the stag through dense woods. He had considerable experience with the wapiti, in North America, and found that by throwing up the head, thereby placing the horns along the back, the animals were enabled to go forward with great rapidity and follow the hinds. He asked this, as it had been stated at a previous meeting of the Society that the antlers tended to entangle the deer. Mr. Fortescue said that Sir James Hector was quite correct in stating that the antlers assisted the stags in penetrating dense forests. Mr. Higginson also bore out this statement from his experience in India.

ON July 23, at 11.17 p.m., a brilliant meteor was seen in the province of Småland, in Sweden. At Nexjö it was seen due east, falling perpendicularly towards the horizon, when it suddenly burst.

DURING the month of June severe frosts occurred in the north of Finland, doing great damage to the crops.

NORWEGIAN hunters returning from the Arctic regions report much ice and severe storms.

ZOOLOGICAL GARDENS are being laid out in Christiania and Helsingfors.

THE additions to the Zoological Society's Gardens during the past week include a Feline Douroucouli (*Nyctipithecus vociferans*) from Savanilla, presented by Master Lester Ralph; a Crested Grebe (*Podiceps cristatus*), British, presented by Mr. W. Nicholls; a Brazilian Cariama (*Cariama cristata*) from South-East Brazil, presented by Mr. Fredrick Rose, jun.; an Indian Kite (*Milvus govinda*) from India, presented by Mrs. Dean; a Green Turtle (*Chelone viridis*) from the West Indies, presented by Baron Henry de Worms; a Hawk's-billed Turtle (*Chelone imbricata*) from the Bahamas, presented by Mr. W. T. Manger; a Corn Snake (*Coluber guttatus*) from North America, presented by Mr. J. Garnett; a Common Viper (*Vipera berus*), British, presented by Mr. F. C. Smith; a Virginian Fox (*Canis virginianus* ♀) from North America, deposited; a Derbian Screamer (*Chauna derbiana*) from the Northern Coast of Columbia, a Prince Albert's Curassow (*Crax alberti* ♀) from Columbia, four Beautiful Grass-Finches (*Poëphila mirabilis*), four Gouldian Grass-Finches (*Poëphila gouldiæ*) from Australia, purchased; two Rose-coloured Pastors (*Pastor roseus*) from India, received in exchange; two Collared Fruit Bats (*Cynonycteris collaris*), two Mule Deer (*Cariacus macrotis* ♂ ♀), a Canadian Beaver (*Castor canadensis*), a Thar (*Capra jemlaica*), born in the Gardens; a Brazilian Cariama (*Cariama cristata*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

VARIABLE STARS.—Mr. Sawyer gives, in Nos. 174 and 176 of *Gould's Astronomical Journal*, the results of his observations of variable stars in the year 1887. The following are the observations for the more regular variables:—

R Virginis	M	June 17	Mag. 7·1	Calculated June 21
S Coronæ	M	Apr. 19	7·1	Apr. 6
R Lyræ	M	Sept. 9		Aug. 31
	M	Oct. 15		Oct. 16
	m	Nov. 10		Nov. 16
	M	Nov. 29		Dec. 1

The calculated dates are those which have been given in NATURE in the column headed "Astronomical Phenomena." U Monocerotis was observed at maximum on Jan. 15, March 4, April 28; and at minimum Feb. 18 and April 6; R Scuti was observed at maximum on Oct. 27, and at minimum on Sept. 14 and Nov. 23; W Cygni was at minimum, mag. 6·7, on July 23 and Dec. 8, and at maximum, mag. 6·1, on Sept. 13; Mira Ceti was at maximum, mag. 4·4, on 1886 December 30.

Mr. John Tebbutt reports (*Astr. Nachr.*, No. 2849) that η Argûs has undergone a notable increase of brilliancy of late, as he observed it as 7·0 mag. on May 19 of this year; whilst on April 23, 1887, it was only 7·5.

COMET 1888 *a* (SAWERTHAL).—The following ephemeris for Greenwich midnight for this object is from the *Dun Echt Circular*, No. 157:—

	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log *r*.
Aug. 3	1 3 26	53 51·6 N.	0·3409	0·3881
5	1 2 5	54 5·5		
7	1 0 35	54 18·3	0·3424	0·3973
9	0 58 55	54 30·0		
11	0 57 5	54 40·5	0·3439	0·4062
13	0 55 5	54 49·8		
15	0 52 57	54 57·8	0·3455	0·4149
17	0 50 40	55 4·4		
19	0 48 15	55 9·7	0·3471	0·4234
21	0 45 42	55 13·6		
23	0 43 2	55 16·0 N.	0·3489	0·4316

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *AUGUST* 5–11.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 5

Sun rises, 4h. 33m.; souths, 12h. 5m. 41·8s.; sets, 19h. 39m.: right asc. on meridian, 9h. 3·6m.; decl. 16° 48′ N. Sidereal Time at Sunset, 16h. 38m.

Moon (New on August 7, 18h.) rises, 1h. 57m.; souths, 10h. 8m.; sets, 18h. 17m.: right asc. on meridian, 7h. 5·8m.; decl. 21° 12′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury	2 55	10 54	18 53	7 51·3	20 47 N.
Venus	5 7	12 35	20 3	9 32·6	16 2 N.
Mars	12 37	17 21	22 5	14 19·9	15 17 S.
Jupiter	14 16	18 40	23 4	15 38·6	18 44 S.
Saturn	4 13	11 55	19 37	8 52·7	18 15 N.
Uranus	10 16	15 54	21 32	12 52·7	4 58 S.
Neptune	23 17*	7 4	14 51	4 1·3	18 58 N.

* Indicates that the rising is that of the preceding evening.

Aug.	h.	
6	9	Mercury in conjunction with and 0° 18′ north of the Moon.
7	10	Saturn in conjunction with and 0° 16′ south of the Moon.
7	—	Partial eclipse of Sun: visible as little more than a bare contact at Greenwich, beginning at 18h. 49m. and ending at 19h. 6m.
8	9	Venus in conjunction with and 0° 42′ south of the Moon.
10	23	Mercury at least distance from the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m	
U Cephei	0 52·4	81 16 N.	Aug.	9, 19 49	*m*
Algol	3 0·9	40 31 N.	,,	5, 20 2	*m*
U Hydræ	10 32·0	12 48 S.	,,	11,	*M*
δ Libræ	14 55·0	8 4 S.	,,	9, 23 26	*m*
U Coronæ	15 13·6	32 3 N.	,,	9, 1 42	*m*
U Ophiuchi	17 10·9	1 20 N.	,,	9, 1 16	*m*
			,,	9, 21 24	*m*
Z Sagittarii	18 14·8	18 55 S.	,,	8, 0 0	*m*
β Lyræ	18 46·0	33 14 N.	,,	9, 21 0	*M*
η Aquilæ	19 46·8	0 43 N.	,,	6, 22 0	*m*
X Cygni	20 39·0	35 11 N.	,,	8, 1 0	*M*
T Aquarii	20 44·0	5 34 S.	,,	5,	*M*
T Vulpeculæ	20 46·7	27 50 N.	,,	10, 23 0	*M*
δ Cephei	22 25·0	57 51 N.	,,	9, 20 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.		Decl.	
	°		°	
The *Perseids*	44	...	56 N.	... Max. August 10. Swift; streaks.
Near 41 Arietis	44	...	25 N.	... Swift; streaks.
	96	...	72 N.	... Slow.
Near θ Cygni	293	...	52 N.	... Rather slow.

ON PARTIAL IMPREGNATION.[1]

DURING our researches on the formation of polar-bodies (see NATURE, vol. xxxvi. p. 607) we made the following observations, which are of considerable interest in connection with the theory of sexual reproduction.

As we were able to show that parthenogenetic eggs form only *one* polar-body, while sexual eggs give rise to *two*, we looked out principally for those cases in which both kinds of eggs are present in the same species.

On examining the sexual eggs ("Dauereier") of certain species of *Moina*, we found, to our astonishment, that even those which possessed a firm vitelline membrane, and in which four segmental cells were already present, still contained a sperm-cell.

We first of all took this to be a supernumerary spermatozoon which had penetrated into the egg, but it was soon apparent that all eggs of a corresponding stage contained a similar sperm-cell, and that there was always *one* only. Further observations showed us *that we had had here to do with a case of partial impregnation. Only one of the first four segmental cells, and not the entire egg-cell, becomes united with the sperm-cell.* This is the case, at least, in *Moina paradoxa*. In *Moina rectirostris*, impregnation must occur at a rather later stage, for in this species we have seen eggs in which the first four segmental cells were again ready for division, and still the sperm-cell had not fused with one of them.

In *Moina paradoxa* the process takes place as follows:—Immediately after the extrusion of the egg into the brood-chamber, it is a naked sausage-shaped mass. In this stage, a spermatozoon penetrates into it in the region of the vegetative pole, and then the vitelline membrane becomes formed, and prevents the entrance of a second. The germinal vesicle at the same time becomes transformed into the first polar-spindle, which lies at the surface; the first, and soon afterwards the second polar-body then becomes constricted off, and the nucleus of the ovum, surrounded by protoplasmic particles, migrates to the centre of the egg, which has by this time contracted to the usual form. Now follows the first division of the ovum, which, however, only consists in a separation of these first, or, as we will call them, *secondary* egg-cells in the centre of the egg;—the two first segmental cells come to lie, as usual, in its longitudinal axis—one, which is always recognizable by the proximity of the polar-bodies, nearing the animal pole, the other the vegetative pole. *The sperm-cell always lies in the neighbourhood of the latter*, without, however, yet becoming united with it.

Then follows a second division of the segmental cells, together with the separation of the daughter-cells in the transverse direction. There are now four star-shaped daughter-cells present, which lie at an almost equal distance apart, at a right angle with one another. The sperm-cell can be seen near one of the two lower (*hintern*) cells, and it now begins to show amœboid movements, and to approach the segmental cell, a short narrow bridge of protoplasm being formed, and the two cells beginning to unite with one another. *Fusion then follows, and in the next following stage, of eight segmental cells, no sperm-cell can any longer be seen in the egg.*

The uniting of the sperm-cell with the cell- and nuclear-constituents of the egg thus only takes place *after* the embryonic development has already advanced to the four-celled stage. It would naturally be of great interest to know what eventually becomes of those segments which are concerned in fertilization—that is, which parts of the embryo are formed from them. A very possible supposition is, that only those parts of the egg become fertilized out of which the germ-cells of the young animal will subsequently be formed. This conjecture is rendered by no means improbable by the fact that it is one of the two segmental cells lying at the vegetative pole of the ovum which becomes fertilized; for it is from these cells, according to Grobben's beautiful discovery with regard to the summer eggs of *Moina*, that the germ-cells arise. At a future time we hope to be able to speak more definitely on this point: at present it is only necessary to add that we are studying these processes in other *Daphnidæ*, and have already observed a similar series of stages in *Sida crystallina* to those above described. But in this case fertilization occurs earlier, in the two-celled stage of segmentation.

Freiburg i/B., December 12, 1887.

P.S.—In the continuation of the above observations another case has presented itself, in which impregnation does not take place until eight segmental cells have been formed. This happens in *Daphnia pulex*. Further details concerning partial impregnation, as well as theoretical support of the facts treated of above, we reserve for a future occasion.

May 21, 1888.

[1] Translated from a paper by A. Weismann and C. Ischikawa (*Berichte der Naturforschenden Gesellschaft zu Freiburg i/B.*, Bd. iv., Heft 1, p. 51).—W. N. P.

Addendum to the above Note on Partial Impregnation,[1] *by Weismann and Ischikawa.*

SINCE giving a short abstract of the observations which led us to the conclusion of the existence of partial impregnation, we have continued our researches, and have come to the conclusion that, in spite of the entire accuracy of our facts, we were mistaken as to the explanation of the phenomena described. The fusion with one of the eight first segmental cells does indeed take place regularly, but the uniting cell is not the sperm-cell. The first segmentation nucleus is here, as in all sexual eggs, formed by the fusion of the nucleus of the ovum with the sperm-nucleus, and the fusion of the two cells observed by us at a later stage *is something additional to the ordinary impregnation.* That this is the case is quite certain: we found the sperm-nucleus and its subsequent fusion with the egg-cell to occur in *the same* ova in which we could prove the presence of that cell which we at first took to be the sperm-cell.

We can hardly be blamed for this error if it be borne in mind that we found this cell, *without exception*, in every egg which had just passed into the brood-chamber; that the vitelline membrane was formed directly afterwards; and that, on the other hand, a fusion of this cell with one of the first eight segmental cells lying at the vegetative pole of the egg could be seen in *all* ova which we possessed of this stage, viz. in five species—two species of *Moina*, two of *Daphnia*, and one of *Polyphemus*. The fact that the form and size of the supposed sperm-cell differ from those of the sperm-cells in the testis of the corresponding species was indeed an objection to our explanation: it has, in fact, almost the same size and shape in all species. But the sperm-cells become altered as soon as they pass into the egg, and it was shown some time ago by Fol and Hertwig, and more recently by Boveri, that the sperm-nucleus grows considerably when within the ovum. Moreover, in one of the species examined (*Polyphemus*), as well as in *Bythotrephes*, the sperm-cell is extraordinarily large, and in both these species we followed the entrance of the enormous amœboid sperm-cell into the ovum by means of sections, step by step, and were able to convince ourselves of its essential correspondence with the supposed sperm-cell in the eggs of other species. What else could this cell within the ovum be, if it were not the sperm-cell? It was never wanting, and on the other hand there was *always one only*, so that any idea of its being a parasitic organism was out of the question. Moreover, the two polar cells were always present, so that it could not be mistaken for one of these. And up to the present time no one had ever seen any other cell but the sperm-cell within the ovum.

We should hardly, indeed, have discovered our error so soon, if we had not remembered that one of us had found some years ago that unimpregnated sexual eggs of Daphnidæ soon become disintegrated,[2] and had we not asked ourselves how the embryonic development advanced in such unimpregnated eggs before disintegration begins. For, as we believed that the sperm-cell was only ready for conjugation in impregnated eggs after they had segmented into eight parts, it was to be expected that segmentation would take place up to this stage in unimpregnated ova, and that then only would the disintegration begin. Had we found

[1] Translated from the proof of a paper to appear in the *Berichte der Naturforsch. Gesellschaft zu Freiburg i/B.*, Bd. iv. Heft 2, 1888.—W. N. P.

[2] See Weismann, "Beiträge zur Naturgeschichte der Daphnoiden," iv.; "Ueber den Einfluss der Begattung auf die Erzeugung von Wintereiern," *Zeitsch f. Wiss. Zool.*, Bd. xxviii. p. 198 *et seq.*

it otherwise, and did the first stages of division not occur in unfertilized eggs, we should have supposed that the sperm-cell present in the ovum, although in a resting-stage, had some invisible influence over it.

It was possible, however, to arrive at a decision on this point; for, although most Daphnidæ do not lay their eggs if copulation does not take place at the time the eggs ripen, in one species (*Moina paradoxa*), the extrusion of the ova occurs independently of copulation. We therefore isolated females of this species which contained ripe eggs in the ovary, and examined them when they had passed the eggs into the brood-chamber. How great was our astonishment to find that these ova, killed shortly afterwards, were already beginning to disintegrate, and a cell corresponding to that which we had taken for the sperm-cell was present in *each* of them! At first we considered the possibility of copulation having taken place before the females were isolated, and of the retention of the sperm-cell, which had become inactive, in the brood chamber. But sections which we made through nearly ripe ovarian eggs showed us that the supposed sperm-cell was already present in them. It was thus proved that this cell which unites with one of the eight first segmental cells (we will for the present call it the "conjugating-cell," *Copulationszelle*) cannot be an ordinary sperm-cell; and, moreover, that, besides it, an active sperm-cell from the male, which had previously escaped our notice, passes into the egg in consequence of copulation. In fact, this true spermatic element was found after renewed examination of old and new series of sections as an exceedingly small nucleus in the yolk-mass. It is difficult to recognize, but nevertheless may plainly be traced passing into the yolk, and finally uniting in the ordinary manner with the nucleus of the ovum.

Thus the impregnation of these ova is not exceptional, inasmuch as a normal fusion of the male and female nuclei takes place. But, besides this normal conjugation of sperm-nucleus and egg nucleus, another fusion of cell-bodies and cell-nuclei occurs between the enigmatical "conjugating-cell," present already in ovarian eggs, and one of the eight first segmental cells lying at the vegetative pole of the ovum.

It will be impossible to conjecture as to the meaning of this process until we know definitely how the "conjugating-cell" arises: at present we are not able to state anything about it with certainty.

We intend to continue our observations, and hope before very long to have more to say on this subject.

Freiburg i/B., July 12, 1888.

HOW TO INCREASE THE PRODUCE OF THE SOIL.[1]

IN this pamphlet Prof. Wagner distinctly asserts the power of leguminous cultivated plants, such as peas, beans, vetches, lupines, and clovers, to use the free nitrogen of the air for purposes of nutrition. As this conclusion is distinctly at issue with the opinions of the Rothamsted school, it revives a question of deep interest, the answer to which has varied with our knowledge from time to time. In the earlier days of agricultural chemistry the "mineral theory" of plant nutrition was in the ascendant. According to this theory the mineral, earthy, or ash constituents were taken from the soil, while the gaseous, combustible or organic portions of the plant were derived from the air. As knowledge progressed, this somewhat bold and sweeping generalization required to be modified, and the most usually received view (in this country, at least) for some time past has been that of the absorption of mineral matter and nitrates from the soil, and of carbonaceous matter from the air, and to a limited extent from the soil in the form of carbonic acid gas in solution. It has been urged that proof is entirely wanting of the alleged power of plants to take free or combined nitrogen from the atmosphere, while the intense effect of nitric nitrogen upon growing crops, when added to the soil, has amply proved that the soil is a source of nitrogen, and, according to received views, the chief or only source of nitrogen to growing crops. The results obtained by Sir John Lawes, Dr. Gilbert, and Mr. Warrington at Rothamsted, upon the cultivation of red and Bokhara clover, have been considered as proving that the source of nitrogen in these plants was not the atmosphere, but the soil and the subsoil, the plants having been found to send down their roots some fifty-four inches in depth into sections of the soil which, although out of reach of most cultivated plants, were able to yield sufficient nitrogen for the uses of these nitrogen-loving plants. Collectors of nitrogen these plants are allowed to be by all, but at Rothamsted the collection is considered to be carried on in the deeper layers of the soil, and not to extend above ground. Prof. Paul Wagner declares that cultivated plants may be properly divided into nitrogen *collectors* and nitrogen *consumers*, or as we might put it, into nitrogen savers and nitrogen wasters. In the first class are arranged the various members of the Leguminosæ already named. At a certain stage of their development these plants acquire the power of taking all their nitrogen from the air. They thus become a means of securing fertilizing matter from a free source, and are therefore profitable. In the second class are placed the cereals, grains, turnips, flax, &c., all of which are able to take next to nothing from the store of nitrogen in the air, but which waste the nitrogen of the soil, and must take from it, in the form of nitrates, all the nitrogen they contain. In the pamphlet under notice no proof is adduced for these views, but reference is made to the detailed investigations carried out by the author, Hellriegle, and E. von Wolff. These views must be considered as reactionary and startling, and as diametrically opposed to the current of opinion in this country for some years past.

It is not to be wondered at that Prof. Wagner should give considerable prominence to a feature in agricultural practice which has almost entirely disappeared—green crop manuring. If clovers, lupines, and vetches, extract their nitrogen from the supernatant aërial ocean, and are able to supply upwards of 180 pounds of atmospheric nitrogen per acre per annum continuously for a period of three years, no easier system could be devised for obtaining the necessary nitrogen for fertilizing purposes. All that is required is to secure the full development of the nitrogen collector by supplying it with sufficient water, sufficient phosphoric acid, potash and lime, so that it may exert its powers upon the constantly passing stream of air—it then provides nitrogen for itself. What is this but a re-statement of the old mineral theory applied especially to the Leguminosæ?

Prof. Wagner's views upon the absorption of atmospheric nitrogen and his consequent recommendation of green crop manuring, are the two principal features of this little work. In some places the German fault of verbiage is only too evident—whole paragraphs being devoted to what is perfectly self evident. Still, various practical suggestions of great value are made. The remarks upon the proper method of applying nitrate of soda are particularly worthy of attention. The effect of this active manure in developing stem and leaf rather than flower and fruit is acknowledged, but only as a consequence of the period of the plant's growth when it is applied.

Nitrate of soda enables the plant to seize upon the stores of phosphoric acid, potash and lime in the soil, and the effect is rapid growth. This effect is however short lived, as the nitrate is freely movable in the soil, and readily finds its way to lower sections when it is no longer available. The case is therefore as follows:—Nitrate of soda applied in February, March, or April, is employed in the development of leaf and stem, and by the time the period has arrived for grain formation it is spent. If the same dressing had been applied later in the history of the crop, and at the time when the embryo grain was being formed, the same stimulus would have been given towards grain formation, which under ordinary circumstances takes the form of leaf and stem development. The practical recommendation based upon the consideration is to apply one-sixth part of the application in autumn, two-sixths in March, and the remaining three-sixths in May. The plant is to be fed during its whole life, and not only at the period when it is forming leaves and stem, but especially at the important period when it is forming fruit. The remarks upon the ripening effects of superphosphate upon root crops are also well worthy of attention. Excessive quantities of superphosphate hasten too rapidly the processes of maturation, and tell against prolongation of growth into the late autumn, and this, it is submitted, accounts for the occasionally smaller results obtained by the use of phosphates in large quantities as compared with those produced by more moderate dressings.

Prof. Wagner comes to the conclusion, which we quite agree in, that nitrogen, phosphoric acid, and potash are the principal elements of fertility that require to be added to soils. The remaining essential substances, although equally important to the well-being of the plant, are usually present in ample quanti-

[1] "The Increase in the Produce of the Soil through the Rational Use of Nitrogenous Manure." By Prof. Paul Wagner, of Darmstadt. Translated by George G. Henderson. (London: Whittaker and Co., 1888.)

ties in cultivated soils. We might be disposed to eliminate the potash as also usually sufficiently prevalent. The fact that straw is almost invariably returned to arable land is in itself a safeguard against the exhaustion of potash; and the considerable percentage found in most soils, especially those of argillaceous character, points to the same conclusion. The farmer has then chiefly to consider the supply of phosphates and of nitrates, and, with regard to these two, Prof. Wagner thinks that the former ought to be in excess of what is required, and that the farmer should equally devote his attention to the proper supply and application of nitrates to the soil. The recommendation that phosphates should be in excess is based on the observation that growth is seldom regular. It depends on climatic conditions, and sometimes is arrested by drought or low temperature for two or three weeks, while in well cultivated and well fertilized ground vegetation makes extraordinary progress in three or four days. The supply of phosphates ought therefore to be in excess of what may be required under ordinary conditions of growth, and should be abundant enough to supply the plant under the most rapid conditions of growth. The conclusion is that phosphates may be applied liberally and without hesitation or limit, *i.e.*, without scientific accuracy. The case of nitrates is different, as they are so easily available and so freely mobile in the soil, that the plant has no difficulty in appropriating them. The nitrates probably find their way into the plant before they are required, and are stored up and elaborated gradually as the plant takes up further supplies of mineral nutriment. The rapidity with which they disappear and their extraordinary effect mark the nitrates out as the chief object of study in manuring land.

JOHN WRIGHTSON.

THE BURIAL CUSTOMS OF THE AINOS.

MR. BACHELOR, to whose investigations on the subject of the Ainos of Yezo we have frequently referred, writes, in a recent issue of the *Japan Weekly Mail*, on the burial customs of this race. He says that as soon as a person dies, a blazing fire is made, the corpse is dressed in its best garments, which are neatly laced up, and is laid lengthways on the right-hand side of the fireplace. The relatives and friends of the deceased sit around the remaining parts of the fireplace, and usually they are so numerous as to fill the hut. In all cases many sacred symbols (*inao*) are made, and placed around the hut and the dead body. Mr. Bachelor has seen the corpse of a woman laid out. She was well dressed, and had her utensils and paraphernalia about her (the rings and beads being, in this instance, laid upon her bosom), and was shod with pieces of white calico which Mrs. Bachelor had, a few days previously, given to the husband of the deceased to bind up his wounded foot. Any white material seems to be especially welcome to the Ainos for wrapping up the bodies of their dead. When the body has been properly dressed, and when the necessary eating-vessels or hunting materials are placed in position, a cake made of millet, or a cup of boiled rice and some wine, are placed by its side, and the spirit of the departed is supposed to eat up the essence of these things. Then the goddess of fire is implored to take charge of the spirit and lead it safely to the Creator of the world and the possessor of heaven, and she receives various messages to the Deity setting forth the praises of the dead and extolling his many virtues. Millet cakes and wine are then handed round to every member of the assembled company, and each of them offers two or three drops of the wine to the spirit of the dead, then drinks a little, and pours what remains before the fire as an offering to the fire-goddess, to whom they have not ceased to pray; then part of the millet cake is eaten, and the remainder buried in the ashes on the hearth, each person burying a little piece. After the burial these scraps are collected and carried out of the hut and placed before the east window, which is regarded as the sacred place. The corpse is then carefully rolled up in a mat, neatly tied up, attached to a pole, and carried to the grave by two men. The mourners follow after the corpse, in single file, each carrying something to be buried in the grave, the men leading and the women following them. The grave is from $2\frac{1}{2}$ to $3\frac{1}{2}$ feet deep, and round the inside of it stakes are driven, and over them and at the bottom of the grave mats are placed. Then the body is laid in the grave, with numerous little knick-knacks—cups, rings, beads, a saucepan and some clothing being buried with the woman, a bow and quiver, an eating and a drinking cup, tobacco, a pipe, a knife with the men, and play-things with the children. These things are always broken before being put into the grave, and it is noticeable that they are not usually the best the deceased had during life. Everything is then closely covered with mats; pieces of wood are placed so as to form a kind of roof, and on this the earth is piled. A pole is generally stuck at the foot of the grave to mark the spot. No prayers are offered up during burial. The mourners then return to the hut, where the men pray, make *inao*, *i.e.* sacred symbols, eat, drink, and get drunk. The dead body is never allowed to remain in the house longer than one day; and, once the funeral is over, the name of the departed is never mentioned.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE following is the list of Scholarships, prizes, and Associateships awarded in July 1888, at the Normal School of Science and Royal School of Mines, South Kensington, for the session 1887-88:—

First Year's Scholarships: Samuel H. Studley, Sydney Wood, William S. Jarratt, and George N. Huntly. Second Year's Scholarships: Savannah J. Speak and William Tate.

Edward Forbes Medal and Prize of Books for Biology: Arthur M. Davies. Murchison Prize of Books for Geology: William Tate and Samuel Truscott. The Murchison Medal was not awarded. Tyndall Prize of Books for Physics: William Watson. De la Beche Medal for Mining: Edmund L. Hope. Bessemer Medal and Prize of Books for Metallurgy: Harry C. Jenkins. Frank Hatton Prize of Books for Chemistry: James W. Rodger.

Prizes of Books given by the Science and Art Department:—Mechanics, James Whitaker; Astronomical Physics, William S. Jarratt and William Watson; Practical Chemistry, James W. Rodger and James Young; Mining, John M. Beckwith. The prize for Principles of Agriculture and Agricultural Chemistry was not awarded.

Associateships (Normal School of Science):—Mechanics, 1st Class: James Whitaker and William Kelsall. Physics, 1st Class: Harry E. Hadley and Philip L. Gray; 2nd Class: Herbert Anderson and Philip L. Coultas. Chemistry, 1st Class: James W. Rodger, James Young, Barker North, and Harold E. Hey; 2nd Class: William MacDonald, George Grace, Francis J. Hardy, George C. McMurtry, and Henry Sowerbulls. Biology, 1st Class: Arthur M. Davies. Geology, 1st Class: Thomas H. Holland.

Associateships (Royal School of Mines):—Metallurgy, 1st Class: Harry C. Jenkins, Thomas Clarkson, and William McNiell; 2nd Class: Alfred Howard. Mining, 1st Class: Edmund L. Hope, John M. Beckwith, James A. Chalmers, William F. Thomas, Sydney Allingham, Charles G. Thompson, John Leechman, Frederick H. P. Creswell, Ernest Lichtenburg; 2nd Class: Ferdinand F. L. Dielyrch, Henry L. Lewis, Henry B. Budgett, William F. Hamley, and Harold Macandrew.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, April 26.—"On the Coagulation of the Blood." Preliminary Communication. By W. D. Halliburton, M.D., B.Sc., Assistant Professor of Physiology, University College, London. Communicated by Prof. E. A. Schäfer, F.R.S. (From the Physiological Laboratory, University College, London.)

The present research was directed to determining the nature of the ferment that produces the change of fibrinogen into fibrin.

Some preliminary experiments showed that the following proteids were present in lymph cells (obtained from lymphatic glands).

(1) A mucin-like proteid similar to that described by Miescher in pus which swells up into a jelly-like substance when mixed with solutions of sodium chloride or magnesium sulphate. This is a nucleo-albumin.

(2) Two globulins.

(3) An albumin.

The Globulins.—There is a small quantity of a globulin which enters into the condition of a heat coagulum at about 50° C. The most abundant globulin is, however, one which resembles serum globulin in its heat coagulation temperature (75° C.), and in the way in which it is precipitated by saturation with salts, or by dialyzing out the salts from its solutions.

The term serum globulin is hardly applicable to a proteid existing in lymph cells; hence it is necessary to multiply terms, and to designate this globulin by a new name, viz. *cell globulin*. It has, moreover, certain characteristic properties which will be fully dealt with later on.

The Albumin resembles serum albumin in its properties. It coagulates at 73° C. It is present in very small quantities. It may be provisionally termed *cell albumin*.

Having thus recognized the various proteids that occur in the cells of lymphatic glands, my next endeavour was to ascertain what action, if any, these exerted on the coagulation of the blood. My experiments in this direction have been mostly performed with salted plasma. The blood is received into an approximately equal volume of saturated sodium sulphate solution. By this means coagulation is prevented, and the corpuscles settle. On subsequently removing the supernatant salted plasma, and diluting it with four or five times its bulk of water, coagulation occurs after the lapse usually of several hours; but if, instead of water, a solution of fibrin ferment be used, coagulation occurs in a few minutes.

I first tried to prepare fibrin ferment from the lymphatic glands; these were freed from blood, chopped small, and placed under absolute alcohol for some months; they were then dried over sulphuric acid, powdered, and the dry powder extracted with water. The water was found to contain the fibrin ferment. It hastened very considerably the coagulation of salted plasma. This activity was destroyed at a temperature between 74° C. and 80° C. The watery extract gave, moreover, the xanthoproteic reaction; it contained also some sodium chloride and phosphates which it had dissolved out of the dried glands.

A watery or saline extract of fresh glands also had very considerable clotting powers; that is to say, the addition of a few drops of such an extract caused diluted salted plasma to clot in a few minutes, which otherwise did not clot until after the lapse of 12–24 hours. The activity of this extract was not altered by heating to 70°; it was therefore independent of the nucleo-albumin which is disintegrated at about 50° C., or of the globulin which coagulates at that temperature. Its activity was destroyed, however, if heated above 75° C. These facts show that the extracts of both dried and fresh glands contain a substance which has the same properties as fibrin ferment, and which, moreover, is rendered inactive at the temperature at which fibrin ferment, as ordinarily prepared from serum, loses its activity.

The next question which I investigated was whether the ferment action was dependent upon, or independent of, the presence of the proteids of the cells. An extract of the cells was made with sodium sulphate solution, and saturated with ammonium sulphate; the precipitate of the proteids so produced was filtered off; the proteid-free filtrate dialyzed till free from excess of salt, and it was then found to have no power of hastening coagulation. The precipitate which contained all the proteids was washed by saturated solution of ammonium sulphate, and redissolved by adding distilled water; this solution hastened the coagulation of salted plasma very considerably. This experiment showed either that the ferment was identical with or precipitated with the proteids in the extract. It was, moreover, destroyed at a temperature at which these proteids were coagulated, viz. about 75° C.; there are, however, in the solution two proteids which are coagulated at about this temperature, viz. the cell globulin and the cell albumin. The globulin and the albumin were then separated from one another, and it was found that the globulin and not the albumin had the properties of fibrin ferment.

After I had performed the experiments just related, the question naturally arose, Is this cell globulin the same thing as what has been termed fibrin ferment when prepared from serum? From the experiments which were performed in order to elucidate this question the following conclusions were drawn:—

(1) Lymph cells yield as one of their disintegration products a globulin which may be called cell globulin. This has the properties that have hitherto been ascribed to fibrin ferment.

(2) Fibrin ferment as extracted from the dried alcoholic precipitate of blood serum is found on concentration to be a globulin with the properties of cell globulin.

(3) The fibrin ferment as extracted by saline solutions from "washed blood clot" is a globulin which is also identical with cell globulin.

(4) Serum globulin as prepared from hydrocele fluid has no fibrinoplastic properties. It may perhaps be better termed plasma globulin.

(5) Serum globulin as prepared from serum has marked fibrinoplastic properties. This is because it consists of plasma globulin, and cell globulin derived from the disintegration of white blood corpuscles, which are in origin lymph cells.

(6) The cause of coagulation of the blood is primarily the disintegration of the white blood corpuscles; they liberate cell globulin, which acts as a ferment converting fibrinogen into fibrin. It does not apparently become a constituent part of the fibrin formed.

This confirmation and amplification of Hammarsten's views concerning the cause of the coagulation of the blood is in direct opposition to the theories of Wooldridge, which may be stated as follows:—The coagulation of the blood is a phenomenon essentially similar to crystallization; in the plasma there are three constituents concerned in coagulation, A, B, and C fibrinogen. A and B fibrinogen are compounds of lecithin and proteid, and fibrin results from the transference of the lecithin from A fibrinogen to B fibrinogen. C fibrinogen is what has hitherto been called fibrinogen; A fibrinogen is a substance which may be precipitated by cooling "peptone plasma," and on the removal of this substance coagulation occurs with great difficulty. The precipitate produced by cold consists of rounded bodies resembling the blood-plates in appearance. He further found that other compounds of lecithin and proteid, to which he has extended the name of fibrinogen, exist in the thymus and other organs, in the fluid of lymph glands, and in the stromata of red corpuscles; these substances may be extracted from the organs by water, and precipitated from the aqueous extract by acetic acid, and on redissolving this in a saline solution, and injecting it into the circulation of a living animal, intravascular clotting occurs which results in the death of the animal. This form of fibrinogen (?) that acts thus he looks upon as the precursor of A fibrinogen. From these points of view the fibrin ferment and the white corpuscles are looked upon as of secondary import in causing coagulation, though it is admitted that fibrin ferment converts C fibrinogen into fibrin.

The Influence of Lecithin in the Coagulation of the Blood.—Lecithin hastens the coagulation of blood-plasma, which has been prevented from clotting by the injection into the circulation of a certain quantity of commercial peptone; but peptone plasma, as I shall show more fully in the next section, differs so much from normal plasma, that it is impossible to draw correct conclusions from experiments performed with it, unless they be supported by confirmatory evidence on solutions of fibrinogen and pure plasma, such as one obtains from a vein, or from the pericardial sac, and lecithin does not cause coagulation in such cases.

The supposition that "fibrinogen A" acts by giving up its lecithin to "fibrinogen B" to form fibrin, seems, therefore, to be a pure assumption, and is unsupported by analytical evidence. Cell globulin contains no phosphorus, and can therefore contain no lecithin.

The Precipitate produced by cooling Peptone Plasma.—The chief point I wish to urge is that this precipitate is obtained on cooling peptone plasma only, and from no other form of plasma. I have repeatedly attempted to obtain such a precipitate by cooling to 0° C. pure plasma from the veins of the horse, salted plasma, hydrocele fluid, and pericardial fluid, but in all cases with a negative result. It therefore occurs in peptone plasma alone; and that it is due to the peptone is supported by the fact that if one takes an aqueous solution of "Witte's peptone" and cools it to 0° C., a precipitate is formed consisting of rounded granules very similar to blood-tablets. This precipitate moreover consists of hetero-albumose. (Witte's peptone contains a large admixture of albumose.) That peptone blood does differ in one other important particular from normal blood, viz. in the heat coagulation temperatures of its proteids, was shown by Wooldridge himself. It is on these grounds, then, that I hold we cannot regard peptone plasma as being at all comparable to normal plasma.

Intravascular Coagulation.—No doubt the crude and impure substance introduced into the veins produces intravascular clotting; but I must protest against the extension of the name fibrinogen to such substances. It seems to me it would be just as correct to call a piece of iron wire introduced into the sac of an aneurysm to produce coagulation there, a fibrinogen.

With regard, however, to these tissue-fibrinogens of Wooldridge, I think we may venture to offer a suggestion as to their real nature, or, at any rate, as to the nature of one of their constituents. From the last paper published by Wooldridge, we find that they are imperfectly soluble in water, readily precipitated by acids, and soluble in excess of those reagents; that they yield on gastric digestion a substance which is insoluble and which is rich in phosphorus. From these details of their properties, I think we may draw the conclusion, not that they contain lecithin, as Wooldridge affirms, but that they belong to the group of proteids described in the former part of this paper under Hammarsten's name of nucleo-albumin. Nucleo-albumins yield when poured into water a stringy precipitate resembling mucin, and in a former paper Wooldridge speaks of the precipitate of his tissue fibrinogen (precipitated by acetic acid) as being a bulky one. If my conjecture is correct, it would be exceedingly likely that when a saline solution of such a substance was injected into the circulation, it would form strings of a slimy mucinoid description in the vessels, and that these would form the starting-point for the thrombosis or intravascular coagulation that ensues.

May 3.—"On the Induction of Electric Currents in Conducting Shells of Small Thickness." By S. H. Burbury.

(1) *Definition and Explanation of the Notation employed.*—A *current-sheet* in any field of electric currents is a surface to which the stream-lines are everywhere tangential. A *current-shell* is the space between two current-sheets very near each other. The *superficial current* in a current-shell is the quantity of electricity which in unit time crosses unit length of a line drawn on either sheet perpendicular to the current. If U, V, W be the components of superficial current, there always exists a function, ϕ, called *the current function*, such that—

$$U = n\frac{d\phi}{dy} - m\frac{d\phi}{dz}, \text{ \&c.},$$

l, m, n being the direction cosines of the normal. This function completely determines the superficial currents.

The corresponding expressions for the component currents per unit of area are—

$$u = \frac{dS}{dz}\frac{d\Phi}{dy} - \frac{dS}{dy}\frac{d\Phi}{dz}, \text{ \&c.},$$

where S and Φ are any two functions of x, y, and z.

The components of *vector potential* due to a current-sheet are—

$$F = \iint \frac{U}{r}dS = \iint \frac{1}{r}\left(n\frac{d\phi}{dy} - m\frac{d\phi}{dz}\right)dS.$$

And if the sheet be closed, this may be put in the form—

$$F = \iint \phi\left(m\frac{d}{dz} - n\frac{d}{dy}\right)\frac{1}{r}\,dS\text{ ; } G = \text{\&c.}$$

So that F, G, and H are linear functions of the ϕ's with coefficient functions of the co-ordinates.

If the current-sheet be spherical, the vector potential is tangential to any concentric spherical surface.

The *electro-kinetic energy* of a system of current-sheets is—

$$2T = \iiint (FU + GV + HW)dS$$

over all the sheets; that is—

$$\iiint \left(\phi\left(m\frac{dF}{dz} - n\frac{dF}{dy}\right)\text{\&c.}\right)dS,$$

if the surfaces be closed; and if Ω be the magnetic potential, this reduces to—

$$-\iint \phi\frac{d\Omega}{d\nu}dS,$$

$\frac{d\Omega}{d\nu}$ denoting the space variation of Ω per unit length of the normal measured outwards. Also, $\frac{d\Omega}{d\nu}$ is shown not to be discontinuous in passing through a sheet of superficial currents. T is expressible as a quadratic function of the ϕ's with coefficients functions of the co-ordinates.

(2) *Comparison with Magnetic Shells.*—The components of vector potential due to a magnetic shell placed on a closed surface, S, with variable strength, ϕ (reckoned as positive when the positive face is outwards), are—

$$F = \iint \phi\left(m\frac{d}{dz} - n\frac{d}{dy}\right)\frac{1}{r}\,dS.$$

They are, then, the same as those due to a system of currents on S determined by ϕ as current function. Hence the magnetic induction due to the magnetic shell is the same as that due to the corresponding system of currents at any point in free space.

(3) If Ω_0 denote the magnetic potential due to any magnetic system outside of S, it is possible to determine ϕ so that a shell of strength ϕ on S has, at all points on or within S, potential equal and opposite to Ω_0. General determination of ϕ to satisfy this condition. The solution is unique.

(4) Therefore, also, there exists a system of currents on S, having ϕ for current function, such that the magnetic force due to it is equal and opposite to that due to the external system at all points on or within S. This system is called the *magnetic screen* on S to the external system. Example of a sphere.

(5) *General Solution of the Problem of Induction, Resistance not being yet taken into account.*—If S_0, Ω_0, ϕ_0, &c., relate to a magnetic system outside of S, Ω and ϕ to S and superficial currents upon it, the whole electro-kinetic energy is—

$$\iint \phi_0\left(\frac{d\Omega_0}{d\nu} + \frac{d\Omega}{d\nu}\right)dS_0 + \iint \phi\left(\frac{d\Omega_0}{d\nu} + \frac{d\Omega}{d\nu}\right)dS.$$

In this form, T has as many variables—namely, the values of ϕ—as it has degrees of freedom.

If, therefore, the external system be continuously varied, the induced current on S will be given by

$$\frac{d}{dt}\frac{dT}{d\phi} = 0 \text{ on S},$$

that is,

$$\frac{d}{dt}\left(\frac{d\Omega_0}{d\nu} + \frac{d\Omega}{d\nu}\right) = 0 \text{ on S},$$

that is,

$$\frac{d}{d\nu}\left(\frac{d\Omega_0}{dt} + \frac{d\Omega}{dt}\right) = 0 \text{ on S}.$$

And since $\nabla^2\frac{d\Omega_0}{dt} = 0$, and $\nabla^2\frac{d\Omega}{dt} = 0$ at all points within S it follows that $\frac{d\Omega_0}{dt} + \frac{d\Omega}{dt} = 0$ at all points within S.

That is, the induced currents, on their creation, are the *magnetic screen* to the time variation of the external field. This gives the law of formation of the currents, however rapidly they may decay by resistance.

(6) *Of a Solid Conductor.*—If S be a hollow shell, there will, as the direct result of induction, be zero magnetic force at all points within it. Therefore, if it be filled with conducting matter so as to form a solid conductor, none but superficial currents will, as the direct consequence of the variation of the external field, be induced in it. But as the superficial currents decay by resistance, their variation induces currents in the inner strata of the solid, so that in time, and no doubt generally in a very short time, the solid becomes pervaded by currents. The currents penetrate the solid, and the initial *rate of penetration* can be calculated under certain conditions (see *post*, 15).

(7) *Of the Associated Function.*—If F, G, H be the components of any vector which satisfy

$$\frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz} = 0$$

at all points within a closed surface, S, there exists a function, χ, called the *associated function*, such that—

$$\frac{d\chi}{d\nu}, = lF + mG + nH \text{ on S},$$

$$\nabla^2\chi = 0 \text{ within S}.$$

The components F, G, H of vector potential of a system of closed currents outside of S have an associated function, χ, on S. In like manner $-\frac{dF}{dt}$, $-\frac{dG}{dt}$, and $-\frac{dH}{dt}$ have an associated function, which shall be denoted by ψ.

(8) If $\frac{dF}{dt}$, $\frac{dG}{dt}$, and $\frac{dH}{dt}$ relate to an external system and its magnetic screen on S, we have

$$\frac{d}{dt}\frac{dF}{dy} = \frac{d}{dt}\frac{dG}{dx}, \text{ \&c., within S,}$$

whence it follows that

$$-\frac{dF}{dt} = \frac{d\psi}{dx}, \; -\frac{dG}{dt} = \frac{d\psi}{dy}, \text{ \&c.}$$

If, therefore,

$$-\frac{dF}{dt}, \; -\frac{dG}{dt}, \; -\frac{dH}{dt}$$

are the components of an *electromotive force* within S, there will form on S a distribution of statical electricity having potential ψ, and forming a complete *electric screen* to the external system.

(9) *Of Self inductive Systems of Currents on a Surface.*—If any system of currents in a conducting shell be left to decay by resistance, uninfluenced by any external induction, it may be the case that they decay proportionally; so that, if U_0, V_0, W_0 denote the initial values of the component currents, their values at time t are $U = U_0\epsilon^{-\lambda t}$, $V = V_0\epsilon^{-\lambda t}$, $W = W_0\epsilon^{-\lambda t}$, and $\frac{dU}{dt} = -\lambda U$, &c., where λ is a constant proportional to the specific resistance, and inversely proportional to the absolute thickness. If this be the case, the system is defined to be *self-inductive.*

(10) By Ohm's law we have, whether the system be self-inductive or not,—

$$\sigma u = -\frac{dF}{dt} - \frac{d\psi}{dx}, \text{ \&c.,}$$

where u is the component current per unit of area, and σ the specific resistance.

If h be the thickness of the shell, $\sigma u = \frac{\sigma}{h}U$, and the equations may be written—

$$\frac{\sigma}{h} = \frac{-\frac{dF}{dt} - \frac{d\psi}{dx}}{U} = \frac{-\frac{dG}{dt} - \frac{d\psi}{dy}}{V}, \text{ \&c.}$$

If the system be self-inductive $-\frac{dF}{dt} = \lambda F$, &c., and $-\frac{d\psi}{dx} = \lambda\chi$, where χ is the associated function to F, G, and H, and ψ to $-\frac{dF}{dt}$, $-\frac{dG}{dt}$, and $-\frac{dH}{dt}$. Therefore—

$$\frac{\sigma}{h} = \lambda\frac{F + \frac{d\chi}{dz}}{U} = \lambda\frac{G + \frac{d\chi}{dy}}{V} = \lambda\frac{H + \frac{d\chi}{dz}}{W}.$$

(11) Now if we assume as current function on S any arbitrary function, ϕ, we thereby determine U, V, W, and therefore also F, G, H, and χ, at all points on S. It will not be generally true that—

$$\frac{F + \frac{d\chi}{dx}}{U} = \frac{G + \frac{d\chi}{dy}}{U} = \frac{H + \frac{d\chi}{dz}}{W}.$$

These equations constitute a condition which the current function ϕ must satisfy in order that the system may be capable of being made self-inductive. Their geometrical interpretation is that the tangential component of vector potential of the currents in the sheet coincide with the current at every point.

If ϕ be chosen to satisfy that condition, then by the equation—

$$\frac{\sigma}{h} = \lambda\frac{F + \frac{d\chi}{dx}}{U} = \lambda Q \text{ (suppose)}$$

we determine h, the thickness of the shell at every point, necessary to make the shell self-inductive, *i.e.* $h = \frac{\sigma}{\lambda Q}$.

(12) *Examples of Self-inductive Systems.*—1. S a sphere, and ϕ any spherical surface harmonic of one order. Here h is a constant.

2. S a surface of revolution about the axis of z, and ϕ a function of z only.

3. Any surface, with ϕ a function of z only, if χ is independent of z. Example: an ellipsoid whose axes are the axes of co-ordinates, and $\phi = Az$. It is found in this case that $\psi \propto xy$, and therefore the necessary condition for a self-inductive system is satisfied; and also, to make it self-inductive, h varies as the perpendicular from the centre on the tangent plane at the point.

(13) *Co-existence of Self-inductive Systems.*—If any number of self-inductive systems be created in the same shell, each decays according to its own law, unaffected by the others. If all have the same value of λ, then, as the effect of resistance apart from induction, we have

$$\frac{d\Omega}{dt} + \lambda\Omega = 0,$$

where Ω is the magnetic potential of the whole system.

(14) *General Property of Self-inductive Systems.*—If an external system so vary as that the system of currents in the shell S, induced at any instant, shall always be self-inductive, and with the same value of λ, we have, to determine the currents in the shell at any instant, the equation—

$$\frac{d\Omega_0}{dt} + \frac{d\Omega}{dt} + \lambda\Omega = 0,$$

from which Ω can be found, if $\frac{d\Omega_0}{dt}$ is given.

Example 1.—Let $\frac{d\Omega_0}{dt} = C$, a constant. In this case we find $\Omega = \frac{C}{\lambda}\left(1 - \epsilon^{-\lambda t}\right)$. If C be very great, and t very small, this approximates to the ideal case of an impulsive force, and Ω becomes equal to Ct, and is independent of the resistance. If, on the other hand, λt be very great, we have $\Omega = \frac{C}{\lambda}$, and Ω varies inversely as the resistance.

Example 2.—Let $\Omega_0 = A \cos kt$, where k is constant, and A independent of the time, but a function of position. This leads to the result—

$$\Omega = -A \sin \alpha \sin \overline{kt - \alpha},$$

$$\Omega_0 + \Omega = A \cos \alpha \cos \overline{kt - \alpha},$$

at all internal points. Here, α is *the retardation of phase*, and is equal to $\cot^{-1}\frac{\sigma}{Qhk}$.

For instance, if S is a sphere of radius a, and $\phi = A \cos kt$, $Q = \frac{2n + 1}{4\pi a}$, and the result obtained agrees with that given by Prof. Larmor in *Phil. Mag.*, January 1884.

(15) If the shell be infinitely thin—

$$\alpha = \sin\alpha = \frac{Qhk}{\sigma},$$

the same phase is reached in the inner field at a time later by $\frac{\alpha}{k}$, that is, $\frac{Qh}{\sigma}$, than in the outer field. The ratio which in the limit h bears to this difference of time is $\frac{\sigma}{Q}$, and is, in case of a solid conductor, the initial *velocity* with which the currents penetrate the solid.

(16) If S be any homogeneous function of positive degree in x, y, and z, the space within S = 0 may be conceived as divided into a number of concentric similar and similarly situated shells, each between two surfaces of the type S = c and S = $c + dc$. Let ϕ be a function, which, as current function, gives a self-inductive system of currents in each shell of the series, if made a conductor. Let an outer shell of the series be described on S, and an inner shell of the series on S′. Let currents of the type ϕ be generated in the shell S. Let u, v, w be the functions—

$$u = \frac{dS}{dz}\frac{d\phi}{dy} - \frac{dS}{dy}\frac{d\phi}{dz}, \text{ \&c.}$$

Then u, v, w may be the components per unit of area of a system of currents in the shell S. And since this system is self-inductive,—

$$\sigma u = \lambda\left(F + \frac{d\chi}{dx}\right) \text{ on S.}$$

Now $\nabla^2 F = 0$, and $\nabla^2\chi = 0$ at all points within S.

If, therefore, $\nabla^2 u = 0$ at all points within S,

$$\sigma u = \lambda\left(F + \frac{d\chi}{dx}\right) \text{ at all points within S.}$$

That is,

$$\sigma u = -\frac{dF}{dt} - \frac{d\psi}{dx}, \text{ \&c., on } S'.$$

Therefore the creation of the given system of currents on S acts as an electromotive force tending to produce the currents u, v, w with reversed signs on S′. And since this system of currents in S′ is self-inductive, it will be actually generated by induction. As an example, if

$$S = \frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2}$$

and $\phi = Az$,

$$u = \frac{dS}{dz}\frac{d\phi}{dy} - \frac{dS}{dy}\frac{d\phi}{dz} = -2A\frac{y}{b^2};$$

and therefore $\nabla^2 u = 0$.

It follows that the creation in an ellipsoidal shell of thickness proportional to the perpendicular from the centre on the tangent plane of a system of currents of the type $\phi = Az$ generates by induction the corresponding system of currents with reversed sign in an inner concentric similar and similarly situated ellipsoidal shell.

(17) *Case of an Infinite Plane: Arago's Disk.*—In this case, if the shell be of uniform thickness, a system of currents in it will not be generally self-inductive, but admits, nevertheless, of mathematical treatment. Suppose the plane to be fixed, and the field to revolve round an axis perpendicular to it, taken for that of z, with uniform angular velocity, ω.

Let γ be the normal force due to the field, γ' that due to the induced currents. Then we have, as the effect of induction,—

$$\frac{d\gamma}{dt} + \frac{d\gamma'}{dt} = 0.$$

As the effect of resistance—

$$\frac{d\gamma'}{dt} = \frac{\sigma}{2\pi}\frac{d\gamma'}{dz};$$

and, therefore, for the whole variation of γ'—

$$\frac{d\gamma}{dt} + \frac{d\gamma'}{dt} = \frac{\sigma}{2\pi}\frac{d\gamma'}{dz}.$$

When the motion is steady—

$$\frac{d\gamma}{dt} = \omega\frac{d\gamma}{d\theta}, \quad \frac{d\gamma'}{dt} = \omega\frac{d\gamma'}{d\theta},$$

θ being the angle through which the field has turned. Hence—

$$\omega\left(\frac{d\gamma}{d\theta} + \frac{d\gamma'}{d\theta}\right) = \frac{\sigma}{2\pi}\frac{d\gamma'}{dz},$$

a result which agrees with Maxwell's (23) of Art. 699.

June 21.—"Effects of Different Positive Metals, &c., upon the Changes of Potential of Voltaic Couples." By Dr. G. Gore, F.R.S.

In this research numerous measurements were made, and are given in a series of tables, of the effects upon the minimum-point of change of potential of a voltaic couple in distilled water (Roy. Soc. Proc., June 14, 1888), and upon the changes of electro-motive force attending variation of strength of its exciting liquid (*ibid.*), obtained by varying the kind of positive and of negative metal of the couple, and by employing different galvanometers. The measurements were made by the method of balance through a galvanometer, with the aid of a suitable thermo-electric pile (Birm. Phil. Soc. Proc., vol. iv. p. 130; *The Electrician*, 1884, vol. xi. p. 414). The kinds of galvanometer employed were, an ordinary astatic one of 100 ohms resistance, and a Thomson's reflecting one of 3040 ohms resistance.

The following were the proportions of hydrochloric acid (HCl), required to change the potential of different voltaic couples in water:—

TABLE I.—*Hydrochloric Acid.*

Astatic Galvanometer.

Zn + Pt	between 1 in	9,300,000	and	9,383,185
Cd + Pt	,, 1 ,,	574,000	,,	637,000
Mg + Pt	,, 1 ,,	516,666	,,	574,000
Al + Pt	,, 1 ,,	12,109	,,	15,000

Reflecting Galvanometer.

Zn + Pt	between 1 in	15,000,000	and	23,250,000
Cd + Pt	,, 1 ,,	1,162,500	,,	1,550,000
Mg + Pt	,, 1 ,,	775,000	,,	930,000
Al + Pt	,, 1 ,,	42,568	,,	46,500

With iodine and the astatic galvanometer the following proportions were required:—

TABLE II.—*Iodine.*

Zn + Pt	between 1 in	3,100,000	and	3,521,970
Mg + Pt	,, 1 ,,	577,711	,,	643.153
Cd + Pt	,, 1 ,,	2-0,431	,,	224,637

With bromine and the astatic galvanometer:—

TABLE III.—*Bromine.*

Mg + Pt	between 1 in	310,000,000	and	344,444,444
Zn + Pt	,, 1 ,,	77,500,000	,,	84,545,000
Cd + Pt	,, 1 ,,	3,470,112	,,	3,875,000

The magnitudes of the minimum proportions of bromine required to change the potentials of the three couples in water varied directly as the atomic weights of the three positive metals.

With chlorine the following were the minimum proportions required:—

TABLE IV.—*Chlorine.*

With the Reflecting Galvanometer.

Mg + Pt	between 1 in	27,062,000,000	and	32,291,000,000

With the Astatic Galvanometer.

Mg + Pt	between 1 in	17,000,000,000	and	17,612,000,000
Zn + Pt	,, 1 ,,	1,264,000,000	,,	1,300,000,000
Zn + Au	,, 1 ,,	518,587,360	,,	550,513,022
Cd + Pt	,, 1 ,,	8,733,585	,,	9,270,833
Zn + Cd	,, 1 ,,	55,436	,,	76,467

In the case of chlorine, as well as that of bromine, the magnitudes of the minimum proportions of substance required to change the potential of magnesium-platinum, zinc-platinum, and cadmium-platinum, varied directly as the atomic weights of the positive metals.

The examples contained in the paper show that the proportion of the same exciting liquid necessary to disturb the potential of a voltaic couple in water varied with each different positive or negative metal, and that the more positive or more easily corroded the positive metal, or the more negative and less easily corroded the negative one, the smaller usually was the minimum proportion of dissolved substance necessary to change the potential.

By plotting the results in all cases, it was found that the order of change of potential, caused by uniform change of strength of liquid, varied with each positive metal.

The results also show that the degree of sensitiveness of the arrangement for detecting the minimum-point of change of potential depends largely upon the kind of galvanometer employed.

As a more sensitive galvanometer enables us to detect a change of potential caused by a much smaller proportion of material, and as the proportion of substance capable of detection is smaller the greater the free chemical energy of each of the uniting bodies (Roy. Soc. Proc., June 14, 1888) it is probable that the electromotive force really begins to change with the very smallest addition of the substance, and might be detected if our means of detection were sufficiently sensitive, or the free chemical energy of the uniting bodies was sufficiently strong.

"The Voltaic Balance." By Dr. G. Gore, F.R.S.

A New and Simple Lecture Experiment.—Take two small clean glass cups containing distilled water; simultaneously immerse in each a small voltaic couple, composed of either unamalgamated magnesium or zinc with platinum, taking care that the two pieces of each metal are cut from the same piece and are perfectly clean and alike. Oppose the currents of the two couples to each other through a sufficiently sensitive galvanometer, so that they balance each other and the needle does not move. Now dip the end of a slender glass rod into a very weak aqueous solution of chlorine, bromine, iodine, or hydrochloric acid, and then into the water of one of the cups. The voltaic balance is at once upset, as indicated by the measurement of the needle, and may be shown to a large audience by means of the usual contrivances.

The chief circumstance to be noticed is the extremely great degree of sensitiveness of the arrangement in certain cases. This is shown by the following instances of the minimum proportions of substance required to upset the balance with an ordinary astatic galvanometer, and with a Thomson's reflecting one of 3040 ohms resistance.

1. *Zinc and Platinum with Iodine.* — With the astatic galvanometer, between 1 part of iodine in 3,100,000 and 3,521,970 parts of water.

2. *Zinc and Platinum with Hydrochloric Acid.*—With the astatic galvanometer, between 1 in 9,300,000 and 9,388,185 parts; and with the reflecting one, between 1 in 15,500,000 and 23,250,000 parts.

3. *Magnesium and Platinum with Bromine.*—With the astatic galvanometer, between 1 in 310,000,000 and 344,444,444 parts.

4. *Zinc and Platinum with Chlorine.*—With the astatic galvanometer, between 1 in 1,264,000,000 and 1,300,000,000 parts.

5. *Magnesium and Platinum with Chlorine.*—With the astatic galvanometer, between 1 in 17,000,000,000 and 17,612,000,000 parts; and with the reflecting one, between 1 in 27,062,000,000 and 32,291,000,000 parts of water.

Every different soluble substance requires a different proportion, and with unlike substances the difference of proportion is extremely great. With solutions of neutral salts, the proportion of substance required to upset the balance is large; for instance, with chlorate of potash, a zinc-platinum couple, and the astatic galvanometer, it lay between 1 part in 221 and 258 parts of water.

The degree of sensitiveness of the balance is usually greater, the greater the degree of chemical affinity the dissolved substance has for the positive metal and the less it has for the negative one.

By first bringing the balance with a magnesium-platinum couple and the astatic galvanometer nearly to the upsetting-point by adding 1 part of chlorine to 17,612,000,000 parts of water, and then increasing the proportion to 1 in 17,000,000,000, the influence of the difference, or of 1 part in 500,000,000,000, was distinctly detected.

"Magnetic Qualities of Nickel." (Supplementary Paper.) By J. A. Ewing, F.R.S., Professor of Engineering in University College, Dundee.

The paper is a supplement to one with the same title by Prof. Ewing and Mr. G. C. Cowan, which was read at a recent meeting of the Society. It describes experiments, conducted under the author's direction by two of his students, Mr. W. Low and Mr. D. Low, on the effects of longitudinal compression on the magnetic permeability and retentiveness of nickel. The results are exhibited by means of curves, showing the relation which was determined between the intensity of magnetisation of the metal and the magnetising force, when a nickel bar, reduced to approximate endlessness by a massive iron yoke which formed a magnetic connexion between its ends, was magnetised under more or less stress of longitudinal compression. Corresponding curves show the relation of residual magnetism to magnetising force, for various amounts of stress; and others are drawn to show the relation of magnetic permeability to magnetic induction. Initial values of the permeability, under very feeble magnetising forces, were also determined. The experiments were concluded by an examination of the behaviour of nickel in magnetic fields of great strength. Magnetising forces ranging from 3000 to 13,000 C.G.S. units were applied by placing a short bobbin with a narrow neck made of nickel between the poles of a large electromagnet, and it was found that these produced a practical constant intensity of magnetisation which is to be accepted as the saturation value.

PARIS.

Astronomical Society, June 6.—M. Flammarion, President, in the chair.—Various drawings and observations were sent by MM. Petit, Rengel, and G. Vallet.—M. Flammarion read a paper on the solar eclipses of the 19th century, shewing strong discrepancies between M. Oppolzer's charts and the results of observation. Replying to M. Oppert, M. Flammarion said he should not advise historians to base their investigations on those charts.—M. M. Cornillon sent drawings of a large sunspot from May 11 to 23. M. Schmoll said that this spot was just on the limits of visibility to the naked eye from May 16 to 18.—M. Gaudibert sent a drawing of the lunar crater Flammarion. A fine rill traverses this crater, and extends to Réaumur after being interrupted by some hills.—M. Schmoll related an observation of the lunar crescent on May 12, the moon being 42½ hours old. Its breadth was from 30″ to 35″.—M. Trouvelot presented to the Society a series of celestial photographs offered by Prof. Pickering, of Harvard College. The photograph of the Pleiades is specially interesting, and shows the straight trails of nebulous matter which form such a striking feature in the last negatives obtained by MM. Henry.—Thanks were returned to Prof. Pickering, who was unanimously named honorary member of the Society on the proposition of M. Trouvelot and Colonel Laussedat.—Colonel Laussedat explained his method of computing solar eclipses graphically, which is two or three times more rapid than the usual numerical calculation.

AMSTERDAM.

Royal Academy of Sciences, June 30.—M. Beyerinck stated the results he has obtained from experiments on hybridism or crossings with common barley (*Hordeum vulgare, H*) *hexastichon, H. distichon, H. Zeocriton*, and *H. trifurcatum.* made by him since 1884 on a large scale, and illustrated his subject with specimens, some dried and others preserved in spirits. He described the precautions to be taken in such crossing experiments, and deduced the following conclusions:—(1) All the above-mentioned sorts of barley may be crossed with facility, indiscriminately. (2) The hybrids thus obtained are very perfectly self-fertile; those produced from *H. vulgare* (fem.) and *H. distichon* (m.), and those from *H. vulgare* (fem.) and *H. Zeocriton* (m.) even cleistogamous. (3) The hybrids of the first generation partake in general of a middle shape between the two parents. An exception to this rule was made by those of *H. nudum* (fem.) and *H. trifurcatum* (m.), a great part of which proved to belong to the not expected common intermediate form between *H. vulgare* and *H. distichon*. A few specimens belonged to the expected *cornutum* form. (4) The seedlings from hybrids obtained by self-fertilization are very various. The speaker obtained, besides a few already known ones, some quite new varieties. It was remarkable that the third generation of a cross between *H. vulgare* (fem.) and *H. Zeocriton* (m.) produced *H. hexastichon*. (5) In the present summer, a cross effected in 1884 between *H. distichon* (fem.) and *H. trifurcatum* (m.) produced a form almost completely without awns.—M. Fürbringer imparted the results of a research made by M. J. F. van Bemmelen into the origin of the fore-limbs and of the lingual muscles in reptiles.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Electric Lighting; Its Present Position and Future Prospects: Hammond and Co. (Whitehead, Morris, and Lowe).—A System for the Construction of Crystal Models on the Type of an Ordinary Plait: John Gorham (Spon).—An Introduction to the Science and Practice of Photography: Chapman Jones (Iliffe and Sons).—Religion and Science: W. Fitzgerald (Hodges, Figgis, and Co.).—A Practical Decimal System for Great Britain and her Colonies: R. T. Rohde (E. Wilson).—The Rothamsted Experiments on the Growth of Wheat, Barley, and the Mixed Herbage of Grass Land: Prof. W. Fream (Horace Cox).—Rock-Forming Minerals: Frank Rutley (T. Murby).—Smithsonian Report, 1885, Part 2 (Washington, U.S.)—The Glasgow and West of Scotland Technical College Calendar, 1888.

CONTENTS.

THURSDAY, AUGUST 9, 1888.

THE ZOOLOGICAL RESULTS OF THE "CHALLENGER" EXPEDITION.

Report on the Scientific Results of the Voyage of H.M.S. "Challenger" during the Years 1873-76, *under the command of Captain George S. Nares, R.N., F.R.S., and the late Captain Frank T. Thomson, R.N.* Prepared under the superintendence of the late Sir C. Wyville Thomson, Knt., F.R.S., and now of John Murray, one of the Naturalists of the Expedition. Zoology—Vols. XXIII., XXIV., and XXV. (Published by Order of Her Majesty's Government, 1888.)

THE first two memoirs in Vol. XXIII. are Reports on the Pteropoda by Dr. Paul Pelseneer. Dr. Pelseneer's Report on the Gymnosomatous division of the Pteropods was published in Vol. XIX., and we now have his Report on the Thecosomata and one on the anatomy of the whole group.

In the first of these Reports all the certainly genuine species at present described are enumerated, and full details are given about all those which have been more or less imperfectly described. As the diagnoses of the families and genera of the Pteropods seem to have been copied from originals of a comparatively early date and without modification, it has been necessary on re-study to re-write these, so as to bring them up to the level of scientific accuracy. This monographic study of the subgroup of the Thecosomata has been based not only on the collections made by the *Challenger*, but on those in the British and Brussels Museums, as well as those of several private collections. Like the Gymnosomata, these Thecosomata are pelagic Mollusks, which descend to certain depths to avoid too bright a light, and reascend to the surface of the water when the light is feeble or absent, and when the sea is calm. With a less highly organized alimentary system than the Gymnosomata, the Thecosomata content themselves with humble prey, feeding mainly on Radiolaria, Foraminifera, Infusoria, and even on some of the lower Algal forms.

The Thecosomata were taken alive at seventy different stations, and while they include twenty-eight species, representing all the known genera, they have all been already described. Of those dredged from the deep sea, where "Pteropod ooze" was found, some twenty-four species could be distinguished, of which one was new to science. The total number of Thecosomata now known amounts to forty-two.

While the generic titles given to living forms amount to thirty-four, these can well be included, according to the author, in the following eight:—

Limacina, Cuvier.	Cavolinia, Abildgaard.
Peraclis, Forbes.	Cymbulia, Peron and Lesueur.
Clio, Linn.	Cymbuliopsis, gen. nov.
Cuvierina, Boas.	Gleba, Forskal.

In an appendix to the account of the species of the first two of these genera, some account is given of the forms described by A. Adams as *Agadina stimpsoni*, and *A. gouldi*, which are proved to be Gastropod larvæ.

Shells of Thecosomata have not been found in a greater depth than 1950 fathoms. Mr. J. Murray attributes this to the greater proportion of carbon dioxide in the water at greater depths, and to the more rapid solution of the delicate shells in sea-water under great pressure.

The third part of Dr. Pelseneer's Report treats of the anatomy of the whole of the Pteropods. With it this Report is now the most comprehensive treatise in existence on the group. As a result of his studies he regards the Pteropods as forming not a primitive group, but, on the contrary, a recent and specialized one—a terminal group. There are in it but a small number of species; these exhibit only a slight variability, and they are profoundly modified in adaptation to a special mode of existence.

Since the days of Cuvier the Pteropoda have been regarded as forming a distinct class among the Mollusca, of the same value as the Cephalopoda, Gastropoda, &c.; but Dr. Pelseneer regards it as proved that they are but Gastropods, in which the adaptation to pelagic life has so modified their external characters as to give them an apparent symmetry; that even among the Gastropods they do not constitute a distinct sub-class, nor even an order, that they belong to the Euthyneura, and among these to the Pectibranchiate Opisthobranchs, differing less from these than they differ from the other Opisthobranchs. The Thecosomata and Gymnosomata are two independent groups, not having a common origin, the former having descended from the Bulloidea, and the latter from the Aplysioidea.

These Reports are illustrated with seven plates.

The third Report in this volume is by Prof. G. J. Allman, forming the second part of his memoir on the Hydroida. The author has taken advantage of the opportunity afforded by the typical character of the collection to make it the basis of a general exposition of Hydroid morphology, and this from the present standpoint of our knowledge, so that this Report is not a mere mass of descriptive and distributional details, but one which will have an abiding interest for the biologist.

The rare occurrence in the collection of British species is striking, and would seem to indicate a peculiar definiteness in the geographical distribution of the Hydroids.

The few Gymnoblastic Hydroids in the collection belong to three genera—Stylactis, Eudendrium, and Monocaulos; the species (*M. imperator*), by which the last genus is represented, being perhaps the most remarkable Hydroid obtained during the Expedition. The stem, though only half an inch in thickness, was 7 feet in height, the hydranth extending, from tip to tip of the tentacles, to a width of 9 inches, so that, as regards size, all other Hydroids sink into insignificance when compared to it; while the depth of about 4 statute miles from which it was brought up adds to the special interest of this marvellous animal.

The families of the Calyptoblastea were numerously represented in the collection, and among those of which few examples had hitherto been known are those to which belong the genera Cryptolaria and Grammaria, as well as a new and interesting genus, Perisiphonia. Idia, hitherto only known by the poor description and figure of Lamouroux, proved, on the examination of good specimens of the only species, *I. pristis*, Lamx., to be constructed on a type quite unique among the Hydroida.

Among other families largely represented was that of the Haleciidæ, with not only many new species, but with a new genus, marked by the phenomenon that the colony is provided with bodies which admit of close comparison with the sarcostyles and sarcothecæ of the Plumularinæ.

The curious genus Synthecium, in which the gonangia spring from within the cavity of the hydrotheca, is represented by two new species, both from the Australian seas. There also occur fine examples of the remarkable genus Thecocladium, in which every branch of the colony springs, like the gonangium in Synthecium, from within the cavity of the hydrotheca.

As regards the classification of the Hydroida, the author acknowledges that the time for a complete system has not yet come, such a one should include not only all Hydroid trophosomes with their associated gonosomes; but all the existing Hydromedusæ should have been traced to their respective trophosomes, there are however many of these Hydromedusæ not so traced, though we may be certain that their trophosomes exist. Of those Hydromedusæ into whose life history a polypoid term has never apparently been intercalated, a separate and well defined group must be formed. Thus the sub-orders may be neatly defined as :—(1) *Gymnoblastea.* No hydrothecæ or gonangia. Nutritive zooids when more than one forming permanent colonies Planoblasts in the form of Anthomedusæ. (2) *Calyptoblastea.* Hydranths protected by hydrothecæ. Sexual buds protected by gonangia. Nutritive zooids forming permanent colonies. Planoblasts in the form of Leptomedusæ. (3) *Eleutheroblastea.* No hydrothecæ or gonangia. Nutritive zooids not forming permanent colonies. No differentiated gonophores. (4) *Hydrocorallia.* A calcareous corallum (cœnosteum) permeated by a system of ramified and inosculating cœnosarcal tubes from which the hydranths are developed. (5) *Monopsea.* Free Hydromedusæ which are developed directly from the egg without the intervention of a polypoid trophosome. Auditory clubs with endodermal otolites on the umbrella margin, and (6) *Rhabdophora* (Graptolites). Hydranths replaced by sarcostyles. Hydrocaulus traversed by a chitinous longitudinal rod.

Thirty-nine plates accompany this portion of Prof. Allman's memoir, the enlarged figures on these are all from the pencil of the author, while the figures representing the forms of their natural size have been for the most part drawn from the specimens by Miss M. M. Daniel, and transferred to the stone by Mr. Hollick.

The Report taken in connection with the previously published one on the legion of the Plumularinæ constitutes a most comprehensive and valuable history of the Hydroids for which all biological students will feel their indebtedness to the author.

The fourth Report is on the Entozoa, by Dr. O. von Linstow of Göttingen. The number of Entozoa collected was but small, and chiefly from the alimentary tract of birds; four new species of Ascaris, three of Filaria, and one of Prothelmins, among the Nematodes, four species of Tænia, and two of Tetrabothrium among the Cestoids, are described and figured in the two plates accompanying the Report.

The fifth Report, also a short one, is by Edgar A. Smith, on the Heteropoda. Although no new species are described, several are indicated of which the material was not sufficient to enable the form to be described with certainty.

A most useful and wonderfully complete synonymic list of all known forms of the group is given, and this Report will be found of the greatest value to all interested in the Heteropods.

Vol. XXIV. contains the Report, by C. Spence Bate, F.R.S., on the Crustacea Macrura, or rather on the larger portion of those found during the Expedition. This Report forms a volume of over 1030 pages, which is bound up separately from the 157 lithographic plates; and in the preparation of this great and laborious work and its illustrations Mr. Spence Bate has occupied all his leisure during the last ten years.

Of the enormous mass of detail in this volume it would be impossible to give within our limits any intelligible account; not only are the generic and specific diagnoses given with minute accuracy, but we are, in addition, favoured with a deeply interesting account of all that is known as to the developmental stages of the species; for this latter purpose the notes and drawings from life of the late Dr. Willemoes Suhm have been largely and most properly used. The extreme imperfection of the records of the life-history of even some of our well-known forms is strongly insisted upon, and we would call attention to the subject in the hope that we may direct the energies of some of our younger biologists to this fertile field of research.

The great and recognized experience of the author in all that concerns this section of the Crustacea makes his opinions, founded on so large a knowledge, as to the classification thereof, of importance. Accepting the divisions of this sub-order of the Decapods, called by Huxley Trichobranchiata and Phyllobranchiata, though with a slightly different arrangement of some of the families, the author follows Dana in placing the Penæidea in a separate division, with the name Dendrobranchiata, "while the Squillidæ, Mysidæ, &c.—that is, the Schizopoda originally, and later the Stomapoda of Latreille, Milne-Edwards, and De Haan—are arranged under the head of Anomobranchiata, which term was first used by Dana and afterwards by Heller; it has therefore priority of date, and is less liable to misconception than the term Abranchiata" of Huxley (p. 6). Afterwards we find, on a review of the forms included under the Dendrobranchiata, that the Schizopoda may be regarded as an aberrant group of this tribe. Prof. Sars, who, it will be remembered, described the Schizopoda of the *Challenger* Expedition ("Zool. Reports," Part 37) thought "it more appropriate for the present to assign to this group the rank of a distinct tribe or sub-order, there being several well-marked characters distinguishing these Crustacea rather sharply from all other known Decapods." Mr. Spence Bate, however, thinks "that with the exception of the variable condition of the pereiopoda, the several genera do not possess a single character that is not held in common with some genus of the Macrura," and concludes from excellent reasons given in detail "that the natural position of these animals is that of an aberrant tribe of the Dendrobranchiata, more nearly allied to the degraded forms of the Penæidea than to those of any other group" (p. 472).

Each of the three divisions of the Macrura are divided into two sections—the Aberrantia and the Normalia. In the former section of the Trichobranchiata the family Galathæidæ occurs, which will form the subject of a Report yet to appear by Prof. J. R. Henderson.

The group Aberrantia of the division Phyllobranchiata consists of several tribes and families that in their adult condition approach more nearly to the characters common to other divisions, but which nevertheless during the progress of development pass through a stage common to the normal Phyllobranchiate Macrura. This aberrant group has long been known to biologists under the name of Anomura, and by some has been regarded as a distinct order of Crustacea. Here it is however regarded as a group of the Phyllobranchiate division of the Macrura, "for undoubtedly in their earlier stages they pass through a morphological change that is essentially Macrurous, in which the scaphocerite and rhipidura are both present as well-devoloped appendages, the latter of which they never entirely lose."

This group will be reported on by Prof. John R. Henderson, although two new genera and several new species are described and figured in the present Report.

It only remains to mention that with the exception of two out of the 157 plates all have been lithographed from the original drawings of Mr. Spence Bate. By this fact the value of this Report is intensified, as the author has been able to describe and figure what he has seen with a clearness and distinctness which far surpasses in effect the most brilliant work of the cleverest of artists. In an appendix Dr. Hoek gives a description, with figures, of *Sylon challengeri*, a new parasite Cirriped.

Vol. XXV. also contains but a single Report, that on the Tetractinellida, by Prof. W. J. Sollas. Perhaps no department of zoology has made during the last twenty years such rapid progress as the Sponges, and it is astonishing to think of the large number of forms that have been very fully examined during this period. Certainly no group has benefited more largely by the researches made during the expedition of the *Challenger*, and it was the greatest good fortune that the collections made were submitted to such excellent workers as Polejaeff, F. E. Schulze, Ridley, Dendy, and Sollas. The joint Reports of these authors, and the splendid series of illustrations which accompany them, form a complete history of this group up to the existing state of our knowledge, a history which shows the worker what is not known as well as what is.

The last of these Reports treats of the Tetractinellida, and in an appendix of a small group of Monaxonida, about the exact location of which there was for long some doubt. In its monographic completeness it surpasses all the other Reports on the Sponges, while in the fullness of its morphological details it may well serve as an introduction to a knowledge of all the orders.

The Tetractinellid Sponges of the *Challenger* having been well preserved, it was possible to make a thorough investigation of their minute anatomy, a work involving an enormous amount of labour in the cutting of thousands of thin sections, and the separate examination of most of them. The number of species and varieties obtained by the *Challenger* was 87, of which 73 are new to science. These are arranged in 38 genera, of which 18 are new. In addition there are 221 species mentioned, making the total number of described species 294, and of accepted genera 81.

Dividing the Sponges into the two classes of the Megamastictora (with the single sub-class Calcarea) and Micromastictora, the latter is divided into the three subclasses of Myxospongiæ (Halisarca, &c.), Hexactinellida, and Demospongiæ. The subdivision of this last may be made primarily into two tribes : (1) the Tetractinellida, (2) the Monaxonida. The former may be characterized as Demospongiæ in which some or all of the scleres are tetraxons, triænes, or desmas. The name Tetractinellida was first proposed by Marshall (1876) in practically the same sense as it is used now by Sollas.

Into the details of the sub-orders and families of this tribe our space forbids us to enter. Their descriptions, with those of the genera, will be found in orderly sequence in the introductory chapter, while the descriptions of the species occupy 410 pages of the Report.

In an appendix we have an account of the Sponges belonging to the Spintharophorous sub-order of the Monaxonida, which, under the impression that they were more nearly related to the Tetractinellida, had been omitted from Ridley and Dendy's Report of the Sponges of this tribe.

The figures of the Sponges on the forty-four chromolithographic plates accompanying the Report were drawn by the well-known artist, T. H. Thomas, R.C.A. The Sponge portraits are really beautiful studies from the originals. The figures representing structure were first traced by the author with the camera lucida, and were then drawn by Mr. Thomas direct from the preparation under the microscope.

MATTHEW FONTAINE MAURY.

A Life of M. F. Maury, U.S.N. and C.S.N. Compiled by his Daughter, Diana Fontaine Maury Corbin. (London: Sampson Low, 1888.)

A MEMOIR of the illustrious founder of the science of the physical geography and meteorology of the sea, written by the tender and loving hand of his daughter, cannot fail to be of interest, not merely to that section of thinkers and workers who are engaged in the branch of science which Maury especially cultivated and adorned, but to the larger world who appreciate, and are benefited by, the perusal of the biography of a man of powerful and vitalizing imagination, disinterested labour for the public good, self-denying patriotism, and indomitable perseverance.

Family memoirs are too often apt to degenerate into a mere panegyric of public and private virtues, coupled with a disinterment of private matters which an unbiassed stranger would have too much tact and modesty to expose, and which often destroy all the effects of the accompanying eulogy. Mrs. Diana Corbin has, fortunately, succeeded in avoiding these pitfalls, and by a judicious blending of history, correspondence, and extracts from lectures, has enabled the reader to form his own judgment of the merits and services of her renowned father.

Descended from the French Huguenots on one side, and the English Protestants on the other, Maury seems to have united in his own person the lively imagination we unconsciously associate with the former, together with the somewhat austere and unflinching determination of the latter; and it was by the rare union of these two qualities that he was enabled to vivify the dry statistics which, until his arrival, lay buried in the log-books on the shelves of the Hydrographic Bureau at Washington, like the ooze at the bottom of the Atlantic.

An accidental fall from a tree, early in life, took him from the farm to school; and a subsequent fall from a stage-coach, which permanently crippled him, appears to have exercised a still greater effect on his career by diverting him from his active physical service in the American Navy, to the mental study of the scientific branches of the profession. His appointment to the Naval Office at Washington, mainly through the publication of his anonymous "Scraps from the Lucky-bag," on naval reform, led to its subsequent development into what is now the world-known National Observatory and Hydrographical Department of the United States. Here it was that he inaugurated his "sailing directions," and elaborated his famous "wind and current charts," the absolute commercial value of which, in shortening voyages, was soon universally recognized, though, as usually happens, most tardily by his own country, where, though a Bill for remunerating their author to the extent of £5000 appears to have been brought forward (unknown to Maury) in January 1855, in the following month he was virtually placed in official disgrace, by being retired from the Active Naval List and having his salary reduced to £300.

This manifest injustice to a man, whose mind, if not body, was actively engaged in the highest branches of naval service to his country, was, after persistent vindication of his rights, repaired in 1858, when he was promoted to the rank of Commander, with back pay from the time of his retirement.

While tabulating the observations for his charts, Maury fascinated the world by the publication of the "Physical Geography of the Sea and its Meteorology," a book which, although some of its conclusions—such as an open sea surrounding the North Pole, and the crossing of the winds at the calm belts—have been found to be untenable in the light of more recent facts and research, still remains substantially trustworthy, and certainly unequalled by any modern treatise embracing the same subjects. It would be difficult to adequately estimate the immense contemporaneous and subsequent value of such a work, written in the charming and enthusiastic style which characterized all its author's productions. The present writer traces with gratitude his first attraction to physical geography and meteorology to this delightful book, of which most truly it can be said, that it realized Matthew Arnold's ideal combination, "sweetness and light."

By this book, Maury not only taught the world, but he pleased it at the same time, and he accomplished this rare result, without pandering in any way to mere popular taste, or forsaking the platform of truth. His popularization of a subject until then hardly dreamed of as a science resulted in the greatest achievement of his life, viz. the assembly, chiefly through his instrumentality, of the International Meteorological Congress at Brussels, in 1853, which marked the commencement of the present co-operation of nations in the work of both marine and land meteorology. Regarding the latter, indeed, Maury uttered a prediction, on p. 350 of his "Physical Geography," to the effect that "the greatest move that can now be made for the advancement of meteorology is to extend this system of co-operation and research from the sea to the land, and to bring the magnetic telegraph regularly into the service of meteorology."

At the present time, when the old question between the "cyclonologists" so-called and the "aspirationists" seems likely to be renewed by M. Faye and some of his disciples, it is interesting to notice that Maury never accepted either the purely circular doctrine of Reid, or the purely radial theory of Espy, but agreed with Thom and Redfield in thinking that the wind in a true cyclone blows in spirals, and he gave excellent reasons for his belief.

Maury's study of marine meteorology and physical geography not merely aided commerce by shortening passages, but enabled him to give material assistance to the laying of the first Atlantic cable to Europe; and, in fact, it was to his prediction of the "telegraphic plateau," and acute suggestion of a cord instead of heavy cable for the deep-sea portion, that the ultimate success of this enterprise was mainly due. From the sea, Maury turned his attention to the Great Lakes and the land, and his ardent espousal of the cause of agricultural meteorology, and the lecturing tours he made on behalf of this subject in all parts of the States, not only led to the establishment of the present magnificent Weather Bureau of the United States, but incidentally to his own decease through the fatigue and exposure thus encountered.

Maury's early religious training and temperament appear to have exercised a large influence on his public and private life. His physical geography is illustrated by frequent extracts from the Book of Job, and is instinct with the same spirit which prompted and pervaded the memorable Bridgwater Treatises. The following extract from his address to the University of the South will indicate this phase of his mind:—

"Astronomy is grand and sublime, but astronomy overpowers with its infinities and overwhelms with its immensities. Physical geography charms with its wonders, and delights with the benignity of its economy. Astronomy ignores the existence of man; physical geography confesses that existence, and is based on the Biblical doctrine that the earth was made for man. Upon no other theory can it be studied—upon no other theory can its phenomena be reconciled."

The Civil War unfortunately destroyed the continuity of Maury's work at Washington, and altered the whole course of his subsequent life. Impelled by a spirit of pure patriotism towards the State of Virginia which had reared him, he threw up his post in the North, and devoted himself to the Southern cause. No one who reads the life before us, and his "vindication of the South and of Virginia" in the appendix, can doubt the pure unselfishness of his motives. He had everything to lose, and nothing to gain, from a material point of view, by his action, and well he knew it. Essentially a man of peace, and deeply attached to his work at Washington, we cannot but admire his voluntary resignation of all to a sense of duty.

His scientific abilities being directed into a new channel, led to the development of the electrical torpedo, by which he materially aided the South, and which he afterwards introduced into Europe, whither he was sent during the war, to purchase torpedo materials.

His subsequent connection with Mexico, and his scheme for emigrating Southerners thither, though designed with a view to ameliorate the condition of his countrymen, and to open up a grand country, was never approved of by his friends, was politically a mistake, and terminated abruptly with the abandonment of the country by the French, and the assassination of the Emperor Maximilian. After this he returned to England, and, ultimately, to a Professorship in Virginia.

All through his chequered life he maintained an unfaltering devotion to meteorology, and his latest efforts were directed to developing a comprehensive system of crop and weather reports throughout the States.

The perusal of this interesting book leaves us with a deep impression of the comprehensive grandeur and philanthropy of Maury's mind. A rare spirit of devotion to science, not merely for the pleasure it always affords its devotees, but for the good it could achieve in the service of man, pervaded his whole life, and the additional record here presented of work done and schemes initiated, will add fresh laurels to the imperishable fame of its subject. E. DOUGLAS ARCHIBALD.

OUR BOOK SHELF.

Pflanzen-Teratologie. Von Maxwell T. Masters, M.D., F.L.S. Ins Deutsche übertragen von Udo Dammer. (Leipzig: H. Haessel, 1886.)

IT will be satisfactory to English botanists to find that a translation of Dr. Masters's classical work on vegetable teratology has been called for in Germany. The present German edition is not, however, simply a translation, as it has received many additions from the hand of the author. The work is thus of interest to English as well as to German readers, for it constitutes the most complete account in any language of abnormal structures in plants. The great value of such a record of teratological facts will be admitted by all botanists, however much they may differ as to the morphological significance of these phenomena.

In the German edition, the number of figures in the text has increased from 218 to 243. As a few of the original woodcuts have been omitted, the number of new figures is somewhat greater than appears from the total increase. Besides the additional woodcuts, a lithographed plate has been added, drawn by the translator from original figures of Göschke and Magnus.

Some of the more important additions to the original work may here be noticed. At p. 35 a new section is introduced, on fasciation of the root, illustrated by a woodcut (Fig. 8) of the singularly fasciated aërial roots of *Aerides crispum*. Caspary's view that only a single growing point takes part in the formation of each fasciated root is cited.

Fig. 66 (p. 155) shows a proliferous male flower of a Begonia, in which the stamens are entirely absent, and replaced by flower-buds. The curious case of the development of flower-buds on the root in Pyrus is illustrated by Fig. 91, described at p. 188. A remarkable abnormality in a Fuchsia is shown in Fig. 98 (p. 208). Here two stamens (one simple and the other branched) have arisen in the axils of a pair of foliage-leaves, which are adherent to the inferior ovary. On p. 213 some figures have been added to further illustrate the formation of adventitious siliquæ in Cruciferæ in the interior of the normal fruit. In Figs. 131, 132, and 133 (p. 257) three interesting cases of regular peloria in orchids are shown.

A striking instance of pistillody of the stamens in a Begonia is figured on p. 353 (Fig. 178). In this flower the stamens were replaced by open carpels each bearing a large number of marginal ovules. A conspicuous abnormality in an Anthurium is shown in Fig. 204 (p. 411), under the head of "Polyphylly." Here a great number of large foliaceous bracts are developed on the spadix, completely altering the character of the inflorescence.

Two instances of polyandry in an Odontoglossum are represented in Figs. 213 and 214 (p. 439). In the former of these cases all the six stamens of the typical Monocotyledonous flower are present.

It should be mentioned that the additional woodcuts are generally reproductions of figures originally published by the author in the *Gardener's Chronicle*. In the plate added by the translator the most interesting figures are perhaps those illustrating a remarkable series of abnormal forms of the foxglove, the number of parts in a whorl varying from one to fourteen, and the flower in many cases being actinomorphic instead of zygomorphic. These figures, like most of those on the plate, are taken from papers by Magnus.

It is much to be wished that the numerous observations on teratology accumulated by Dr. Masters and others since 1869 could be embodied in a new and complete English edition. Until this wish is realized, the present German edition is likely to remain the most extensive treatise on the subject. D. H. S.

Parish Patches. By A. Nicol Simpson. (Arbroath: Thomas Buncle, 1888.)

THIS volume consists of a series of short essays, each of which gives expression to the author's delight in some particular aspect of Nature. He presents no new ideas or observations, but he has so warm a love for what he calls the pastoral side of life, that most of his readers will find something to interest them in his glowing descriptions of scenes which appeal strongly to his sympathies. The work is well printed on good paper with wide margins, and it is carefully illustrated by engravings from drawings by Mr. John S. Fraser.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Functionless Organs.

I HAVE read with extreme interest the abstract, given in your number of July 26 (p. 310), of a paper by Prof. Ewart, on the "Structure and Development of the Electric Organ of *Raia radiata*." It bears upon a question of fundamental importance in biological science. Organic nature is full of organs, or of structures, which are either wholly or partly functionless. Sometimes they are called "aborted"; sometimes "degenerated"; sometimes "rudimentary"; sometimes "representative." But under whatever name, the Darwinian philosophy almost invariably explains them as structures, or parts of structures, which must have once been useful, and have become functionless by atrophy or disuse.

This is a natural and necessary consequence of the doctrine which ascribes all organic structures to utility as a physical cause. Utility as a mental purpose is kept out of sight. Utility in this last sense explains rudimentary structures by the uses or purposes which they are to serve in the future, or which, at least, they are capable of serving in the future. In this aspect rudimentary structures become "prophetic germs." But we now know that Darwin denounced this interpretation of them, and saw that if the doctrine of prophetic germs could be established, his own theory would be reduced to rubbish.

Accordingly the more advanced Darwinians always consider functionless organs or structures as relics of a past in which they were useful. They are never interpreted as utilities which are yet to be.

I have always thought that if the doctrine of development be true, functionless organs must be, as often as not, the germs of potential use, and not necessarily at all the remains of past actual use.

What we want in this great question is physiological facts to indicate the one interpretation or the other. Hitherto I have never met with a case in which any expert interprets functionless organs as structures on the way to use. Perhaps no organ in any creature is more wonderful than the electric organs of certain fish. Any light cast upon their origin is a light cast on all organic apparatus. Here we have a case in which a distinguished physiologist detects, or thinks he can detect, an organ in process of being built up for the discharge of a very definite and peculiar function—a function for which it is not yet fit, or is but very imperfectly fitted.

This fact does not tell against development or evolution. But it does tell, and tells fatally, against the element of fortuity, which is inseparable from the idea of "natural selection," and to which Darwin attached so much importance, at one period of his life, and to which many of his disciples attach equal importance still. The fortuitous element is, in fact, the main ground on which they value it. But everywhere, in reasoning and in observation, it is breaking down. ARGYLL.

"Syrrhaptes paradoxus."

CONCERNING Prof. Newton's remark in NATURE, July 26, p. 295, on the occurrence of *Syrrhaptes paradoxus* in France, I beg to communicate that I picked the following dates out of several journals:—

May 28: On the sand-downs of Noirmoutier, Dieu, and Olonne, in the Vendée (several hundreds; three were killed).

May 31: Calais (ten specimens; one was killed).

Commencement of June: Nantes, Bretagne (one killed).

Middle of June: North of the country.

I am sure that we shall get much more news from France.

Dresden, August 2. A. B. MEYER.

Milk *v.* Fire.

IN Mr. Rust's note in NATURE, vol. xxxvii. p. 583, there is mention of a superstition that milk alone can extinguish a fire kindled by lightning—a belief that existed in Cambridgeshire, and which is entertained by the Sudan Arabs.

The Sinhalese (natives of Ceylon) have a similar belief in the efficacy of milk. When an epidemic such as small-pox breaks out in a village, two games of a religious character, *An-Edima* (horn pulling) and *Pol-gehima* (striking cocoa-nuts together), are played in public for a couple of days. Then the Kapurála (lay priest), and those who have taken part in the games, go in procession with music, &c., to every house in the village, where arrangements have been made for the Kapurála's reception. The house and grounds are cleaned; the inmates wear newly-washed clothes; and portions of the ceiling and floor are covered with white cloths. A lamp is lit at the threshold of the building. The Kapurála carries an earthen pot containing either cocoa-nut milk or water medicated with saffron leaves, and over which charms have been pronounced. On his arrival at the door he chants a song about a fire in Madurápura (Madura, South India) which was quenched by the goddess Pattini with milk. He then pours the fluid from the earthern vessel upon the lighted lamp and extinguishes it.

The Sinhalese use the expression "May milk be poured on him [or her]," when desiring to avert from some one an impending calamity, or to counteract a curse or prophecy of evil pronounced against him.

The idea of employing milk to quench the fire of an epidemic (typified by the flame of a lamp), and the idea of the deity pouring milk on an individual in order to protect him from malignant influences, appear to be somewhat analogous to the belief that milk alone will extinguish a conflagration kindled by the fire from heaven. F. M. WICKRAMASINGHA.

Colombo Museum, Ceylon, June 30.

The Red Spot on Jupiter.

AN observation with my 10-inch reflector, power 252, on August 5, 1888, showed the red spot passing the planet's central meridian at about 7h. 48m. Comparing this with the first observation I obtained of this object during the present opposition, viz. on December 28, 1887, at 20h. 23m., I find that the rotation-period of the spot during the 220d. 11h. 25m. elapsed during the period referred to was 9h. 55m. 40·34s. (533 rotations), which is slightly less than what I derived from the preceding opposition, 1886–87, when the figures were 9h. 55m. 40·5s. (609 rotations).

If the entire interval is taken between observations secured here on November 23, 1886, and August 5, 1888 (embracing 620½ days), I find that the mean rotation-period has been 9h. 55m. 39·7s. (1500 rotations). This clearly proves that the velocity of the spot is increasing, for at the opposition of 1885–86 the period was 9h. 55m. 41·1s. (659 rotations), and it had been increasing since 1879, when it was only 9h. 55m. 34s. The inference now seems tenable that its accelerated motion may so reduce the rotation-period in a few years that it will return to the rate it had in 1879. There is also great probability that the spot is affected by cyclic variations, the period of which may be determined by further observations.

It is desirable to obtain views of the central passages of the red spot as late as possible in every opposition. A good telescope directed to the planet at the following times will show the spot very near its mid-transit:—

	h. m.		h. m.
Aug. 12	8 36	Sept. 8	6 2
17	7 45	15	6 51
24	8 34	20	6 0
29	7 43	27	6 49
Sept. 3	6 53	Oct. 2	5 58

The low position of Jupiter during the present year has somewhat hindered the successful observation of his more delicate features, and during the next opposition of 1889 the planet will be in 23° S. declination, so that the study of his surface ought to be undertaken in southern latitudes, where the conditions are more favourable. W. F. DENNING.

Bristol, August 6.

Circles of Light.

THE appearance described below was visible in Penrith and the surrounding district on Thursday, the 2nd inst., from 5 p.m. nearly till sunset. Round the sun as centre, at a distance of about 28°, about three-quarters of a circle of light were visible, the lowest quarter being absent. About a quarter of a circle of equal size touched this circle at its highest point. In the region of contact of the circles a space about 4° long and ½° broad seemed common to the two circles, as if they there overlapped, and this part was very bright, and bordered with red on the side towards the sun. The remaining parts of the circles were faint, and only to be seen when the disk of the sun was hidden by some obstacle; they were about ½° wide.

EDMUND CATCHPOOL.

Westleigh, Weston-super-Mare, August 6.

Michell's Problem.

THE issue of NATURE of July 19 (p. 272) contains a communication from Mr. Sydney Lupton on "Michell's Problem." I regret the author has not seen my paper on the same subject published in the *Philosophical Magazine*, November 1887, "On Random Scattering of Points on a Surface." The objections put forward by the late Prof. Forbes to the argument of Michell concerning the physical connection of double stars are there analyzed, and it is shown that the experiments by which Prof. Forbes assumed to invalidate it are on the contrary a very decisive experimental proof for and illustration of this argument. Mr. Lupton says, "The probability of exactly uniform distribution is *nil*. Michell, however, seems to assume this probability to be 1, or certainty." I fully agree with the former part of the statement. But never did Michell assume the obviously erroneous view on the distribution of stars ascribed to him by Mr. Lupton in the letter. It is true that it is a common error—not only of the ἀγεω μετρητοὶ—to confound random scattering with uniform distribution, but Michell has not fallen into this error.

London, August 3. JOSEPH KLEIBER.

Cloud Electric Potential.

UNDER the above heading, in NATURE of July 19 (p. 269), which has just come into my hands, Mr. E. Douglas Archibald criticizes a statement of mine in Part III. of "Deschanel," respecting electrified drops of water in a cloud. The following is the statement:—

"The coalescence of small drops to form large ones, though it increases the electrical density on the surfaces of the drops, does not increase the total quantity, and *therefore cannot directly influence the observed potential.*"

At the word "therefore" I give a reference to a previous section, in which it is shown that the potential at a point is the sum of all the quotients q/r, q denoting an element of the electricity to which the potential is due, and r the distance of this element from the point in question. Since the coalescence of drops is without effect on the value of each q and its corresponding r, it cannot affect any one of the quotients q/r, whose sum constitutes the potential.

Mr. Archibald's criticism is:—

"Surely this entirely omits the fact that the capacity of a sphere is equal to its radius, and thus, in the case of eight equal spheres coalescing into one, not merely would the density be doubled, but the potential of the *same quantity* would be increased *four times.*"

This criticism rests on two false assumptions:—

First, that the potential of a drop depends on its own charge only, and can therefore be computed by dividing its charge by its radius.

Secondly, that the potential of the drops (which on this supposition would be very different from the potential at a point midway between two drops) can be identified with "the observed potential." J. D. EVERETT.

Cushendall, Co. Antrim, August 3.

THE ABSORPTION SPECTRA OF CRYSTALS.

ALL who are interested in the difficult work now going forward in so many chemical laboratories, in connection with the nature and constitution of those most complex mixtures known as "rare earths," and who recognize the extremely important influence which the solution of this subject must exert upon the very basis of our modern chemistry, will gladly welcome a new and exquisite means of investigation which M. Becquerel has recently brought to light.

As the reward of a most exhaustive study of the changes which are brought about in a beam of light by its passage through a crystal, M. Becquerel has discovered the key by means of which he is enabled to interpret the subtle indications which the issuing rays afford as to the nature of the molecules among which they have been threading their way. It appears at first sight more than wonderful that these delicate indications can have led to precisely the same weighty conclusions as those arrived at from the renowned physico-chemical researches of Auer von Welsbach, Lecoq de Boisbaudran, Demarçay, Soret, Crookes, and Krüss and Nilson. Yet such indeed is the case, and it even appears likely that the new method may be carried still further into the region beyond that which has up to the present been reached by these experimenters.

In order to explain the nature of this discovery, it will be necessary to describe the experimental steps which have led M. Becquerel towards it. In the year 1866 Bunsen found what now appears to be the germ of a great principle—that when a crystal of the sulphate of the substance didymium, now known to be a most complex mixture, was traversed by a beam of plane-polarized light vibrating at an angle of 20° to the horizontal diagonal of the crystal, the absorption spectrum was slightly different from that which was obtained when the ray was polarized in a plane at right angles. This observation did not attract much attention at the time, it being considered merely as a curious manifestation of the phenomenon of pleochroism.

Sorby, however, in 1869 again reopened the question, having found that in zircons the ordinary and extraordinary rays presented different bands of absorption. Since that time Becquerel himself has shown that the same applies to all birefractive crystals which give absorption spectra.

With so much premised, we are now in a position to consider the main results of this more recent investigation. They may be very briefly summarized as follows:—

(1) The bands in the absorption spectra of all crystals have fixed positions: the intensity alone varies with the direction of propagation of the light.

(2) In most crystals, the principal directions of absorption coincide with the directions of optical elasticity.

(3) In certain crystals the directions appear to be very different for different bands, but they always remain subject to the conditions imposed by the crystalline symmetry; thus in monoclinic crystals one of the principal axes of absorption always coincides with the axis of symmetry, and the other two rectangular axes are situated in the plane of symmetry.

Hence it appears to be a fact that the absorption of luminous radiations of fixed wave-length admits of three directions of symmetry. These directions appear generally to coincide with the principal directions of optical elasticity, with the exception of certain remarkable anomalies in particular crystals. Here, however, is the whole gist of the matter. Why these anomalies? Just as from a consideration of the deviations from Boyle's law physicists have learned how to measure the size of those wonderfully minute entities familiar to us as molecules, so has M. Becquerel extracted a most important principle out of the anomalies to the law of absorption in crystals.

It appears probable that absorption may be due to a physical phenomenon dependent upon the intermolecular movements. The intimate relation between phosphorescence and absorption, notably in the compounds of uranium and certain of the rare earths, appears to show that in solids and liquids the radiations absorbed are those which vibrate in unison with the molecular movements. This conception is in fact nothing more than an extension to solids and liquids of the well-verified law of the absorption by incandescent vapours.

As the molecular elasticity varies in different directions in crystals not isotropic—that is, not belonging to the cubic system—so will the absorption vary; and if, in two isomorphous substances, the directions of molecular elasticity do not exactly correspond, the directions of different absorption in the two substances will vary in like manner. Now it is quite true that many crystals of isomorphous substances—that is to say, substances of analogous chemical constitution crystallizing in similar forms—have their optic axes unequally inclined.

If we crystallize two such substances together, in gradually increasing proportions of one of them, we find that the angle between the optic axes in the mixed crystals diminishes progressively until it reaches zero, after which the two axes again diverge in a plane perpendicular to their original plane. Thus can we cause the influence of each in turn to preponderate.

Each chemical substance therefore exerts its own influence, and the molecules retain the optical properties which they manifest when the substance crystallizes alone. Hence the propagation of luminous waves is the resultant of the actions which each of the molecules composing the crystal exerts upon the luminous vibrations. If the directions of absorption do not coincide with the axes of optical elasticity, it indicates the presence of molecules of different substances in the crystal. From these considerations it will be evident that the anomalies are probably due to the coexistence in the same crystal of different matters, geometrically isomorphous, but optically unlike, and which from the absorption point of view behave as if each were alone. The use of the spectroscope will therefore enable us to recognize the individuality of differently absorbing molecules in the molecular groupings, which other optical methods cannot indicate; for the absorption due to one molecule is independent of that of a neighbouring molecule, while the phenomena of refraction only show resultant effects.

Further, as experiment shows that in most crystalline substances the principal directions of absorption coincide with the principal directions of optical elasticity, and as it is probably right to assume that each molecule is subject to the same laws as the whole of the crystal, there is no reason to suppose that the directions of symmetry should be different in the molecule and in the crystal, provided the latter presents no optical anomaly. One can therefore assume that the principal directions of absorption in the molecules themselves coincide with their axes of optical elasticity, and that in mixed crystals the anomalous directions of absorption indicate the direction of the optic axes of the different absorbing substances. If this is really the cause of the anomalies in the direction of certain bands, each group of anomalous bands ought to belong to different substances, of which the existence in the crystal is thus revealed.

To prove the truth of this beautiful theory, M. Becquerel points out the significant fact that among the substances which he finds to be characterized by anomalous bands, several have been separated chemically into their components.

We have, therefore, in the observation of anomalous directions of absorption a new method of spectral analysis, a method of determining in a crystal the presence of isomorphous substances, optically dissimilar. We can even go further still, and recognize the existence of different substances, although they may not manifest anomalous directions of absorption. For, suppose the same bands are noticed to occur in the spectra of several crystals; if in one of these crystals two bands become maxima or minima at the same time for the same direction of vibration, and if in another crystal one of them disappears for the direction which renders the other a maximum, one may conclude that the bands are due to two different molecules.

This new method of analysis appears to be specially suitable for use in unravelling the mystery of the constitution of the rare earths. If, as seems now almost certain, they consist of the oxides of a large number of elementary substances, the difficulty experienced in separating them points to the fact that these constituent oxides must resemble each other closely. It is therefore most probable that their salts will be isomorphous, and the crystals of these salts may consequently be expected to give absorption spectra of great interest in the light of the foregoing theory. M. Becquerel has therefore subjected the crystalline salts of didymium to the test of experiment, with the important result that several substances have been detected which chemists have recently isolated chemically; and also new substances have been identified as constituents, of which chemical methods have not as yet revealed the presence.

It will be remembered that Auer von Welsbach, by fractional crystallization of the double nitrates of didymium and ammonium, obtained two solutions—one possessing a green colour, showing almost exclusively the three bands $\lambda = 482$, 469, and 445, and which he termed praseodymium; the other a red solution, giving the other bands of the didymium mixture except $\lambda = 475{\cdot}5$, which received the name neodymium. The study of the absorption spectrum of crystals of sulphate of didymium now shows that the two groups $\lambda = 483{\cdot}6$–$482{\cdot}2$ and $\lambda = 471{\cdot}5$–470, which have anomalous directions to a remarkable extent, are characteristic of praseodymium, while most of the bands of neodymium have directions quite different. Again, on examining these same groups belonging to praseodymium in the crystals of double nitrate of didymium and potassium, it is noticed that the bands which appear to have the same principal directions in the sulphate have in the double nitrate directions quite different, characterizing two distinct substances. Later experiments by Demarçay have indeed shown the possibility of chemically isolating two constituents—one exhibiting the band $\lambda = 469$, the other giving the bands of praseodymium.

Hence the new method proves a most valuable test of the accuracy of chemical work. In multiplying the observations, M. Becquerel concludes that didymium is, as expected, a mixture of a large number of substances, chemically different; among the identified constituents are almost all that have been already chemically isolated, and very probably others, notably one substance which is characterized by the band $\lambda = 571{\cdot}7$.

A remarkable confirmation of this new law of crystal absorption was obtained in the following way. When a crystal of the sulphate or nitrate of didymium is dissolved in water, the spectrum of absorption of the solution presents curious differences from that of the crystal. Certain bands have remained permanent, but others are displaced, and some have entirely disappeared. This is readily explained if one admits that the crystal consisted of a mixture of compounds unequally acted upon by water. The most interesting fact, however, is that the bands which manifest these variations are precisely those which in the crystal present the anomalies.

In conclusion, we see that by the employment of this new method of analysis we are enabled, without destroying the crystal, as is necessary in chemical analysis, to recognize the presence of different chemical molecules; and as we obtain three distinct spectra from the three directions of optical elasticity, the method is one of extreme sensibility. Every investigator likes to see his work confirmed, and in this most difficult field of the rare earths we cannot have too many confirmations. The more points of the compass from which we approach it the better, for we are sure then of surrounding and finally of grasping the truth itself, in all its grand simplicity.

A. E. TUTTON.

THE NEW VEGETATION OF KRAKATÃO.

THE great volcanic eruption of Krakatão in August 1883 will be fresh in most memories. It was at one time reported that the island had totally disappeared, but this was not so. Previous to the eruption, however, it was covered with a luxuriant vegetation, no trace of which existed after the event.

Dr. M. Treub, the Director of the Botanic Garden at Buitenzorg, Java, when at Kew last year informed the writer that he had visited the island the previous year, and intended publishing the results of his botanical investigations. This he has now done,[1] and as the derivation of insular floras is a subject of great interest to many persons, the substance of Dr. Treub's communication deserves a place in NATURE.

The existing portion of Krakatão Island is about three miles across, and has the form of a mountain rising out of the sea. On one side it is nearly perpendicular almost to the summit of the peak, which has an altitude of about 2500 feet, and on the other it presents a steep slope. It is situated ten miles distant from the Island of Sibesie, the nearest point where there is terrestrial vegetation; twenty miles from Sumatra, and twenty-one miles from Java. Verlaten and Lang Islands, lying much nearer Krakatão, were utterly desolated and denuded of their vegetation by the great catastrophe, and were still absolutely bare in 1886.

With regard to the total destruction of vegetable life in the island, Dr. Treub asserts that there can be no doubt:

[1] *Annales du Jardin Botanique de Buitenzorg*, vii. pp. 213–23, with a sketch map.

the most durable seed and the best protected rhizome must have lost all vitality during the intense heat, and not a germ was left. The whole island from the summit of the peak down to the water's edge is now covered with a layer of cinders and pumice stone, varying from one to sixty metres in thickness. Furthermore, the possibility of the new vegetation having been conveyed thither by man is out of the question, because the island is uninhabited, uninhabitable, and difficult of access.

Therefore, the present vegetation must be due to other agencies, of which three different ones may have operated —namely, winds, waves, and birds.

Now, as to the composition of the vegetation met with on Krakatão by Dr. Treub in June 1886, nearly three years after the eruption, the bulk consisted of ferns with isolated plants of Phanerogams, both on the shore and on the mountain itself. Eleven species of ferns were collected, and some of them were already common. They are all species of wide distribution, and it may be of interest to give their names: *Gymnogramme calomelanos, Acrostichum scandens, Blechnum orientale, Acrostichum aureum, Pteris longifolia, Nephrolepis exaltata, Nephrodium calcaratum, N. flaccidum, Pteris aquilina, P. marginata, and Onychium auratum.*

It is not at all surprising that the spores of the foregoing and many other ferns should have been carried to the island by winds; but, as Dr. Treub remarks, it is almost incomprehensible that they should grow under such extraordinarily disadvantageous conditions. Chemically and physically the volcanic matter covering the island is as sterile as could well be, yet the prothallia of ferns readily developed. A closer investigation, however, revealed the fact that ferns were not the first organisms in the new vegetation of Krakatão, the cinders and pumice-stone being almost everywhere covered with a thin coating of *Cyanophyceæ* (fresh-water Algæ) belonging to the genera *Lyngbya*, *Tolypothrix*, &c.,—altogether six species. The presence of these Algæ gives the surface of the soil a gelatinous and hygroscopic property, in the absence of which Dr. Treub doubts the possibility of fern-growth. Thus these microscopic organisms prepare the soil for the ferns, much as the latter provide the conditions under which the seeds of Phanerogams can germinate and grow.

The phanerogamic element (flowering plants) of the new vegetation consisted, on the shore, of young plants of *Calophyllum Inophyllum*, *Cerbera Odollam*, *Hernandia sonora*, *Scævola Kœnigii*, *Ipomœa pes-capræ*, a species of *Erythrina*, two species of *Cyperaceæ*, and *Gymnothrix elegans*. With the exception of *Gymnothrix elegans*, a common grass in Java, all the plants named are among those which take possession of newly-raised coral islands.

In the interior of the island, on the mountain itself, Dr. Treub discovered *Scævola Kœnigii*, *Tournefortia argentea*, a species of *Wollastonia*, a species of *Senecio*, two species of *Conyza*, *Phragmites Roxburghii*, and *Gymnothrix elegans*.

In addition to the foregoing Phanerogams, Dr. Treub observed on the sea-coast seeds or fruits of *Heritiera littoralis*, *Terminalia Catappa*, *Cocos nucifera*, *Barringtonia speciosa*, and *Pandanus*. These also are among the commonest sea-shore and coral island trees throughout the Malayan Archipelago and Polynesia.

A more interesting record of the processes of a new flora can hardly be imagined, especially that in relation to the preparation of the soil by microscopic sporiferous plants. Of course this is not a new discovery; but it is perhaps the first actual observation of the renewal of the vegetation of a volcanic island.

Dr. Treub intends visiting Krakatão again, and reporting fully on the progress of the new flora, and his report will doubtless be looked forward to with great interest.

W. B. Hemsley.

THE NON-CHINESE RACES OF CHINA.

A VALUABLE Report which has just been laid before Parliament contains an account of a journey made by Mr. Bourne, British Consular Agent at Chung-King in Szechuen province, through South-Western and Southern China, to study certain commercial questions in these regions. The journey lasted 193 days, and carried the traveller through the great provinces of Yunnan, Kwangsi, Kweichow, and Szechuen. Mr. Bourne was constantly brought into contact with various non-Chinese tribes inhabiting these provinces, and his Report contains a large amount of information respecting their language and habits. He also devotes a special appendix to them. He says that there is probably no family of the human race, certainly none with such claims to consideration, of which so little is accurately known as the non-Chinese races of Southern China, and he attributes this to the "perfect maze of senseless names" in which the subject has been involved by the Chinese. The "Topography of the Yunnan Province," published in 1836, gives a catalogue of 141 classes of aborigines, each with a separate name and illustration, without any attempt to arrive at a broader classification. To Mr. Bourne it appeared that before the tribes could be scientifically assigned by ethnologists, they must be reduced to order amongst themselves, and that something might be done in this direction by taking a short vocabulary and obtaining its equivalent in the dialect of every tribe met with, when a comparison would reveal affinities and differences. Accordingly he gives twenty-two vocabularies, containing the numerals up to 12, 20, 30, 100, 1000, father, mother, brother, sister, heaven, gold, hand, foot, sun, dog, horse, iron, &c.—in all, thirty-six words. In each case the date, place, the name by which each tribe calls itself, the name by which the Chinese know it, and the name by which it knows the Chinese, is given. A comparison of these vocabularies and a study of Chinese books lead him to the conviction that, exclusive of the Tibetans, there are but three great non-Chinese races in Southern China—the Lolo, the Shan, and the Miao-tsze. The vocabularies do not convey the whole evidence that these scattered people respectively speak the same language, for the Lolo, Shan, and Miao-tsze are all languages of the Chinese type that make up for poverty of sound by "tones"; the resemblance is much more striking to the ear accustomed to these distinctions of sound than when the words are written in English, when the similarity of tone is lost. Among the 141 tribes described in the Chinese topography of Yunnan, with short vocabularies of the principal dialects, there are very few, and those unimportant, that cannot be identified from the illustrations or letterpress as belonging to one or other of the three families or to Tibetan. As to the names of these families, Lolo is a Chinese corruption of Lulu, the name of a former chieftain of the people, who call themselves Nersu, and has come to stand for the people themselves. Shan is the Burmese term adopted by Europeans for the people who call themselves "Tai," "Pu-nong," &c. Miao-tsze, a Chinese word, meaning "roots," is confined by the more accurate to the aborigines of Kweichow and Western Hunan.

The Lolos were formerly called by the Chinese the "Tsuan barbarians," a name taken from one of their chiefs. They call themselves Nersu, and the vocabularies show that they stretch in scattered communities as far as Ssu-mao, and along the whole southern border of Yunnan. They are also said by the Chinese to be found on the Burmese frontier. In a topography of Momien, a town not far from Bahmo, in the extreme south-west of Yunnan, the following information is given about them, which is at least surprising:—"The old Tsuan (Lolo) of Mengshan do not die. When old, they grow tails, eat men, not distinguishing their own children, love the hills, fear the abodes of men, and run as strongly as wild beasts. The natives call them autumn foxes. But, still, they are not invariably to be found." Although it is not yet known where the Lolo came from, Mr. Bourne gives a notion of their present habitat. In the great bend of the Yangtsze, in 103° E. longitude, between that river and the Anning, the Lolo are at home; there they live in independence of China, under their own tribal chiefs and aristocracy. Thence they extend in a scattered manner as far north as Wen-chuan, in latitude 31° 15′ N., and longitude 103° 30′ E. To the west they extend to the Meikong; to the south they are found occupying here and there the higher ground, until the plateau breaks into the plain, and they extend eastward to Kweiyang. They seem to be more numerous as Taliang Shan, their present home, is approached, and they form much the largest part of the population of North-Eastern Yunnan and North-Western Kweichow. Mr. Bourne adds about thirty names by which different tribes of Lolo are known to the Chinese.

The Shans are not found north-east of the city of Yunnan, but they inhabit all the lower levels along the south Yunnan border; and from the city of Kwang-nan along Mr. Bourne's route to the frontier of Kweichow province, they form almost the whole population. They must have been masters of the Kwangsi province before the Chinese, as some of the Chinese official buildings in the province are said to have been erected on the sites of Shan palaces. It would be interesting, says Mr. Bourne, to know how the Shans reached Kwangsi, whether through Tonquin or across the Yunnan plateau. The Shans in Southern Kweichow are undoubtedly immigrants from Kwangsi, and did not cross the plateau. The Shan language is softer than Chinese or Lolo, with fewer gutturals and aspirates, and appears easy to learn. The numerals show a curious resemblance in sound to the Cantonese.

The Miao-tzse apparently are divided into a number of tribes speaking dialects of one language which is of the Chinese sort. They occupy at present Eastern Kweichow and Western Hunan, being very numerous in the south-east of the former province. They are known to the Chinese by a multitude of names, but always with the prefix Miao.

So far the appendix; but scattered throughout Mr. Bourne's elaborate report of his journey there are numerous interesting references to these non-Chinese races. Near Maling, in Southern Yunnan, on a tributary of the Yangtsze, he came on a sandstone bluff containing about twenty Mantzu caves. Most of the entrances, 3 to 4 feet square, are cut in the vertical cliff some 10 feet above the ground, so that they cannot be reached without a ladder. The face of the cliff is adorned in one or two cases by sculptures in relief, the most striking being a round human face. The valley was, no doubt, formerly the head-quarters of a Mantzu tribe, for some miles lower down the site of the castle of a chief is pointed out. The sculptured blocks that lie about bear witness to a considerable advance in civilization. The Lolos are described as having larger and more irregular features than the average Chinese; the colour of the skin seems much the same, but the eyes were deeper sunk. They are divided into three tribes, known as the black, white, and dry Lolos—a meaningless distinction, but corresponding apparently to a real tribal division. They believe in a future state of retribution, burn the dead, worship their ancestors with the sacrifice of an ox, and have no idols. Four pieces of brown paper were said to represent the potentialities of the other world, and three sticks of bamboo their ancestors. A special Lolo vocabulary, with the written characters, procured from a *perma*, or tribal sorcerer, in Ssu-mao, is carefully reproduced. This sorcerer was able to read his prayer-book, but not to explain what it meant. In his own opinion this was not important, as the ritual had been arranged between his ancestors and the gods, who knew very well what was meant so long as he read the right section and gave the characters their proper sound.

The Report it should be added contains numerous and comprehensive tables of meteorological observations and levels, although the rate of travelling prevented anything like a running survey being made.

THE BATH MEETING OF THE BRITISH ASSOCIATION.

THIS will be the fifty-eighth meeting of the British Association for the Advancement of Science. Twenty-four years ago—in 1864—the Association met at Bath under the presidency of the late Sir Charles Lyell. So many other names are now missing, that the retrospect is the reverse of cheering. Sir Roderick Murchison, Admiral Fitzroy, Dr. Whewell, Sir John F. W. Herschel, Sir David Brewster, Dr. William Farr, Prof. Fawcett, Dr. Livingstone, Capt. Maury, and Mr. Scott Russell, are only a very few of the numerous names of note that spring to the memory in connection with the last Bath meeting.

But if this is the retrospect, the prospect is in every way most satisfactory. Under the genial presidency of Sir Frederick Bramwell, with the aid of very efficient local officers, a thoroughly successful meeting may fairly be looked for. Bath has the advantage of a good position for railway facilities. It is not more than 2½ hours from London, 2 from Exeter, 1½ from Cardiff, 1½ from Birmingham, and 5½ from Manchester. The endeavours of the Local Executive Committee to obtain the issue of cheap tickets, as usual, have been crowned with success. As Bath—strangely enough—does not possess a Public Hall, a temporary building, to serve as reception-room and offices, is in course of erection, at a cost of some £700. The President's address, the evening discourses, and Sir John Lubbock's lecture to working men will be given in the Drill Hall.

It is unnecessary to say anything as to the fitness of Sir Frederick Bramwell for the office of President. The following are the Presidents of the Sections:—Mathematics and Physics, Prof. Fitzgerald; Chemistry, Prof. Tilden; Geology, Prof. Boyd Dawkins; Biology, Mr. Thiselton Dyer; Geography, Sir Charles Wilson; Statistics, Lord Bramwell; Mechanics, Mr. Preece; Anthropology, General Pitt-Rivers.

Bath itself is so well known as to need very few words. The old Roman Bath and its adjuncts, recently uncovered, with other remains, will of course excite general interest. Attention will also be given to the new buildings erected by the Corporation to meet the increasing demand for the Bath waters. On every side the city is surrounded by objects that will interest the geologist, the archæologist, and the lover of the picturesque. Provisional arrangements have been made for a set of excursions—half-day, on Thursday, September 6, and whole day on Saturday, September 8—to Stantonbury, Stanton Drew, Maes Knoll; Dyrham, Sodbury Camp, Bannerdown; Stourton, Pen Pits, Whitesheet, Longleat; Frome, Wells, Glastonbury, Cheddar, Chepstow, Tintern, Box Quarries, Cirencester, Severn Tunnel, Tytherington, Thornbury, Berkeley, Ebbor Gorge, Wookey, and elsewhere.

PROF. H. CARVILL LEWIS.

THE loss to the geological world by the death of Prof. Henry Carvill Lewis at the early age of thirty-four, and in the midst of his work, is greater than they may realize, as the more important of his results acquired during the last three years have not been fully published. When, in 1882, he began to study the glacial phenomena of

Pennsylvania, though numerous observations had been made on the direction of the striæ and the location of the moraines, in the northern part of the States, nothing had been attempted towards gathering the results into a consistent whole, or tracing the limits of the glaciation. In that year he succeeded in tracing a great terminal moraine from New Jersey to the Ohio frontier, and showing that beyond this line glaciation was absent, while within it the direction of the motion could be traced as well by the striæ as by the derivation of the boulders. Of the truth of these views he succeed in convincing almost all the American geologists who had studied the question. Fired by his success in interpreting the glacial phenomena of his native country, and believing that the same key might be found to unlock the mysteries of European glaciation, he practically threw up his position in Philadelphia, and devoted himself to the study of these phenomena in Great Britain. Devoting his summers from 1885 to the work, he visited—accompanied by his wife, whose active assistance he constantly enjoyed—almost every locality in Great Britain and Ireland where striæ had been recorded or moraines were likely to occur. To reduce the whole of the previous observations to order was a task he had not yet succeeded in completing, but which he boldly undertook, and to continue which he had once more landed in England. Important results were, however, already obtained, and at the British Association last year he gave English geologists the firstfruits, by presenting them with a map of England in which he had traced a great terminal moraine, as in America, on the north of which the striæ and the dispersion of the boulders indicated a continuous ice-sheet, while to the south the various glacial deposits were accounted for by extra-morainic lakes. He boldly advocated the view of the ice mounting up to the heights of 1100 feet in Moel Tryfaen and elsewhere, where the ice-sheet had crossed the sea, declaring that anyone who was acquainted, as he was, with the far greater results of ice-motion in Pennsylvania would have no difficulty in accepting this, and pointing out that these localities were everywhere on the line of the great terminal moraine. So startling a generalization could scarcely be accepted all at once, and there were many things to account for before the history even of this greatest ice-sheet could be considered complete. Had Prof. Lewis been spared to us, he was prepared to devote himself to the completion of this work. He has left a large mass of notes and drawings bearing on it, which must now wait for some Elisha capable of taking up his mantle. Every glacialist is no doubt more or less satisfied with the account he can give of the glacial history of his own district; but to the general geologist the whole has hitherto presented a chaos of conflicting histories, fit only to bewilder him. In the clear account given by Prof. Carvill Lewis of one great portion of that history, light seemed at last to dawn, and the hope was raised that glacial chaos would cease. This hope now seems quenched for a time.

Prof. Lewis was born in Philadelphia, November 16, 1853, and took his B.A. degree in 1873 in the University of Pennsylvania. From 1879 to 1884 he was a volunteer member of the Geological Survey of the State. In 1880 he was elected Professor of Mineralogy in the Academy of Natural Sciences, Philadelphia, and in 1883 Professor of Geology in Haverford College. His work was by no means confined to his glacial studies, the most important of his minor works being the discovery of the matrix of the diamond in an ultra-basic volcanic rock in contact with a carbonaceous tuff. The prediction that, if such was the origin of diamonds, they might be found in meteorites, had just been fulfilled in Russia; and he had lately visited a locality in Carolina, where the same conditions obtain, but had not proceeded further when he was stopped by death. During the last three years he spent his winters in Heidelberg, studying microscopic petrography with Prof. Rosenbusch.

Those who knew him personally, were charmed with the beautiful frankness of his nature, his love of truth, his invariable possession of a reason for what he said, and his total lack of pride or assumption of authority. They saw in him a type of what a genuine student of Nature should be, tempered and refined by general culture. Few who knew him at all but must feel they have lost a friend, and an example.

He married in 1882, and leaves a wife and one daughter.

*SONNET**

TO A HIGH SOPRANO

Accompanying herself on the Piano.

THE larks who sing at Heaven's high gate despair
To match thy notes so piercing-sweet and true
That, pure as in night's hour fresh-fallen dew,
Vouch thou art good, e'en as thou art most fair.
Why seek with gems to deck thy radiant hair,
Thy flashing, rushing, fingers to indue
With rubies' blaze or Opal's orient hue—
Thou canst in nobler wise thy worth declare.
Oft shall the rapt enthusiast in his cell
Intent on Nature's all-pervading clue
Pause, to bid Memory with her magic spell
Restore that heavenly, loved, lithe form to view
And in fond fancy hear thy voice anew
Till life to gladness breathes its last farewell.

New College, Oxford, July 20. J. J. S.

NOTES.

THE annual meeting of the British Medical Association was opened at Glasgow on Tuesday, the 7th inst. Prof. Gairdner, the President, delivered an address on "The Physician as Naturalist." Speaking of the methods of education necessary for the training of a physician, Prof. Gairdner urged that medical students do not at present receive adequate instruction in physics. "When we consider," he said, "how completely modern science has demonstrated the subordination of living bodies and physiological processes, not to a wholly detached set of laws termed vital, but to all the most elementary laws of matter; and, further, the correlation of all the physical forces throughout the universe, so that the living body and its environment act and react on each other throughout infinite space and time, it will be readily admitted, I think, that some kind of systematized instruction in physics, and not a mere elementary examination in mechanics, should be an essential part of an education with a view to the medical profession. And when we further consider that most of the great advances in medical diagnosis in the present day, through the stethoscope, microscope, laryngoscope, ophthalmoscope, sphygmograph, electricity as applied to muscle and nerve, &c., involve applications of pure physics which are neither remote from practice nor yet very easily mastered by the beginner; and that, in the case of electricity and other physical reagents, even heat and cold, &c., we are every day extending the domain of these sciences in therapeutics, and still more perhaps in preventive medicine and sanitary science, their claim for an extended recognition in teaching seems to be enormously enhanced. I am persuaded that in a very few years the physical laboratory will become an absolutely essential preliminary step in the education of the physician of the future, and that those who have not undergone this training will be hopelessly distanced in the race."

* In the next number of NATURE will appear the Original of this sonnet addressed *To a Young Lady with a Contralto Voice.*

THE Organizing Committees of Sections A and G of the British Association have arranged a joint discussion on lightning conductors, to be held at the Bath meeting in September. Mr. W. H. Preece, F.R.S., will open the discussion, and Prof. Oliver J. Lodge, F.R.S., willl defend the position he laid down this year before the Society of Arts.

AGREEABLY to a resolution of the International Congress of Hydrology and Climatology held at Biarritz, in October 1886, the second triennial session of the Congress will be held in Paris next year, at the beginning of October, in connection with the Exhibition there. The President of the Committee is M. E. Renou, Vice-President of the French Meteorological Society. A preliminary programme has been issued, setting forth the questions to be discussed under (1) scientific hydrology; (2) medical hydrology; and (3) climatology. The subscription of membership is 12 francs.

THE new Marine Biological Laboratory at Wood's Holl, Massachusetts, was formally opened on the day appointed, Tuesday, July 17. Several members of the Board of Trustees, a few students, and a half-dozen or more of guests were present, and spent the morning in examining the new building and its equipment, and in visiting the laboratories and aquaria of the United States Fish Commission. At two o'clock the whole party dined at Gardiner Cottage—the domestic head-quarters of the new enterprise—which a generous citizen of Wood's Holl, Mr. J. S. Fay, has kindly put at the disposal of the trustees. Shortly after three o'clock the Director, Dr. C. O. Whitman, delivered in the Laboratory an opening address upon the history and functions of marine biological laboratories, referring especially to the Penikese School and to Prof. Baird's labours in this direction. Prof. C. S. Minot then said a few words on behalf of the trustees. Some eight or ten students are already at work in the Laboratory; and *Science* says that the responses from colleges and from students make it certain that next year there will be at the institution a large and enthusiastic gathering of investigators and students in biology. The building, according to *Science*, appears to be admirably adapted to its purposes. It is plainly but strongly built, of wood, two stories high, and with a pitched roof. The roof and sides are covered with shingles, unpainted. There is a commodious and convenient basement under the western half of the building, intended for storage, for the safe keeping of alcohol, boats, oars, and the like. The lower floor of the Laboratory is intended for beginners, and for teachers and students who are learners but not investigators. The upper story is for investigators only. The equipment includes work-tables, specially designed, and placed before the large and numerous windows. Each student is provided with a Leitz microscope, a set of reagents, watch-glasses, dissecting pans, and the dishes and other things indispensable to good work. The Laboratory owns boats, dredges, nets, and other tools for collecting. A small library has been provided, and, under the progressive and efficient management of Dr. C. O. Whitman and Mr. B. H. Van Vleck, a season that promises to be highly successful, and most important in the history of American biology, has been auspiciously begun.

MR. HENRY O. FORBES, the New Guinea explorer, author of "The Naturalist in the Malay Archipelago," has been selected by the London Commission to succeed the late Sir Julius von Haast as Director of the Canterbury Museum, New Zealand.

SOME time ago a good deal of interest was aroused by a controversy as to the effects of light on water-colours. The Committee of Council appointed a Committee of artists to consider the subject; and Dr. W. J. Russell and Captain Abney were invited to investigate the scientific aspects of the question. A Blue-book has just been issued, containing the first report of these two gentlemen.

WE regret to record the death of Miss Glanville, who was well known in South Africa as the Curator of the Albany Museum, Grahamstown, Cape of Good Hope. This clever and accomplished young lady discharged her duties as Curator most conscientiously and ably, and did much to promote an interest in science in her native town and country.

A NEW gas, possessing some remarkable properties, has been discovered by Prof. Thorpe and Mr. J. W. Rodger, in the research laboratory of the Normal School of Science. It is a sulpho-fluoride of phosphorus of the composition PSF_3, and is termed by its discoverers thiophosphoryl fluoride. The best method for its preparation consists in heating pentasulphide of phosphorus with lead fluoride in a leaden tube. It may also be obtained by substituting bismuth fluoride for the fluoride of lead, the only difference between the two reactions being that the second requires a higher temperature than the first. Again, when sulphur, phosphorus, and lead fluoride are gently warmed together, an extremely violent reaction occurs, but if a large excess of the fluoride of lead be employed a tolerably steady evolution of the new gas occurs, the excess of the lead salt appearing to act as moderator. It is an interesting fact, throwing considerable light upon the constitution of the sulpho-fluoride, that it may be obtained by heating together to 150° C. in a sealed tube a mixture of the corresponding chloride—thiophosphoryl chloride, $PSCl_3$, a mobile colourless liquid—and trifluoride of arsenic. The simple exchange of chlorine for fluorine here brings about a striking physical change, from a highly refracting liquid to a colourless gas. And now for the remarkable properties of the gas. In the first place, it is spontaneously inflammable. If it be collected over mercury, upon which it exerts no action, in a tube terminating above in a jet and stopcock, and the latter be slowly turned so as to permit of its gradual escape, the gas immediately ignites as it comes in contact with the air, burning with a greenish-yellow flame tipped at the apex with blue. If, however, a wide tube containing the gas standing over mercury be suddenly withdrawn from the mercury trough, the larger mass of gas ignites with production of a fine blue flash, the yellowish-green tint again being observed as the light dies away. Thiophosphoryl fluoride is readily decomposed by the electric spark with deposition of sulphur. If a quantity contained in a tube over mercury be heated for a considerable time, complete decomposition occurs, sulphur and phosphorus both being deposited upon the sides of the tube and gaseous silicon tetrafluoride left. From a spectroscopic examination, dissociation was shown to occur at the lowest temperature of the electric spark. The gas is slowly dissolved by water, and appears to be somewhat soluble in ether, but alcohol and benzene exert no solvent action upon it. Finally, the colourless, transparent gas was reduced to a liquid, somewhat resembling the sulpho-chloride, by means of Cailletet's liquefaction apparatus.

A VOLCANIC eruption, which began on August 3, in the Island of Vulcano, one of the Lipari Group, is said to have done an immense amount of injury. The greatest damage has been caused on the property of an English company under the management of Mr. Harleau, the estate being completely destroyed.

WE have received the Year-book of the Meteorological Observations of the Observatory of the *Madgeburg Journal* for the year 1886, being the fifth of the series. It contains observations taken three times daily, with means and monthly summaries according to the international scheme, hourly observations of the self-recording instruments, and fac-similes of the sunshine records; also additional observations, such as earth-temperature, evaporation, underground water, &c., as in previous years. The principal alteration is the omission of the continuous barograms: these are now given only in cases of special interest, owing to the expense of the reproduction. We have already expressed our approval of this method of dealing with continuous records, as opposed to the costly reproduction of the curves in their entirety.

The "Annuaire" of the Municipal Observatory of Montsouris for the year 1888, just published, a volume of 612 pages, 18mo, contains a large amount of useful information, relating to the meteorology of Paris, and the microscopical examination of the organisms in the air and water. The report shows that the site of the Observatory is favourable for determining the climate of Paris with exactitude; some of the thermometric differences between Paris and Montsouris are very marked. The amount of rainfall also is somewhat greater at Montsouris, owing probably to better exposure than at Paris, but the differences are not greater than are frequently found with gauges placed near each other. The tables contain monthly means of temperature from the year 1806, and of rainfall since 1689; the values prior to 1873 are those referring to Paris. Self-registering thermometers were first used in 1835; up to this date the minimum temperatures were taken as the readings at sunrise, and the maximum readings, as those at 3 p.m. The yearly extremes of temperature date back to 1699.

We learn from *Science* that the famous Bahia or Bendego meteorite, described by Mornay and Wollaston in the Philosophical Transactions for 1816, and by Spix and Martius in their "Travels in Brazil," was landed in Rio de Janeiro on June 15, and is now in the collection of the Brazilian National Museum. The transportation of this great mass of iron, whose weight was variously estimated from six to nine tons, and which has been found to weigh 5361 kilogrammes, was rendered possible by the recent completion of a line of railroad passing within 115 kilometres of the Bendego Creek, where it has lain since the unsuccessful attempt to remove it to Bahia in 1785. Credit for the removal of the meteorite is due chiefly to Chevalier José Carlos de Carvalho, who gratuitously took charge of the technical part of the operation, and to Baron Guahy, who paid all the necessary expenses. The Brazilian Government also cordially associated itself with the undertaking. After about three months spent in preparing material and in studying the route to be traversed, the march began on November 25, 1887, and the meteorite was placed on the railroad on May 14 of the present year. A road had to be opened for this special purpose, as those existing in the region are only mule paths; and over one hundred streams, one with a width of 80 metres, had to be crossed by temporary bridges. The route lay over several chains of hills and one mountain range, in which an ascent of 265 metres had to be overcome with a grade of 32 per cent.

The Canadian Institute, Toronto, has issued a "sociological circular," asking co-operation in the task of collecting trustworthy data concerning the political and social institutions, customs, ceremonies, &c., of the Indian people of the Dominion. Suitable papers upon the topics indicated will be published in the Institute's Proceedings. The Canadian Pacific Railway carries, free of charge, packages intended for the Institute's Museum, which is open daily.

The Kew Bulletin for the months of November 1887 and January 1888 supplied valuable information, derived from official sources, respecting the capabilities of certain colonies for the production of fruits. The Bulletin for November 1887 was wholly devoted to a comprehensive report on the fruits of Canada. The Bulletin for January 1888 was devoted to reports furnished by their respective Governments on the fruits of Victoria, South Australia, Western Australia, Tasmania, New Zealand, Cape Colony, and Mauritius. In the Bulletin for August, just issued, the publication of such reports is continued. A summary of information is presented relating to the fruit productions and fruit resources of the West Indian colonies —Jamaica, Bahamas Islands, Barbados, St. Lucia, St. Vincent, Grenada, Tobago, Trinidad, and British Guiana.

The Report of the Comptroller-General of Patents, Designs, and Trade Marks for the past year states that the total number of patents applied for was 18,051, being an increase of about 900 on the year before; of designs, 26,000 as against 24,000 of the preceding year; and of trade marks, 10,586, being a decrease of 91 from the preceding year.

The American Statistical Association publishes some interesting figures on the amount of water-power employed in the United States. In 1880 there was a total water-power equal to 1,225,379 horse-power used for manufacturing-purposes, this being 35·9 per cent. of the total power thus employed in the States. The annual value of the water-power thus utilized is set down at 24,000,000 dollars. The New England States alone use 34·5 per cent. of the whole water-power of the country, and altogether the Atlantic States use over three-fourths of the whole.

According to a return of the Board of Trade on sea-fisheries in the United Kingdom, the total amount of fish landed on the English and Welsh coasts, exclusive of shell-fish was, in 1887, about 301,000 tons, of the value of about £3,780,000. Shell-fish taken in that year were of the value of £324,000. For the year 1886 the figures were—fish landed, 320,000 tons, of the value of £3,688,000, and shell-fish of the value of £269,000. Thus, while there was a decrease in weight of about 19,000 tons, there was an increase in value of about £90,000, and in the shell-fish an increase of £55,000.

In a Report of M. Renduel to the French Minister of Marine, he attributes the gradual decline of the sprat-fisheries of France to the methods hitherto pursued in fishing. The sprat seine-net, he says, is most destructive. When thrown out fully, as is usually the case, and then towed towards the shore, it drags the bottom over an enormous area, and brings to land not only the sprats, but shoals of other fish not yet fully developed, and quite unsalable. The French newspapers say, with a little pardonable exaggeration perhaps, that thousands of cubic metres of winter fry, which would give in summer millions of cubic metres of edible fish, have been used as manure in the fields, in order to force grass and cereals. So far has this been carried, that the non-migratory fish are almost exterminated in many places.

In the Report of the British Consul at Tunis to the Foreign Office, he says that the sponge fishery is a very important branch of industry in that country. There are in all about 400 Greeks, 500 Sicilians, and 1400 natives engaged in the pursuit. The diving apparatus was formerly in use, but it has given way to a kind of dredging instrument similar to that used in the oyster fishery. The same Report says that the tunny fishery is a monopoly of the State. The fish enter the Mediterranean in the spring, and one body of them strikes the coast at Cape Bon. Here the net-fishing begins. The boats gather around the nets, and the fish are harpooned and dragged into the boats, as many as 600 being thus frequently taken in one haul. They are then cut up and preserved in olive-oil, packed in tins of various sizes, and soldered up. About three-fourths of the fish are thus treated, and sent away to Italy, where they meet a ready sale. The rest are either eaten fresh, or salted and sent away to Malta or Sicily. Between 200 and 300 men are engaged in this work, which is of the annual value of £20,000.

A very rare fish, *Plagyodus* (*Alepisaurus*) *ferox*, has just been caught in the Karlsöfjord, in Iceland. It is 5 feet 9 inches long, with small shark-like fins, those on the back being about a foot in length. The head is pointed, and the teeth long and sharp. It appeared to lie asleep on the surface of the water, and a fisherman caught it by its tail, when it attempted to bite him. Prof. Lütken states that hitherto only three specimens of this fish have been caught, viz. one at Madeira, one in Greenland, and one previously in Iceland. It is believed that this is the mysterious fish the *fax-ál, i.e.* the eel with a mane, of which the Faroe fishermen stand in such awe.

THE Assistant Superintendent of the Forest Department of Penang has tried the raising of mahogany-trees from seeds, but with what success is not yet known. He also tells us that a trial venture in cultivating patchouli has proved very successful. Experiments in growing olives, oranges, citrons, &c., have proved encouraging, and trials with European vegetables show that tomatoes, carrots, lettuce, onions, celery, &c., can be successfully cultivated in the Straits Settlements.

IN "The Fodder Grasses of Northern India," just published at Roorkee, Mr. J. F. Duthie gives an instructive account of the more important kinds of grasses that are used in the plains of Northern India either for fodder or for forage. Several of the plains species extend up to considerable elevations on the Himalaya, but Mr. Duthie has omitted all mention of those which are exclusively Himalayan. The area of country to which the work refers, and which coincides with that over which his botanical researches generally will in future be conducted, extends from the north-west frontier, and includes the Punjab, the North-West Provinces, and Oudh, Sindh, Rájputána, Central India, and the Central Provinces.

A NEW edition of the Catalogue of Lewis's Medical and Scientific Library has just been issued. It includes a classified list of subjects, with the names of those authors who have dealt with them.

THE first University of Siberia has just been opened at Tomsk. It has for the present only one Faculty, that of Medicine. How urgently necessary the establishment of this Siberian Faculty of Medicine has become may be seen from some figures sent to the *Times* the other day by its St. Petersburg Correspondent. The practice of one doctor is supposed to extend over each of the following districts, with their respective populations :—Tobolsk, 129,785 square versts, 110,323 inhabitants; Akmolinsk, 87,833 square versts, 80,062 inhabitants; Semipalatinsk, 85,705 square versts, 100,225 inhabitants. In short, there are only twenty-two doctors over an enormous territory of 2,815,547 square versts.

IN the article "Lord Armstrong on Technical Education," in our last issue, an unfortunate slip occurs at p. 314, in the second column, which destroys the force of the argument: £74,000, not £24,000, should have been stated as the sum which it was proposed to spend on the erection of a new chemical department of the Zurich Polytechnicum.

THE additions to the Zoological Society's Gardens during the past week include a Purple-faced Monkey (*Semnopithecus leucoprymnus* ♂) from Ceylon, presented by Mr. Martin J. Cole; a Rhesus Monkey (*Macacus rhesus* ♂) from India, presented by Mr. Reginald S. Knott; three Black-eared Marmosets (*Hapale penicillata*) from South-East Brazil, presented by by Mr. J. A. Deintje; a Chipping Squirrel (*Tamias striatus*) from North America, presented by Mrs. Matveiff; a Common Squirrel (*Sciurus vulgaris*) British, presented by Mr. R. Grant Watson; a Tayra (*Galictis barbara* ♂) from South America, presented by Mrs. J. H. Pollard; a Lesser Sulphur-crested Cockatoo (*Cacatua sulphurea*) from Moluccas, presented by Mr. J. Wolfe Barry; a White-backed Piping Crow (*Gymnorhina leuconota*) from Australia, presented by Miss Alice Rutherford; a Herring Gull (*Larus argentatus*), British, presented by Mrs. Huthwaite; an Ashy-headed Gull (*Larus cirrhocephalus*), a —— Bittern (*Butorides* ——) from South America, presented by Dr. A. Boon, C.M.Z.S.; a Common Kestrel (*Tinnunculus alaudarius*), British, presented by Mr. W. A. W. Jones; a Smooth Snake (*Coronella lævis*) from Hampshire, presented by Mr. E. G. Meade-Waldo; a Rhesus Monkey (*Macacus rhesus* ♂) from India, a Common Boa (*Boa constrictor*) from South America, an Æsculapian Snake (*Coluber æsculapii*) from Langenschwalbach, Germany, deposited.

OUR ASTRONOMICAL COLUMN.

ENCKE'S COMET.—Encke's comet was picked up at the Cape Observatory on August 3, its place at 6h. 10m. 56·6s. being recorded as R.A. 12h. 12m. 59s.; Decl. 17° 27′ 46″ S. This compares with Dr. Backlund's ephemeris (*Astr. Nach.*, No. 2843) as follows: O − C; R.A. + 4m. 43s.; S. Decl. + 34′ 52″. The ephemeris for the next few days runs as below:—

For Berlin Midnight.

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Aug. 10	13 9 20	23 30·2 S.	0·0038	9·8790	0·69
12	13 25 34	24 56·1	0·0176	9·8896	0·62
14	13 41 21	26 11·4	0·0308	9·9014	0·55
16	13 56 36	27 17·2	0·0435	9·9142	0·49
18	14 11 21	28 13·7	0·0556	9·9275	0·44
20	14 25 28	29 1·7	0·0673	9·9415	0·39
22	14 38 59	29 41·9	0·0785	9·9559	0·35
24	14 51 52	30 15·4	0·0893	9·9708	0·31
26	15 4 10	30 42·7	0·0997	9·9857	0·27
28	15 15 54	31 5·3 S.	0·1097	0·0009	0·24

The brightness at discovery is taken as unity.

THE MASS OF TITAN.—The values which have been deduced for the mass of Titan by different astronomers showing a wide diversity, Mr. G. W. Hill has undertaken, in *Gould's Astronomical Journal*, No. 176, a new determination of this constant from the influence of Titan on the motion of Hyperion. Assuming Hyperion to be in opposition to Titan, at the same time that it is in perisaturnium, then, at the end of the half-synodic period—viz. 31·8182806d.—it would be in conjunction with Titan; and but for the action of Titan, ϕ, the angle the radius-vector makes with the direction of motion, would = 90° 8′ 51″·85. But the influence of Titan reduces this to a right angle, and this effect may be used to discover the mass of that body. Computing the motion of the line of apsides during the half-synodic period from opposition to conjunction, all powers but the first of the disturbing force being neglected, the value of $\Delta\varpi$ corresponding to the argument 31·81828d. was found to be −2634″ instead of −5898″, as given by observation. The mass, therefore, of Titan would require to be changed from 1/10,000, the value assumed at first, to 1/4466. The eccentricity of the orbit of Titan, 0·028, had been neglected, and that of Hyperion taken as 0·1. With this better value for Titan's mass, the path of Hyperion from opposition to conjunction is then traced by mechanical quadratures, no powers of the disturbing forces being neglected. The two unknowns to be determined were—the velocity with which Hyperion should start from opposition, and the mass of Titan; and the two determining conditions—that the conjunction should take place 31·81828d. after opposition, and that Hyperion must be then moving at right angles to its radius-vector. The resulting mass is found to be 1/4714, and the osculating elements of Hyperion at opposition—

$$\text{Daily } n = 60963''\cdot 23942$$
$$\log a = 0\cdot 0823532$$
$$e = 0\cdot 0994706$$

Prof. Newcomb, in one of the "Papers for the Use of the American Ephemeris," vol. iii., part 3, has also described the perturbations of Hyperion arising from the action of Titan, and deduced the mass of Titan as 1/12,500, but Mr. Hill points out hat this value should have been divided by 3. M. Tisserand's value from a similar inquiry, 1/10,750 (*Comptes rendus*, tome ciii. No. 9), stands out in strong contrast with Prof. Hill's result; but Prof. Ormond Stone, on the other hand, who had obtained a larger result, has more lately, after correction of an error in his investigation, brought it down to a value closely according with that of Prof. Hill.

Assuming the diameter of Titan as 0″·75—the value given independently by Schrœter, Mädler, and Struve—the density of the satellite would be about one-third that of the earth. Pickering's diameter, deduced from photometric observations of the satellite on the assumption that its albedo was equal to that of the primary, would involve a density nearly four times that of the earth. It would seem clear, therefore, that Titan possesses a much greater density than Saturn, but that its surface is less highly reflective.

NAMES OF MINOR PLANETS.—Minor planet No. 276 has been named Adelheid, and No. 278 Paulina.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *AUGUST* 12–18.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 12

Sun rises, 4h. 43m. ; souths, 12h. 4m. 43·8s. ; sets, 19h. 26m. : right asc. on meridian, 9h. 30·2m. ; decl. 14° 47′ N. Sidereal Time at Sunset, 16h. 53m.

Moon (at First Quarter August 14, 17h.) rises, 10h. 14m. ; souths, 16h. 8m. ; sets, 21h. 49m. : right asc. on meridian, 13h. 34·5m. ; decl. 4° 16′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	3 29	11 18	19 7	8 43·5	19 18 N.
Venus ...	5 29	12 41	19 53	10 6·4	13 11 N.
Mars ...	12 33	17 9	21 45	14 35·6	16 38 S.
Jupiter ...	13 50	18 13	22 36	15 40·0	18 50 S.
Saturn ...	3 51	11 31	19 11	8 56·4	18 1 N.
Uranus...	9 50	15 28	21 6	12 53·7	5 4 S.
Neptune..	22 50*	6 37	14 24	4 1·8	18 59 N.

* Indicates that the rising is that of the preceding evening.

ug.	h.	
13	21	Mars in conjunction with and 6° 49′ south of the Moon.
14	4	Mercury in conjunction with and 0° 39′ north of Saturn.
15	0	Jupiter in conjunction with and 4° 7′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
λ Tauri...	3 54·5	12 10 N.	Aug. 12,	3 12	*m*
			,, 16,	2 4	*m*
R Comæ	11 58·5	19 25 N.	,, 12,		*M*
S Virginis	13 27·2	6 37 S.	,, 18,		*m*
δ Libræ	14 55·0	8 4 S.	,, 16,	23 0	*m*
U Coronæ	15 13·6	32 3 N.	,, 15,	23 35	*m*
U Ophiuchi...	17 10·9	1 20 N.	,, 14,	2 2	*m*
			,, 14,	22 10	*m*
R Scuti	18 41·5	5 50 S.	,, 15,		*m*
β Lyræ...	18 46·0	33 14 N.	,, 13,	3 0	m_2
R Lyræ	18 51·9	43 48 N.	,, 18,		*m*
T Sagittarii...	19 9·8	17 10 S.	,, 12,		*M*
η Aquilæ	19 46·8	0 43 N.	,, 14,	2 0	*m*
S Sagittæ	19 50·9	16 20 N.	,, 16,	23 0	*m*
X Cygni	20 39·0	35 11 N.	,, 18,	2 0	*m*
T Vulpeculæ	20 46·7	27 50 N.	,, 12,	0 0	*m*
δ Cephei	22 25·0	57 51 N.	,, 15,	4 0	*M*
			,, 18,	22 0	*m*

M signifies maximum ; *m* minimum ; m_2 secondary minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near γ Andromedæ	25	42 N.	Swift ; streaks.
The *Perseids*	60	56 N.	,, ,,
Near λ Persei	60	50 N.	,, ,,
,, ζ Aurigæ	73	41 N.	,, ,,
,, δ Draconis	290	70 N.	Swift ; short.

THE SCIENTIFIC VALUE OF VOLAPÜK.

THE Committee appointed by the American Philosophical Society, on October 21, 1887, to examine into the scientific value of Volapük, presented the following Report at the meeting of the Society held on January 6, 1888 :—

Your Committee proposes, first, to consider the desirability of a universal language ; secondly, what should be its characteristics ; and, thirdly, whether that invented by the Rev. Mr. Schleyer, called by him Volapük, meets the requirements.

I.—That in the vastly increased rapidity of interchange of thought in modern times, some general medium of intercommunication would be welcome, is unquestioned. Wherever there are close commercial relations between nations speaking different tongues, such media are sure to arise from the necessities of daily life. Thus, the Lingua Franca in the Mediterranean, and "pigeon English" in the Chinese ports, are dialects which have sprung out of the urgency of business needs. These mixed languages are called "jargons," and have a very high interest to the scientific linguist, as illustrating the principles of the evolution of human speech. The English language is a jargon of marked type, and illustrates what was stated by W. von Humboldt early in this century, that from such crossings and mingling of tongues are developed the most sinewy and picturesque examples of human language. This consideration shows that in adopting or framing a universal language we need not hesitate to mould it from quite diverse linguistic sources.

The presence of a number of these jargons in different parts of the world testifies to the desirability for some one simple form of discourse which could be of general adoption. Another and higher testimony to the same effect is the need now frequently and loudly expressed for a uniform terminology in the sciences. For many years it has been urged, both in this country and in Europe, that the neologisms required by the sciences be derived according to a uniform plan from the Greek, and that those heretofore obtained from Greek or Latin be brought into one general form. There is no practical difficulty about this except that which arises from the Chauvinism of some nations which are blinded by egotism or narrow notions to the welfare of the whole. Such a tendency is observable in Germany, a country once noted for its cosmopolitan sympathies. Its medical teachers, for example, have of late frequently dismissed the terms of their science derived from the Latin and Greek, in order to substitute in their place long, awkward, and inharmonious Teutonic compounds. No effort at a uniform international scientific terminology can be successful if the learned in each nation be governed by national prepossessions.

Another obstacle to a universal tongue, and which at the same time is a cogent argument for the adoption of one, is the sentimental love of local dialects and forms of speech by those who have imbibed them in infancy. To-day there are active Societies organized for the preservation of the Welsh, the Armorican, the Basque, the Finnish, and the Flemish. For many generations nearly all learned writings in Europe were in Latin. In the eighteenth century the Latin threatened to be superseded by the French. The Transactions of the Academy of Sciences of Berlin were in French ; so were the articles by the Russian Professors ; and in the earlier decades of the present century French prevailed in the Reports of the Royal Northern Society of Antiquaries, and in most scientific publications in Slavic and Northern Teutonic countries. This is the case no longer. Every little principality claims that it should print what it has to tell the world of science in its own dialect, and claims that the world of science should learn this dialect. Thus we have on the list of our scientific exchanges publications in Roumanian and Bohemian, in Icelandic and Basque, in Swedish and Hungarian, in Armenian and modern Greek, in Japanese and in Portuguese, without counting the more familiar tongues. Even a linguist by profession, such as Max Müller, has exclaimed against the very Babel, the confusion of tongues, which exists in modern scientific literature. He has sounded an earnest appeal to the learned writers of the world to express themselves in one of the half-dozen languages which every man of wide education is supposed to read—to wit, the English, French, German, Spanish, Italian, or Latin.

But even with the advantage of a well-developed international scientific terminology, it is a good deal to ask of a student of science that he should spend the time to acquire a reading knowledge of these six tongues. In many cases it is wholly impossible for lack of time. But time could always be spared to learn one language, if that were enough, particularly if this one were exceptionally simple and easy in its grammar.

Again, the commercial and travelling world demands one tongue only, in addition of course to that which its members learn in infancy, a tongue facile to acquire, and adaptable to their peculiar wants. The time is not far off when one system of weights, measures, and coinage, one division of time, one code of international law, one mode of quarantine and sanitation, one costume, will prevail throughout the civilized world, and along with this unification of action must and will come a unification of speech. It is not only desirable, it is certain to arrive; and, as beings of intelligent self-consciousness, looking before as well as after, it becomes us to employ our faculties to direct the course of events so that this one universal language be not left to blind chance, but be framed and adopted of deliberate choice, and with the wisest consideration.

II.—Convinced, therefore, that the time is ripe for the promulgation of a general form of speech for the civilized members of the race, we will now inquire what should be the requirements of such a tongue to merit the recommendation of this Society.

We begin by the observation that the Aryan stock is now, and has been for 2000 years, the standard-bearer of the civilization of the world; hence, a universal language should be based upon the general linguistic principles of that stock. In the Aryan stock the six principal living tongues in the order of their importance and extent may be ranged as follows: English, French, German, Spanish, Italian, Russian. It should be the aim of the proposed general tongue to ally itself to these somewhat in the order noted, as thus being more readily acquired by the greater number of active workers in the world at the present time.

The elements of all languages arrange themselves to the linguist under three headings—phonetics, grammar, and lexicography; in other words, the vocal, the formal, and the material characteristics of the tongue; and under these three headings we will sketch the traits which should make the projected universal language.

(1) *Phonetics.*—We believe all will assent to the following propositions:—

The orthography of the universal language should be absolutely phonetic.

Every letter in it should always have the same sound.

This sound should be one common to all the leading Aryan languages, and hence present no difficulty to a person speaking any one of them.

Diphthongs, digraphs, and double consonants should all be omitted as misleading.

The meaning should never depend on tone, accent, quantity of vowels, nor rising and falling inflections of the voice. All these are inadequate and unnecessary expedients of the linguistic faculty.

The vowels should be limited to the five pure vowels: *a*, *e*, *i*, *o*, *u*, pronounced as in Italian, and all impure or modified vowel sounds, as the German *ä*, *ö*, *ü*, the French *u*, the English *u* (as in *use*), *o* (as in *not*), and the neutral vowel *u* (in *but*) should be discarded. All the Aryan tongues named possess the five pure vowels, but not all the impure and neutral vowels.

In the consonantal scheme all gutturals, aspirates, lisps, and nasals should be omitted. Thus, the German *ch*, soft or hard, the Spanish *z*, the English *h* and *th*, the French *n*; and likewise all double consonantal sounds, like the Spanish *ñ*, *ll*, *rr*, the German *kn*, *pf*, the Russian *schtsch*, the Italian *zz*, *cc*, &c., should find no place. Of all the Aryan languages the pure Castilian Spanish comes the nearest to such an ideal phoneticism, and it approaches very near indeed, a few consonantal heresies and the accent being its only drawbacks.

In the written alphabet of such a language there should be, and there would be no occasion for, any diacritical marks whatever. The so-called Latin or Roman handwriting and type should be employed, but with the omission of every sign which would require the writer to take his pen from the paper in the middle of a word, or else return to it in order to complete it. Hence the *i* should have no dot (as is the case in German), nor the *j*, and the *t* should not be crossed. No accents should be needed, and no apostrophes.

The sounds of the language should not only be easy, they should also be fairly agreeable to the ear; and combinations should be sedulously avoided which in any of the leading tongues have indecorous or degrading associations.

Brevity is of great importance, and each word should be reduced to its simplest discriminative sound, consistent with sonorousness and lucidity.

(2) *Lexicography.*—The vocabulary of the universal language should be based primarily on the vocabulary which is common to the leading Aryan tongues. There are 1500 words in German which are almost or quite the same in English; there are more than this number common to English, French, Italian, and Spanish. A selection should be made from these similar or identical word-forms as the foundation of the lexicon. At least a thousand words in common use will be found to be the same in all these languages, when we allow for the operation of simple and well-known phonetic laws. Let the learner be taught these laws, and he will at once know a good share of all the more usual terms of daily intercourse in this new language, and he will pronounce them correctly without a teacher, because it will contain no sound which is strange to his ears, and each word would be spelled as it is pronounced.

This existing common property of words, once assorted and presented in the orthography above set forth, would form one element of the lexicon; another will be those words obtained from an international scientific terminology, to be decided upon by the Committees of International Congresses, appointed for that purpose.

Commercial and business terms are already largely the same, and there would be little difficulty in obtaining a consensus of opinion about them which would prevail, because it is of direct pecuniary advantage to business men to have such a uniformity.

There remain the terms in art, literature, poetry, politics, imagination, &c., to be provided for. But in the opinion of this Committee it does not seem desirable at this time to urge the formation of a vocabulary which would be exhaustive. Much of it should be left to the needs of the future, as observed and guided by the International Committees who should have the care and direction of the universal tongue. These Committees should, by common consent, hold the same relation to it that the French Academy has, in theory at least, to the French language, enlarging and purifying it by constant and well-chosen additions. As in France, each writer would enjoy the privilege of introducing new terms, formed in accordance with the principles of the tongue, and such terms would be adopted or not, as they should recommend themselves to other writers in the same field.

(3) *Grammar.*—By far the greatest difficulty is presented by the formal or grammatical features of such a proposed tongue.

We may best approach this part of our task by considering how the grammatical categories, or "parts of speech," as they are called, are treated in the various Aryan tongues, and selecting the simplest treatment, take that as our standard.

It may indeed be inquired whether in the grammar we might not profitably pass beyond the Aryan group, and seek for simpler methods in the Semitic, Turanian, African, or American languages. But it is a sufficient answer to this to say that there is no linguistic process known to these remote stocks but has a parallel in some one of the Aryan dialects; and if such a process is very slightly developed in these dialects, this is probably the case because such a process has been found by experience to be unsuited to the modes of Aryan thought.

Returning to the grammatical categories or parts of speech, we find them usually classified as nine, to wit: articles, noun, pronoun, adjective, verb, adverb, preposition, conjunction, interjection.

The last of these, the interjection, is of no importance; and as for the first of them, the article, we find that the Latin and the Russian move along perfectly well without it, and hence we may dismiss it, whether article definite or article indefinite, as needless in the universal language.

The adjective in Latin has gender, number, and case, and, in most living Aryan languages, has number and gender; but in English it has neither, and, therefore, true to the cardinal principles of economy in the formal portions of speech, in the universal language it should have neither. More than this, in colloquial English and German, and always in English in the comparative degree, there is no distinction between the adjective and the adverb; and upon this hint we perceive the inutility of the distinction and dismiss it as operose only. The comparison of adjectives should be by words equivalent to *more* and *most*, as is practically the case in the Romance languages, and never by comparative and superlative terminations, as in English and German, as our endeavour should always be to maintain the theme unchanged.

This reduces our nine parts of speech to six, which are proved to be enough, by the facts quoted.

The noun in German undergoes changes for gender, number, and case. Of these the gender in all Aryan tongues, except English and modern Persian, is an absurdity, without application to the object, and a most serious impediment to learners. Grammatical gender, therefore, should be absolutely dismissed, and material gender expressed by the feminine adjective of sex, as in English and most American languages (bear, she-bear, rat, she-rat, &c.).

The Greek has a singular, a dual, and a plural number. The dual has dropped out of modern tongues, and in many instances the plural is grammatical only and not material. Indeed, as in most American languages, so often in English and German, the plural form is not used even when the plural number is meant. Thus, we say, ten head of steers, six dozen herring, sechszehn Stück Cigarren, sechs Uhr Abends, &c. It is probable, therefore, that both gender and number could be usually dispensed with in nouns.

With regard to the case of nouns, it will be observed that the tendency of all the Teutonic and Romance languages has been to get rid of them: French and Spanish have succeeded completely; the English retains the genitive, the German the nominative, genitive, dative, and accusative, in some instances. The cases have been supplied by the use of pronouns and prepositions, and we shall be wise to respect this tendency as indicative of linguistic progress. It is historically clear that to attempt to restore the case-endings of nouns would be to steer directly against the current of linguistic evolution. There has even been an effort both in English and German to dispense with the genitive by substituting a possessive pronoun for the case-ending, as "John his book," "Ludwig sein Pferd"; while the Berlin dialect of the lower classes has often but one termination for both genitive and dative, where pure German has two.

The use of the possessive pronoun to indicate the genitive is simple and logical; it prevails in most American languages and most jargons; and is quite adapted to the end. In fact, some dialects, such as the French Creole of Trinidad, Martinique, and St. Thomas, contain no pronominal adjectives, and make out very well by placing the personal pronoun, like any other attributive case, after the noun, as *liv li*, "his book," literally, "book he." It might be queried whether the universal language would not gain in ease and simplicity by adopting this method of placement.

The dative, or *régime indirecte*, could be supplied in a similar manner by a pronoun in an oblique form. There is no necessity for more than two oblique cases of the pronoun, and they can be added to all nouns as a substitute for prepositions, when needed for clearness.

The pronouns of the modern tongues, and especially of their colloquial dialects, demonstrate that the relative, interrogative, and demonstrative forms can be blended without loss of lucidity. The German *der*, the English *that*, are both relative and demonstrative; the French *qui* and *ça* are both relative and interrogative in Creole.

The reflexive pronoun is used very unnecessarily in most modern Aryan tongues. There is no logical propriety in the French *Je me casse le bras*. The use of such a form should be greatly restricted.

The verb has tense and mood, number and person. It is conjugated in all Aryan languages, sometimes regularly, sometimes irregularly, and it has many forms. In studying its history, however, no one can overlook its steady tendency towards simplification of the form of the theme and the adoption of the periphrastic method of conjugation, or that by auxiliaries. By this process the verb loses all inflections and is reduced to a single form; person and number are expressed in the subject, tense, and mode by auxiliaries. This should be the process adopted by the universal language, with perhaps the exception that the simple past and future might be expressed by terminations, every verb being absolutely regular. The future termination is now lost in English and German, and even the past termination is often dispensed with in both tongues, as "I give," "I did give," "ich that geben"; but as both are vigorous in the cultivated Romance tongues, these formal elements might be conceded.

A very delicate question relates to the substantive verb "to be." Shall we omit it or express it? The Latin rarely introduces it, and there are numerous tongues in which it has no equivalent. On the other hand, modern Aryan speech has developed it markedly; the Spanish has its *ser* and *estar*, the German its *sein* and *werden*, expressive of shades of meaning included in our verb "to be." This prominence of the expressions for existence seems to be connected with marked psychological advances, and a ripening self-consciousness, as has been lately set forth by a profound French critic of language, M. Raoul de la Grasserie. We should be inclined, therefore, to respect this expression, and allow it in a universal language the prominence it enjoys in most Aryan tongues of modern type.

The prepositions offer great difficulties in modern languages. The most of them can be omitted by making all verbs which have an active meaning govern their object directly, and have the direct object follow the verb and the indirect object placed later in the sentence. The phrase, "Give to the child a spoon," would be just as intelligible in the form "Give spoon child," if we remember that the direct precedes the indirect object.

The simplification of grammatical forms here proposed involves an equal simplification in syntax, and this is an enormous gain. But it involves also the loss of freedom of position, so conspicuous a feature in Latin, and by some so highly esteemed. But philosophically considered, this freedom of position is solely a rhetorical and artistic gain, not a logical superiority. Grammarians even of the classical tongues are perfectly aware that there is a fixed logical arrangement of words in a sentence, and this, and this alone, is the only arrangement which a universal language should adopt. This arrangement may be briefly given as follows: subject before predicate, noun before its adjective, verb or adjective before qualifying adverbs, immediate object before remote object. This is the logical course of thought, and should be the universal form of expression. It was a dubious advantage to the Greeks and Latins that their numerous inflections permitted them to disregard it.

Those languages which rely largely on position obtain rhetorical grace by a recognized value assigned to alterations of position; and this would apply equally to the scheme proposed.

Other questions will arise in the projecting of a universal language. Shall we adopt postpositions as well as prepositions? Shall we indicate inflections by internal vowel changes? Shall we have free recourse to affixes, suffixes, and infixes? Shall we postfix conjunctions, like the Latin? Shall we manufacture entirely new roots from which to form new words and derivatives?

To all these questions your committee replies with an emphatic negative. All such processes are contrary to the spirit which has pervaded the evolution of the Aryan languages for the last two thousand years, and their adoption would violate the indicated rules for the formation of a universal Aryan speech.

III. In applying the principles which have been above set forth to the creation of the Rev. Johann Martin Schleyer, we find something to praise and much to condemn in his attempt.

Mr. Schleyer first published a sketch of his proposed universal language in 1878, and the first edition of his grammar in 1880. It has been welcomed with applause in Germany, and efforts have been made with some success to introduce it into France, England, and America.

His scheme is evidently the result of conscientious labour and thought, and he manifests a just appreciation of the needs of the time; but unfortunately the theory of construction he has adopted is in conflict with the development of both the Teutonic and Romance languages, and full of difficulties to the learner.

Beginning with its phonetics, we find that he has retained the impure German modified vowels *ä*, *ö*, *ü*, the French *j* (*dsch*), as well as the aspirated *h* or rough breathing. He has eight vowels and nineteen consonants where five vowels and sixteen consonants should suffice; elsewhere he extends his alphabet to thirty-seven letters. He also introduces various diacritical marks indicating accent, tones, vocal inflection, and quantity, all of which we consider needless and obstructive. Double consonants are numerous, and the Volapük is both written and printed with underscoring and italic letters, necessary to facilitate its comprehension.[1]

The lexicography is based largely on the English, about 40 per cent. of the words being taken from that tongue, with phonetic modifications. These modifications do not regard the other Aryan languages, and various sounds of the Volapük alphabet could not be pronounced by a member of any Aryan

[1] These remarks are based upon the seventh edition of Schleyer's "Mittlere Grammatik der Universalsprache Volapük" Konstanz, 1887).

nation without special oral teaching. This we regard as a fatal defect.

Moreover, many words are manufactured from entirely new radicals, capriciously, or even fantastically formed, and this we condemn.

The article is omitted, which is well; but the nouns are inflected through a genitive, dative, and accusative case, and a plural number. The signs of these cases are respectively *a, e, i,* and of the plural *s*.

Diminutives, comparatives, and superlatives are formed by prefixes and suffixes, and on the same plan adverbs are formed from adjectives, and adjectives from nouns. Thus, *silef*, silver; *silefik*, silvery; *silefiküm*, more silvery; *silefikün*, most silvery; *silefiko*, silverly. It will be observed that, while this process is not dissimilar to that once frequent in the Aryan stock, it is not analogous to that which the evolution of that stock indicates as its perfected form.

In the conjugation the subject follows the verb, *bin—ob*, I am; where *bin* = am, *ob* = I. This we object to as contrary to the logical arrangement of the proposition. We are surprised to see the German third person plural (*Sie*) retained by the author as a "courteous" form. It should be the first duty of a universal language to reject such national solecisms.

The tense is indicated by prefixes *a, e, i* for the imperfect, perfect, and pluperfect active, *o* and *u* for the two futures.

The passive voice has the prefix *p*, the subjunctive by the suffix *la*, the optative and imperative by the suffix *ös*, the infinitive by the suffix *ön*. Abstracts are formed by adding *äl*, as *mon*, money; *monäl*, love of money, avarice. These suffixes are to be placed in fixed relations to the root, and hence often become infixes.

The excessive multiplication of forms lends to Volapük an appearance totally un-Aryan. The verbal theme is modified by sixteen suffixes and fourteen prefixes. There are a "durative" tense, and a "jussive" mood, conjunctive, optative, gerund, and supine forms, all indicated by added syllables, reminding one of the overloaded themes of Turanian tongues. This mechanism is not only superfluous, but if any lesson may be learned from the history of articulate speech, it is precisely the opposite to what the universal language should and must be.

The meaning is largely derived from placement, as will be seen in the following example, in which *gudikos* is the neuter adverbial noun "goodness," das Gute; *plidos*, from English "please," the third singular indicative.

Gudikòs plidös Godè.
Goodness pleases God.
Plidos Gode gudik.
It pleases God the good (the good God).
Plidòs gudìk Gode.
It pleases well God.

And so on. It is acknowledged by the author that obscurities may easily arise from these transpositions, and there is much dependence on accents and tones.

From this brief comparative examination of the evolutionary tendencies of the Aryan tongues and the scheme of a universal language as offered in the works of Mr. Schleyer, it is plainly evident that the two are in absolute opposition.

Volapük is synthetic and complex; all modern dialects become more and more analytic and grammatically simple; the formal elements of Volapük are those long since discarded as outgrown by Aryan speech; its phonetics are strange in parts to every Aryan; portions of its vocabulary are made up for the occasion; and its expressions involve unavoidable obscurities. With an ardent wish for the formation and adoption of such a universal tongue, and convinced as we are that now is the time ripe for its reception, we cannot recommend Volapük as that which is suited to the needs of modern thought. On the contrary, it seems to us a distinct retrogression in linguistic progress. Nor in this day of combined activities does it appear to us likely that any one individual can so appreciate the needs of civilized nations as to frame a tongue to suit them all. Such a task should be confided to an International Committee from the six or seven leading Aryan nationalities.

In conclusion, your Committee would respectfully suggest that it would eminently befit the high position and long-established reputation for learning of the American Philosophical Society, to take action in this matter, without delay, and to send an official proposition to the learned Societies of the world to unite in an International Committee to devise a universal language for business, epistolary, conversational, and scientific purposes. As the time once was when the ancestors of all Aryans spoke the same tongue, so we believe that the period is now near when once again a unity of speech can be established, and this speech become that of man everywhere in the civilized world for the purposes herein set forth.

Your Committee therefore offers the following resolution—

Resolved,—That the President of the American Philosophical Society be requested to inclose a copy of this Report to all learned bodies with which the Society is in official relations, and to such other Societies and individuals as he may deem proper, with a letter asking their co-operation in perfecting an international scientific terminology, and also a language for learned, commercial, and ordinary intercourse, based on the Aryan vocabulary and grammar in their simplest forms; and to that end proposing an International Congress, the first meeting of which shall be held in London or Paris.

D. G. BRINTON, *Chairman*,
HENRY PHILLIPS, JUN., } *Committee.*
MUNROE B. SNYDER,

The following Supplementary Report was also read on the same occasion:—

The former Report having been recommitted, your Committee avails itself of the opportunity to explain more clearly the aim of the previous paper, to meet some of the objections offered against particular statements, and, at the request of several members, to enlarge the scope of the Report, so as to embrace a brief consideration of the two other universal languages recently urged upon the public, the "Pasilengua" of Steiner, and the "international language" of Samenhof.

The aim of the Committee was strongly to urge the desirability of taking immediate steps to establish a universal language, both for learned and general purposes. These steps, it asseverated, should be taken by the learned world as a body; the form of language adopted should be indorsed by the scientific Societies of all nations; by their recommendation it should be introduced into schools and Universities, and competent private teachers would soon make it familiar to all who would have occasion to use it. The Report distinctly states that it is in nowise expected that this international language will supplant any existing native tongue. It is to be learned *in addition* to the native tongue, and not *in place* of it.

The aim of the grammatical portion of the Report was simply to maintain three theses: first, that the pronunciation of the proposed tongue should be so simple that it could be learned by anyone speaking an Aryan language, without the necessity of oral instruction; secondly, that its grammar should be simplified to the utmost; and thirdly, that its lexicon should be based on the large common property of words in the Aryan tongues.

Your Committee repeats and insists that these are the indispensable requisites to any such proposed international tongue. It does not insist that the individual suggestions and recommendations contained in the Report should be urged at all hazards. They were advanced rather as hints and illustrations, than as necessary conditions. Nevertheless, they were not offered hastily, and your Committee desires to refer to some of the main arguments advanced against them. This it is prepared for the better, through the complaisance of Profs. Seidenstücker and Easton, who have forwarded to the Committee, at its request, abstracts of their remarks.

Both these very competent critics attack the principle of deducing the grammar of the proposed language from the latest evolution of Aryan speech, to wit, the jargons. Prof. Seidensticker accuses such a grammar of "poverty," and adds: "A higher organism is of necessity more complex than a lower one." Prof. Easton denies that the later is the better form; or, to use his own words, "that the change from an inflected to an analytic tongue marks an advance in psychologic apprehension."

These criticisms attack a fundamental thesis of your Committee, and as they doubtless express the views of very many, they must be met.

In our opinion, they rest upon a radical misconception of the whole process of linguistic evolution. The crucial test of the development of language is that the sentence shall express the thought intended to be conveyed, and *nothing more*. When this can be attained simply by the order of words in the sentence, without changes in those words, such changes are not merely useless, they are burdensome, and impede the mind. All paradigmatic inflections, whether of nouns, adjectives, or verbs, are relics of lower linguistic organization, of a barbaric condition of speech, and are thrown aside as useless lumber by the active linguistic faculty in the evolution of jargons. Compare a simple Latin sentence from Cicero, with its translation into English, which is a jargon of marked type, and note how much is dropped, and with what judicious economy: "*Romanis equitibus literæ afferuntur*"—"Letters are brought to the Roman knights." One word here will serve to illustrate all. In Latin the speaker must think of the adjective *Romanis* as masculine, not feminine, or neuter; as plural, not singular; as a dative, not a nominative, accusative, or vocative form; as agreeing in all these points with the noun it qualifies; and finally, as of the first, and not of the second, third, or of some irregular declension. All this needless labour is saved in the English adjective *Roman* by the method of position or placement. And so it is with every other word in this sentence. The evidence, both from theory and from history, is conclusive that the progress of language, linguistic evolution, means the rejection of all paradigms and inflections, and the specialization of the process of placement.

Prof. Easton maintains that this method (that of placement) "introduces an element of great difficulty into the language," and also doubts the acceptance of the logical order stated in the Report.

To the first of these objections the obvious answer is that the method of placement is that uniformly adopted in all jargons and mixed tongues, which is positive proof that it is the least difficult of any method of expressing relation. As to the logical order referred to by the Committee, it is surprising that any exception should be taken to it, as it is that stated in the common classical text-books.

Some related minor points remain to be noticed. In opposing vocal inflection, signs, and accents, in their Report, the Committee referred only to the written, not to the spoken language. The phonetic formation proposed is insisted upon only to the extent that no sound should be introduced which would be strange to the six leading Aryan languages. The substitution of placement for prepositions, which they recommended, was meant as illustrative merely. The particular statement that the Berlin dialect (of the lower class) has but one termination for both genitive and dative is upon the authority of Dr. and Mrs. Seler, of Berlin, the former a professed linguist, the latter born and raised in that city. The question whether, in the German expression, *sechs Uhr Abends*, the word *Uhr* is a singular form with a plural meaning, is contradicted by Prof. Seidensticker; but, in view of the strictly analogous Spanish expression, *las seis horas de la tarde*, the Committee maintains its original opinion.

Passing from these specific animadversions, there were some general objections which should be answered. Various speakers maintained that the project of an international language is impossible of realization; others asserted that it was unnecessary; others that, even if realized, such a tongue could have no figurative or artistic wealth of resources.

To these strictures it is replied that within eight years Volapük is claimed to have acquired 100,000 students; within a month it has attracted attention all over the United States; within a week a number of German merchants have announced to their foreign correspondents that in future it will be used in their business communications. If this is the case with so imperfect a language, backed by no State, no learned body, not even by the name of any distinguished scholar, what would be the progress of a tongue perfect in adaptation, and supported by all these aids to its introduction? In a decade it would be current among 10,000,000 people. That it would be barren in figurative meanings, or sterile in the expression of the loftier sentiments, is inconceivable, because, formed though it would be of deliberate purpose, the inherent, ever-active linguistic faculty of the race would at once seize upon it, enrich it, mould it, and adapt it to all the wants of man, to the expression of all his loves and hates, his passions and hopes.

Your Committee closes with a reference to the remaining two tongues now claimants for universal adoption.

The "Pasilengua" (*Gemeinsprache*, "Tongue of All") was introduced by P. Steiner, in 1885, with a small grammar and dictionary, published in German. The "international language" of Dr. L. Samenhof, of Warsaw, is an arrival of the present year, and is explained by him in a small volume, issued in French, in his native city, under the pseudonym of "Dr. 'Esperanto."

Both these have pursued the correct path in the formation of their vocabulary; they both proceed on the plan of collecting all words common to the Aryan languages, changing their form as little as possible consistently with reducing them to an agreeable phoneticism, and when the same word has acquired diverse significations, selecting that which has the broadest acceptation. The plan of Dr. Samenhof is especially to be recommended in this respect, and may be offered as an excellent example of sound judgment. It is remarkable, and remarkably pleasant, to see how easy it is to acquire the vocabulary of either of these writers, and this is forcible testimony how facile it would be to secure an ample and sonorous stock of words, practically familiar to us already, for the proposed universal tongue.

Unfortunately, the alphabets of both employ various diacritical marks and introduce certain sounds not universal to the leading Aryan tongues. These blemishes could, however, be removed without much difficulty.

It is chiefly in the grammar that both err from the principles strenuously advocated by your present Committee. The Pasilengua has an article with three genders, *to*, *ta*, *te*, corresponding to the German *der*, *die*, *das*; it has also three case-endings to the noun, besides the nominative form, which itself changes for singular and plural, masculine and feminine. In the verb the tenses are formed by suffixes, six for the indicative, four for the subjunctive; while a number of other suffixes indicate participles, gerunds, imperatives, &c.

In the same manner, Dr. Samenhof expresses the relation of the elements of the proposition in the sentence "by introducing prefixes and suffixes." "All the varying grammatical forms, the mutual relation of words to each other, are expressed by the union of invariable words" ("Langue Internationale," p. 13). He acknowledges that this is "wholly foreign to the construction of European [he means Aryan] languages," but claims that it yields a grammar of such marvellous simplicity that the whole of it could be learned in one hour. In reality, it is what is known to linguists as the agglutinative process, and is found in the Ural-Altaic tongues, in high perfection.

It will be seen at once that the grammatic theories of both these tongues are directly in opposition to that advocated in the present and the previous Reports. These are both distinct retrogressions to an earlier, less developed, and more cumbersome form of language than that which dispenses with paradigms and inflections of all kinds.

Nevertheless, these repeated efforts go to show that an international language is needed, that it is asked for, that it is coming, and justify the propriety of this Society, which, as far back as the second decade of this century, marked itself as a leader in linguistic science, taking the van in this important and living question.

After discussion, during which amendments to the resolution originally proposed by the Committee were offered by Prof. Cope and Mr. Dudley, the Society adopted the following resolution by a unanimous vote—

Resolved,—That the President of the American Philosophical Society be requested to address a letter to all learned bodies with which this Society is in official relations, and to such other Societies and individuals as he may deem proper, asking their co-operation in perfecting a language for learned and commercial purposes based on the Aryan vocabulary and grammar in their simplest forms; and to that end proposing an International Congress, the first meeting of which shall be held in London or Paris.

THE LICK OBSERVATORY.

WE reprint from the *Daily Alta California* the following extracts from a private letter from Prof. Holden to a gentleman in San Francisco, giving details regarding the first astronomical observations made at the Lick Observatory:—

"The Lick Observatory is beginning to present a very different appearance, both by night and by day, from the one it lately had during its period of construction. At night the windows which have been so long dark show the lamps of the astronomers gleaming through them. The shutters of the observing slits are open, and the various instruments are pointed through them at the sky. The actual work of observing has begun, and the purpose for which the Observatory was founded—to be 'useful in promoting science'—is in the way of being accomplished. Prof. Schaeberle, late of Ann Arbor, has commenced the long task which has been assigned to him—namely, to fix with the very highest degree of precision possible to modern science, the position of the 'fundamental stars' with the Repsold meridian circle. The time-service for railway use is now conducted by

Mr. Hill (late assistant to Prof. Davidson), which leaves Mr. Keeler free to make the necessary studies of the great star spectroscope, which is one of the most important accessories of the 36-inch equatorial. Mr. Barnard is assiduously observing comets and nebulæ with the fine 12-inch equatorial, and getting the photographic appliances in readiness to be used with the great telescope. He has already discovered twenty new nebulæ, found in the course of his sweeps for new comets. To show you some of the advantages of our situation here, I may tell you that Prof. Swift, of Rochester, has a fine 16-inch equatorial by Alvan Clark, and has discovered many faint nebulæ by its use. Two nights ago Mr. Barnard was examining some of these excessively faint objects by means of the 12-inch telescope (which gives only a little more than half the light of Prof. Swift's), and in the field of view where Prof. Swift had mapped only one nebula Mr. Barnard found three, two being, of course, new. This is due not only to the observer's skill and keenness of eye, but in great measure to the purity and transparency of our atmosphere here.

"The Eastern astronomers have given up the observation of Olbers's comet, which is now only about 7/100 as bright as last year, but Mr. Barnard has succeeded in following it up to last night, when it finally became too faint to be seen even here. These observations, which are several weeks later than those of other Observatories, are of real value, as they determine a larger arc of the comet's orbit, and enable its motion to be fixed with a much higher degree of accuracy. Mr. Keeler is just reducing his observations of the faint satellites of Mars, made with the large telescope during the past months. You can gain some sort of an idea of the immense advantage of the great telescope in such observations, when I tell you that the brightness of the satellites as observed by him was only about one-sixth of their brightness at the time of their discovery. We can, then, make satisfactory observations of objects which are *six times fainter* than those very minute satellites of Mars were when Prof. Hall discovered them in 1877 with the great telescope at Washington. I am becoming familiar with the performance of the large telescope and learning how to get the very best work from it. It needs peculiar conditions; but when all the conditions are favourable its performance is superb. I am, as you know, familiar with the action of large telescopes, having observed for many years with the great refractor at Washington, but I confess I was not prepared for the truly magnificent action of this, the greatest of all telescopes, under the best conditions. I have had such views of the bright planets (Mars and Jupiter), of nebulæ, the Milky Way, and some of the stars, as no other astronomer ever before had. Jupiter, especially, is wonderfully full of detail that I had not begun to see before. The disks of his moons can be readily noted in smaller telescopes; but here they are full and round, like those of planets. I am almost of the opinion that the curve of Jupiter's shadow might be seen on the surfaces, under favourable circumstances, when the satellites suffer eclipse. There is reason to believe that the satellites of Jupiter, like our own moon, present always the same face to their planet. This can be studied here to great advantage if the disks present any of the markings which are reported by other observers.

"The Milky Way is a wonderful sight, and I have been much interested to see that there is, even with our superlative power, no final resolution of its finer parts into stars. There is always the background of unresolved nebulosity on which hundreds and thousands of stars are studded—each a bright, sharp, separate point. The famous cluster in Hercules (where Messier declared he saw 'no star') is one mass of separate individual points. The central glow of nebulosity is thoroughly separated into points. I have been specially interested in looking at objects which are familiar to me in other telescopes and in comparing our views with the drawings made by Lord Rosse with his giant 6 foot reflector. Theoretically, his telescope should show more than ours, for his collected the most light. But the *definition* (sharpness) of his is far behind our own, as we constantly see. For example, the ring nebula in Lyra is drawn by Lord Rosse with no central star. At Washington, one small star can be seen in the midst of the central vacuity, but here we are sure of seeing three such at least. These are interesting on account of their critical situation in the nebula, not simply as stars.

"The great Trifid and Omega nebulæ are wonderful objects here. Not only is a vast amount of detail seen here which cannot be seen elsewhere, but the whole aspect of them is changed. Many points that are doubtful with other telescopes are perfectly simple and clear here. I have always considered that one of the great practical triumphs of this telescope would be to settle, once for all, the doubts that have arisen and that will arise elsewhere. Now, I am sure that we shall be able to do this, and in a way to end controversy.

"Of course you understand that the period of construction here is not yet quite over, though, I am thankful to say, it is nearly ended. We have been making our observations, so far, under great disadvantages, and now that we see the way out of most of them, and look forward to work uninterrupted by machinists and constructors, we begin to realize the opportunity. It really takes time to understand how to utilize it in the very best way. A great telescope is not like an opera-glass, which can be taken out of one's pocket, and which is at once ready for use. It is a delicate and a complicated machine, which demands a whole set of favourable conditions for its successful use. Every one of these conditions has to be studied and understood, so that it can be commanded and maintained. We have been busy night and day in this work, and in completing the thousand arrangements and contrivances which are essential in order to turn this vast establishment from a museum of idle instruments into a busy laboratory, where the inner secrets of the sky are to be studied. We feel sure now that in a comparatively short period we shall be in full activity. In the meantime every one of us is doing his best under the conditions.

"We expect to open the Observatory to visitors every Saturday night from 7 to 10 o'clock, beginning next Saturday, July 14.

"EDWARD S. HOLDEN."

SCIENTIFIC SERIALS.

Studies from the Biological Laboratory of Johns Hopkins University, vol. iv. No. 4, June 1888.—On the life-history of *Epenthesis mccraydi* (n. sp.), by W. K. Brooks, Ph.D. (plates 13–15). In June 1887, Dr. Brooks found at Nassau, in the Bahamas, a few specimens of a Hydromedusa belonging to the Eucopidæ, bearing upon each one of its four reproductive organs a number of Hydroid blastostyles from which young Medusæ were produced by budding; a method of reproduction which has no parallel among the Hydroids, if, indeed, it occurs elsewhere in the animal kingdom. While in their endless diversity the Hydromedusæ present nearly all imaginable phases of development, yet in all hitherto recorded cases the life-history of each species from the egg to the second generation of eggs is a history of progression, but this Nassau Medusa is an exception to the general rule; the bodies which are carried on the reproductive organs of the Medusa are true blastostyles, so that there is a recapitulation of larval stages without sexual reproduction. This remarkable form had on its first discovery been referred to Oceania, but is really an Epenthesis. The Medusæ carry on their reproductive organs campanularian Hydroid blastostyles, inclosed in chitinous gonangia. These blastostyles do not multiply by budding or from Hydroid cormi, although they produce Medusæ by budding. The ectoderm of the blastostyle is produced by ordinary gemmation, and is directly continuous with the ectoderm of the Medusa, but its endoderm has no direct communication with the medusal endoderm, its germ-cells arising by the process termed sporogenesis by Metschnickoff.—Observations on the development of Cephalopods: homology of the germ-layers, by S. Watase (plates 16 and 17). In this most important paper the history of the formation of the germ-layers is traced, and many disputed points are settled.—On the development of the Eustachian tube, middle ear, tympanic membrane, and meatus of the chick, by Dr. F. Mall (plates 18 and 19). Confirms Prof. His's demonstration, controverted by Fol and others, that the branchial clefts are not fissures.—On the branchial clefts of the dog, with special reference to the origin of the thymus gland, by Dr. F. Mall (plates 19–21).—On experiments with chitin solvents, by T. H. Morgan. Recommends the Labaraque solution (potassium hyperchlorite) as a solvent for chitin.

Notes from the Leyden Museum, vol. x. No. 3, July 1888.—Among the longer articles may be mentioned:—On the Erotylidæ of the Leyden Museum, by the Rev. H. S. Gorham. About seventeen are new, including four for which it has been necessary to make new genera.—On some new Phytophagous Coleoptera from Brazil, by M. Jacoby.—On the Shrews of the Malayan Archipelago, by Dr. F. A. Jentink.—On the habits and anatomy of *Opisthocomus cristatus*, by Dr. C. G. Young. In this paper there are no references to the various memoirs already published on the anatomy of this bird.—On some new or little-known Longicorns (Pachyteria), by C. Ritsema.—On birds from the Congo and South-Western Africa, F. Büttikofer.

Revue d'Anthropologie, troisième série, tome iii. troisième fasc. (Paris, 1888).—Report on the excavations made in the bed of the Liane in 1887 in laying the foundations for a viaduct, by Dr. E. T. Hamy. The mouth of this river, which is now filled with alluvial deposits, was in earlier times a vast estuary opening into the Channel; and in the recently completed excavations there has been found a mingled mass of animal bones, with metal and pottery fragments, belonging to all historic ages, from Roman times to our own, while the deep underlying strata recall, in their general character and appearance, Quaternary formations. Besides these remains, several pieces of a human skeleton have been found, including the cranium, which is considered by M. Hamy and other anatomists as belonging, in regard to its essential characteristics, to the oldest Quaternary cranial type. A slight degree of prognathism is the only feature of inferiority which it presents, and it in no way resembles the Negro or Negroid form. The remarkable elongation of all the bony parts in a vertical direction may be regarded as the special peculiarity of this skull, of which M. Hamy gives numerous measurements, based on the system adopted in the *Crania Ethnica*.—Continuation of an essay on the stratigraphic palæontology of man, by M. Marcellin Boule. The larger portion of this paper treats of the actual condition of palæontological research in England, and describes at length the numerous directions in which light has been thrown by recent British geologists on the effect of glacial action in determining the character and forms of the predominant geological features of the British Isles. The writer gives unqualified praise to the labours of Ramsay, Geikie, and others, lamenting, however, that in regard to numerous important points the views of the leading English palæontologists present great divergencies. In the second part of his essay M. Boule passes in review the results obtained by recent investigations of the traces existing in the Alps of recurring and intermittent glacial periods. In this inquiry he has made special use of Herr Penck's great work, "Mensch und Eiszeit" (1884), in which the strongest evidence is brought forward in proof of more than one advance and retrogression of glaciers in the valley of the Iller, and at other points of the Alpine range. These views have been confirmed by M. Blaas, and quite recently (1887) by M. Baltzer, and with few exceptions they have been generally adopted by Continental geologists; M. Falsan, Prof. Favre, of Geneva, and one or two others alone refusing to renounce the theory of one sole glacial period, while, however, they admit the possibility of the oldest glaciers having experienced more or less prolonged phases of advance and retreat.—The latest stages of the genealogy of man, by M. Topinard. This paper embodies the concluding and most important of the lectures delivered by the Professor at the Paris School of Anthropology. Beginning with the Lemuridæ, he treats of the grounds on which this animal family has been included by some, as Cuvier, under the Quadrumana, while Linnæus, Huxley, Broca, &c., class them with the Primates. To the latter view M. Topinard adheres, while he agrees generally with Prof. Huxley in including three groups under the Primates, viz. man, the Simians, and the Lemurians, the second group being separated into numerous divisions and subdivisions. M. Topinard's paper is interesting as a full and unbiassed exposition of the various hypotheses advanced by the leaders of modern biological inquiry as to the descent of man. While he freely expresses his personal aversion to the views of Vogt, which evidently point to the Ungulata as supplying the point of departure from which the primary source of man's descent emanated, he does ample justice to the great value of his labours, and acknowledges the benefit which he has derived from following the paths of inquiry inaugurated by the daring German physicist. Having minutely described the various anatomical characteristics which are common to man and to different mammalian families, he gives his reasons for believing that our descent is derived from the Simiadæ through a long series of intermediate forms of more or less strongly-marked anthropomorphic character, dating back to the Miocene age, when a divergence from the common type may have appeared, which, widening in the course of countless ages, has resulted in developing in man the perfect brain, and the maximum of differentiation in the extremities which give him his place in Nature.—On palæontology in Austria-Hungary, by M. M. Hörnes. The study of the prehistoric remains of their country is of recent date among Austrians, since the Anthropological Society of Vienna, the only one as yet incorporated by them, owes its origin to Rokitansky, and was only founded in 1870. Since that period, however, highly important results have been obtained from carefully conducted explorations in Carinthia and Carniola, where the discovery of vast burying-grounds and lacustrine stations has thrown much light on the condition and degree of civilization of primæval man in South-Eastern Germany. In Lower Austria almost all isolated hills and mountains present evidence of Neolithic occupation, many of them still retaining megalithic remains. Dr. Hörnes's article is especially interesting as showing what extensive, still almost untrodden tracts are being opened to palæontologists in different parts of the Slavonian and Czeck provinces of Austria; while his summary of the results already achieved, and his remarks on the ethnographic character of the primitive peoples by whom these regions were occupied in prehistoric times, throw considerable light on a hitherto obscure department of European palæontology.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 7.—"Note on some of the Motor Functions of certain Cranial Nerves (V, VII, IX, X, XI, XII), and of the three first Cervical Nerves, in the Monkey (*Macacus sinicus*)." By Charles E. Beevor, M.D., F.R.C.P., and Victor Horsley, B.S., F.R.S. (From the Laboratory of the Brown Institution.)

In the course of an investigation which we are making into the cortical representation of the muscles of the mouth and throat, we have experienced considerable difficulty in describing correctly the movements of these parts, especially when there was any question of bilateral action occurring.

On referring to text-books we failed to find any solution of this difficulty, and we therefore determined to make a few observations of the movements evoked by stimulating the several cranial nerves supplying this region in the monkey,[1] so as to have a definite basis whereon to ground our observations of the movements obtained by stimulating the cortex.

In the course of this work we have observed several facts which do not harmonize with the views hitherto generally received.

The results are summarized as follows:—

Method of Investigation.

The conclusions we have arrived at are based almost entirely upon the results obtained by exciting the respective nerves at the base of the cranial cavity after separating them from the bulb.

We have also stimulated the nerves outside the skull in the neck both before and after division.

In every case the animal was narcotized with ether. In all we have done eight experiments, and in every case we have operated on the same kind of monkey, *i.e. Macacus sinicus*.

The nerves were in each case raised up from their position and stimulated in the air by a faradic current through fine platinum electrodes, the area of the operation having been gently dried.

The current employed was from the secondary coil of an ordinary du Bois-Reymond inductorium, supplied by a 1 litre bichromate cell. The experiment was carefully begun with the secondary coil at a distance of 30 cm. from the primary, this interval being very rarely diminished to more than 15 cm. (zero being of course the point where the secondary coil completely overlaps the primary).

Further Observations respecting the Examination of each Nerve.
A. *Cranial Division.*

Vth Nerve.—Excitation of the motor root of the trigeminus evoked powerful closure of the jaws, and although the muscles of one side only were in action, the teeth were approximated without any lateral deviation of the lower jaw.

VIIth Nerve.—The motor distribution of the facial nerve has for the most part been well known for some time. However, we consider that, unfortunately, a very fundamental error respecting this distribution has crept into the text-books, it being supported by one anatomical authority following another, and, moreover, having been accepted by clinicians as an important aid in the differential diagnosis of facial paralysis. We refer to the supposed supply of motor fibres from the facial to the levator palati through the superficial petrosal nerve.

This idea,[2] upon which so much stress has been laid, is

[1] Previous observers having employed animals of lower orders.

[2] Without definitely supporting this view, Gaskell (Roy. Soc. Proc., vol. xliii. p. 390) shows that some large "somatic" nerve-fibres leave the facial nerve between its origin from the bulb and its exit from the stylo-mastoid foramen. He suggests that some of them may possibly form a nerve to supply the levator palati, but he leaves their real destination undetermined.

entirely hypothetical, as might have been shown at any time by stimulating the facial nerve in the skull, and observing the soft palate.

We have found that stimulation of the peripheral end of the divided facial nerve in the internal auditory meatus failed to cause even with most powerful currents the slightest movement of the soft palate, although the face was thrown into violent spasm. The true motor nerve supply of the levator palati is, according to our observations, the XIth nerve (*vide infra*).

IXth Nerve. Glossopharyngeal.—After exciting this nerve, in addition to the movements of the pharynx, which we attribute to the contraction of the stylopharyngeus, and possibly to the middle constrictor of the pharynx, we have observed certain movements of the palate, as follows :—(1) Stimulation of the nerve while beneath the stylo-hyoid ligament and uncut, gave in two instances elevation of the palate on the same side, and in one instance on both sides. We suppose that everyone will consider with us this movement to be reflex in origin, but we must add (2) that in one case we saw elevation of the palate to the same side when exciting the peripheral end of the cut nerve. In this latter case, perhaps, the result may be explained by the close neighbourhood of the pharyngeal plexus and the possible escape of current thereto, and under any circumstances this is but a single exceptional observation, so that we lay no stress upon it. Finally we never saw movement of the soft palate when the glossopharyngeal nerve was stimulated within the cranial cavity.

Xth Nerve. Vagus.—In stimulating the uncut nerve outside the skull, below the level of its junction with the hypoglossal, rhythmical movements of swallowing were produced, which occurred at the rate of twenty-five times in thirty-five seconds.

In one observation all the constrictors of the pharynx were thrown into action, when the peripheral end of the cut nerve was stimulated outside the skull.

The rhythmical movements of swallowing obtained by stimulating this nerve must be due to, of course, the simple reflex, the stimulus acting on the nerve in the centripetal direction, and that this was the case is proved by the fact that no movement was obtained when the peripheral end of the cut nerve was stimulated inside the skull.

The superior laryngeal branch on being stimulated gave rhythmical movements of swallowing at the rate of seventeen times in fifteen seconds, but when the nerve was cut and its peripheral end stimulated, only very slight movement was produced in the larynx, evidently by contraction of the crico-thyroid muscle.

XIth Nerve. Accessory to Vagus.—In discussing the motor functions of the VIIth nerve, we stated that the hitherto received idea of the soft palate being supplied by the facial nerve was, according to our observations, entirely erroneous. We find that the levator palati is supplied entirely by the XIth nerve.[1] When the peripheral end of the cut nerve was stimulated inside the skull, elevation of the soft palate on the same side was invariably seen. The path by which the fibres from this nerve reach the palate is probably through the upper branch of the pharyngeal plexus.

XIIth Nerve. Hypoglossal.—When the entire nerve was excited outside the skull, just below the point where it is joined by the first cervical nerve, the tongue was flattened posteriorly on the same side, and the tip protruded also on the same side, while in no case was there any heaping up of the tongue.

At the same time the depressors of the hyoid bone were thrown into action, and in some cases this dragging downwards of the hyoid completely prevented the tongue from being protruded.

The movements described above were repeated without alteration when the peripheral end of the cut nerve was excited at the same place.

It must be particularly noted that the movements of the tongue were purely uni-lateral, and this was proved to be the case beyond doubt by two experiments, in which the tongue was divided longitudinally in the middle line to the hyoid bone, when the movements were seen to be entirely confined to the side stimulated.

When the cut nerve was excited within the skull a different result was obtained, the tongue was flattened behind, and protruded towards the same side, but there was no action in the depressors of the hyoid.

[1] I desire to add here that Dr. Felix Semon, in the course of some experiments (unpublished), performed in conjunction with myself, found that in the dog the levator palati was innervated by the XIth nerve.—V. H.

It has always been held that the depressors of the hyoid bone receive their motor nerve supply from the hypoglossal through the descendens noni, but, as will be shown further on, according to our observation, these muscles are supplied by the first and second cervical nerves, and it is only when the hypoglossal is stimulated below the point where it is joined by the branch from the first cervical nerve, that any movement is produced in the depressors of the hyoid.

B. *Spinal Division.*

Our observations of the motor functions of the first three cervical nerves as regards their influence on the hyoidean muscles have been made when the nerves have been excited—

(*a*) In the spinal canal.

(*b*) In the neck immediately upon their exit from between the vertebral transverse processes.

The nerves in the spinal canal were separated from the spinal cord and thoroughly dried, the efficacy of the precautions taken against spread being evidenced by the difference in result obtained by exciting each root.

The effects obtained by the methods *a* and *b* were identical.

Ist Cervical Nerve. Branch of Union with the Hypoglossal.—In the description of the XIIth cranial nerve, we have stated as the result of our experiments that the depressors of the hyoid bone are not thrown into action when this nerve is stimulated within the skull. On carefully dissecting out the branch from the Ist cervical nerve to the hypoglossal we find on excitation of it that there is no movement in the tongue, but the depressors of the hyoid bone are strongly contracted. Of these muscles the sterno-hyoid and sterno-thyroid were always especially affected, while the omo-hyoid was less frequently seen to contract and in some cases not at all. In the cases where this muscle contracted, in one experiment the anterior belly alone acted, and when both bellies contracted the movement in the anterior was in excess of the posterior.

IInd Cervical. Branch to the Descendens Noni.—On stimulating this nerve the depressors of the hyoid were thrown into action, but the muscles involved were not affected in the same way as was the case with the Ist cervical nerve. The muscle which was most constantly set in action by excitation of the IInd cervical nerve was the omo-hyoid and especially its posterior belly. The sterno-hyoid and sterno-thyroid also took part in depressing the hyoid bone, but it was especially remarked in half the cases, that their action was notably less powerful than that of the omo-hyoid. In one experiment in which a very weak current was employed, the omo-hyoid was alone seen to contract. We are consequently led to conclude that while the sterno-hyoid, sterno-thyroid, and omo-hyoid muscles are all set in action by excitation of the Ist and IInd cervical nerves, the first two muscles are relatively supplied by the former nerve, while the IInd nerve is especially connected with the omo-hyoid muscle.

Descendens Noni.—We prefer to mention here the results of exciting this nerve, inasmuch as we regard its motor fibres to be derived entirely from the Ist and IInd cervical nerves. This nerve (ordinarily regarded as a branch of the XIIth cranial), when stimulated above its junction with the branch from the IInd cervical nerve, produced contraction of the sterno-hyoid and sterno-thyroid muscles, and where the current employed was weak there was no contraction of the omo-hyoid, but this movement was superadded on increasing the strength of the current.

We ought here to mention the opinion held by Volkmann (*loc. cit.*) that fibres ascend to the hypoglossal from the spinal rami communicantes by the descendens noni.

IIIrd Cervical Nerve.—On stimulating the branch from this nerve, which forms the IInd cervical nerve just before the ansa thus formed is connected to the descendens noni, there was no action seen in the depressor of the hyoid bone ; it therefore seems certain that these muscles are supplied with motor fibres solely by the branches from the Ist and IInd cervical nerves.

June 14.—"On Meldrum's Rules for Handling Ships in the Southern Indian Ocean." By Hon. Ralph Abercromby, F.R.Met.Soc. Communicated by R. H. Scott, F.R.S.

The results of this paper may be summarized as follows :—

The author examines critically certain rules given by Mr. C. Meldrum for handling ships during hurricanes in the South Indian Ocean, by means both of published observations and from personal inspection of many unpublished records in the Observatory at Mauritius. The result confirms the value of

Mr. Meldrum's rules; and the author then develops certain explanations, which have been partially given by Meldrum, adds slightly to the rules for handling ships, and correlates the whole with the modern methods of meteorology.

As an example, a hurricane is taken which blew near Mauritius on February 11, 12, and 13, 1861, and the history of every ship to which the rules might apply is minutely investigated. The result, dividing Meldrum's rules shortly into three parts, is as follows:—

Rule 1. Lie to with increasing south-east wind till the barometer has fallen 6-10ths of an inch. Seven cases, rule right in every case.

Rule 2. Run to north-west when the barometer has fallen 6-10ths of an inch. Three cases, two failures, one success.

Rule 3. Lie to with increasing north-east or east wind, and a falling barometer. Seven cases, rule right in every instance.

Rule 2 was exceptionally unfortunate in this case, as the path of the central vortex moved in a very uncommon and irregular manner. At the same time, in any case, it appears to be about equally hazardous to follow this rule or to remain hove to.

The following new statements are then examined in detail:—

The shape of all hurricanes is usually oval, not circular. An elaborate examination is made of hurricanes on 60 different days, in 18 different tropical cyclones in various parts of the world, with the following results:—

(1) Out of 60 days, cyclones were apparently circular on only four occasions, and then the materials are very scanty.

(2) The shape was oval on the remaining 56 days, but the ratio of the longer and shorter diameter of the ovals very rarely exceeded 2 to 1.

(3) The centres of the cyclones were usually displaced towards some one side. No rule can be laid down for the direction of displacement, and in fact the direction varies during the progress of the same cyclone. The core of a hurricane is nearly as oval as any other portion.

(4) The longer diameter of the ovals may lie at any angle with reference to the path of the cyclone; but a considerable proportion lie nearly in the same line as the direction of the path.

(5) The association of wind with the oval form is such that the direction of the wind is usually more or less along the isobars, and more or less incurved. This is the almost invariable relation of wind to isobars all over the world.

From an examination of the whole it is proved conclusively that *no rule is possible for determining more than approximately the position of the central vortex of a cyclone by any observations at a single station.*

The relation of a hurricane to the south-east trade is then discussed, and it is shown that there is always what may be called "a belt of intensified trade wind" on the southern side of a cyclone, while the hurricane is moving westwards. In this belt a ship experiences increasing south-east winds and squalls of rain, with a falling barometer, but is not within the true storm field. The difficulties and uncertainties as to handling a ship in this belt are greatly increased by the facts that the longer diameter of the oval form of the cyclones usually lies east and west, and that there is no means of telling towards which side of the oval the vortex is displaced.

The greater incurvature of the wind in rear than in front of hurricanes in the Southern Indian Ocean is next considered, and then facts are collected from other hurricane countries confirmatory of Meldrum's rules for the Mauritius.

Knipping and Doberck in the China Seas find little incurvature of the wind in front, but much in rear of typhoons.

Mr. Willson finds in the Bay of Bengal that north-east winds prevail over many degrees of longitude to the north, *i.e.* in front a cyclone; and this is analogous to the belt of intensified trade so characteristic of Mauritius hurricanes.

Padre Viñez finds at Havana that the incurvature of hurricane winds is very slight in front, and very great in rear.

The author then details further researches on the nature of cyclones, which bear on the rules for handling ships.

(1) Indications derived from the form and motion of clouds. It is shown that the direction of the lower clouds is usually more nearly eight points from the bearing of the vortex than the surface wind; but as the direction varies with the height of the clouds, and as this height can only be estimated, this fact is not of much value.

(2) Looking at the vertical succession of wind currents, if the march of the upper clouds over the south-east trade is more from the east, then the cyclone will pass to the north of the observer: but if the upper clouds move more from the south than the surface wind, then the hurricane will pass to the south of the observer.

(3) As to the form and position of clouds: so soon as the upper regions commence to be covered, the direction in which the cirrus veil is densest gives approximately the bearing of the vortex. Later on, the characteristic cloud bank of the hurricane appears, and the greatest and heaviest mass of the bank will appear sensibly in the direction of the vortex.

The irregular motion of the centre of a cyclone is next discussed, and it is shown that the centre often twists and sways about, in some cases even describing a small loop.

From this and other facts it is shown that the attempts which have been made—

(1) To estimate the track of a cyclone by projection.

(2) To estimate the distance of a ship from the vortex, either by taking into account the entire absolute fall, or by noting the rate of fall, can lead to no useful result.

A series of revised rules for handling ships in hurricanes is given. Comparing these with the older ones it will be remarked—

(1) That the rule for finding approximately the bearing of the vortex is slightly modified.

(2) That the great rules of the "laying to" tacks remain unaltered.

(3) That the greatest improvement is the recognition of the position and nature of the belt of intensified trade wind on the dangerous side of a hurricane, where a ship experiences increasing wind, without change of direction, and a falling barometer. The old idea that such conditions show that a vessel is then *necessarily* exactly on the line of advance of a hurricane is erroneous. She may, but she need not be; and under no circumstances should she run till the barometer has fallen at least 6-10ths of an inch.

(4) There are certain rules which hold for all hurricanes; but every district has a special series, due to its own local peculiarities. Those for the South Indian Ocean are given in this paper.

PARIS.

Academy of Sciences, July 23.—M. Janssen, President, in the chair.—The President announced the death, on July 19, of M. H. Debray, member of the Section of Chemistry, whose name will always be remembered in connection with the laws determining the tension of dissociation, the density of the vapour of sulphur, and other researches throwing much light on many obscure chemical phenomena.—Note on target practice, by M. J. Bertrand. In continuation of his previous communication (*Comptes rendus* of February 6, 1888), the author here shows that the actual results of 1000 experimental shots correspond closely with the theory as expressed by the general equation $k^2x^2 + 2\lambda xy + k'^2y^2 = H$. The practice was at a distance of 200 metres with ten rifles of like model, each marksman firing ten shots with each rifle.—Remarks on the quantitative analysis of nitrogen in vegetable soil, by MM. Berthelot and G. André. The analysis of nitrogen in ground containing nitrates presents some apparent difficulty. But the results of the researches here described show that in the case of ground poor in nitrates, the analysis may be safely and rapidly executed with a blend of lime and soda.—On the luminous bridges observed during the transits and occultations of the satellites of Jupiter, by M. Ch. André. As in the transits of Venus, these optical phenomena are here shown to be entirely due to the optical surfaces of the instruments modifying the direction of the luminous waves. They are, in fact, a result of diffraction in the instruments of observation.—Measurement of the coefficients of thermic conductibility for metals, by M. Alphonse Berget. The author here applies to red copper, brass, and iron, the same method he has already adopted for mercury (*Comptes rendus*, July 25, 1887, and July 16, 1888), with the following results: red copper, $k = 1{\cdot}0405$; brass, $k = 0{\cdot}2625$; iron, $k = 0{\cdot}1587$.—Magnetic determinations in the basin of the West Mediterranean, by M. Th. Moureaux. Having been charged by the Minister of Public Instruction to collect the elements needed for the preparation of magnetic charts for this region, the author obtained in the period from April 19 to June 25, 1887, as many as ninety measures of declination and fifty-nine of inclination for fifty-two stations. The results are here tabulated for these stations, of which four are in Corsica, three in Italy, two in Malta, one in Tripoli, seven in Tunisia, twenty-five in Algeria, one in Morocco, eight in Spain, and one in France. In a future communication will be given the magnetic charts constructed from these observations.—Analysis of the Nile waters, by M. A. Muntz. At the request of M. Antoine d'Abbadie, the author has examined several specimens with a view to determining the proportion of nitrates contained in these marvellously fertilizing waters. The results show that, while the proportion is variable, it does not exceed or even equal that found in the Seine and some other rivers. The analysis gives 4·02 mgr. per litre for nitric acid, which is derived partly from the soil, partly from the tropical rains which cause the periodical floods. The nitrates are not regarded as the chief cause of the great fertility of Egypt, which is more probably due to the chemical properties of the sedimentary matter deposited at each recurring inundation.—Researches on some salts of rhodium, by M. E. Leidié. The author here determines the constituents and formulas of the chloronitrate of rhodium and ammonia, the sulphate of rhodium sesquioxide, the oxalates of rhodium and potassium, of rhodium and sodium, of rhodium and ammonium, and of rhodium and barium.—On a new method of quantitative analysis for the lithine contained in a large number of mineral waters, by M. A. Carnot. The process here described is effected by means of the fluorides, and is based especially on their different degrees of solubility.—On the chloride, bromide, and sulphide of yttrium and sodium, by M. A. Duboin. The paper deals with the preparation and properties of the crystallized anhydrous chloride and bromide of yttrium, and the crystallized sulphide of yttrium and sodium.—On the quantitative analysis of glycerine by oxidation, by M. Victor Planchon. A detailed account is given (with further applications) of Messrs. Fox and Wanklyn's new process of analysis, based on the fact that glycerine, oxidized by the permanganate of potassa in a strong alkaline solution, is transformed to water, carbonic acid, and oxalic acid, according to the equation given in the *Chemical News* of January 8, 1886.—On anagyrine, by MM. E. Hardy and N. Gallois. The authors claim to have first discovered this extract of *Anagyris fœtida*, a poisonous leguminous plant ranging over the whole of the Mediterranean basin. They here describe its toxic properties, and determine the formula of anagyrine as $C_{14}H_{18}N_2O_2$.—Action of aniline on epichlorhydrine, by M. Ad. Fauconnier. Some months ago the author announced that he had obtained by the action of aniline on epichlorhydrine an oleaginous base, the chlorhydrate of which corresponds to the formula $C_{15}H_{20}N_2Cl_2O$. He has since prepared this base in the crystallized state, and has also obtained some derivatives, which have enabled him to determine its constitution and true formula, $C_3H_5(OH)(NH.C_6H_5)_2$. Instead of dianilglycerine, as first suggested, he now proposes to call this base oxipropylene-diphenyldiamine, which has the advantage of indicating its composition.—M. Pierre Zalocostas describes the constitution of spongine; MM. Arm. Gautier and L. Mourgues deal with the volatile alkaloids of cod-liver oil (butylamine, amylamine, hexylamine, dihydrolutidine); M. Massol gives a process for neutralizing malonic acid by means of the soluble bases; and M. H. Moissan describes the method of preparation and the properties of the fluoride of ethyl.

BERLIN.

Physiological Society, July 20.—Prof. du Bois-Reymond, President, in the chair.—Dr. Benda explained his views on the structure of striated muscle-fibres in connection with the statements recently laid before the Society by van Gehnchten. He took as his starting-point the wing-muscles of insects, which are composed of fibrillæ permeated by transverse partitions; each division of the fibre consists of a hollow cylinder of isotropic substance filled with contractile anisotropic material. The latter shrinks under the influence of reagents, leaving above and below a disk of isotropic substance. In the muscles of the body in insects, and in those of the higher animals, the isotropic disks of neighbouring fibrillæ are fused into continuous layers, between which the small cylinders of anisotropic substance run perpendicularly. When the muscles are resolved by the action of reagents into Bowman's disks, the cleavage of the fibrils takes place either across the anisotropic cylinders or the isotropic disks.—Dr. Heymans spoke on the relative toxicity of oxalic, malonic, succinic, and methyl-succinic acids, and of their sodium salts. He had been requested by Prof. Henry, who had studied the chemical and physical properties of these acids, to investigate the relative toxic action of this series of acids, and had found that the strongest acid—namely, oxalic—was the most poisonous. One milligramme of this acid sufficed to kill a frog; of malonic acid, whose physiological action, as well as that of methyl-succinic acid, had not been investigated, 2 to 3 mgr. were necessary; of succinic acid, 3 to 4 mgr.; and of methyl-succinic acid, 6 to 7 mgr. The toxic action of the acids diminished thus as the molecular weight increased. When the sodium salts of these acids were used instead of the free acids, the toxicity was the same in the case of oxalic acid, but was much less in the case of the other three acids.—Dr. Sklarek gave an account of the recently published observations of Weismann and Ischikawa on partial impregnation of the Daphnidæ.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Symons's British Rainfall 1887: G. J. Symons (Stanford).—Mediæval Researches from Eastern Asiatic Sources, 2 vols.: E. Bretschneider (Trübner).—My Microscope, second edition: by a Quekett Club Man (Roper and Drowley).—The Fauna of British India, Mammalia: W. T. Blanford (Taylor and Francis).—Schriften der Physikalisch-Ökonomischen Gesellschaft zu Königsberg i. Pr., 1887 (Königsberg).—Maps Nos. 3 to 7 to accompany Annual Report of the Geological and Natural History Survey of Canada, vol. ii. 1886 (Dawson, Montreal).—Fauna der Gaskohle und der Kalksteine der Permformation Böhmens; Band ii. Heft 3, Die Lurchfische, Dipnoi: Dr. Ant. Fritsch (Prag).—Beobachtungs-Ergebnisse der Norwegischen Polarstation Bossekop in Alten, ii. Theil (Grondahl, Christiania).—The Education of the Imagination: C. H. Hinton (Sonnenschein).—Many Dimensions, C. H. Hinton (Sonnenschein).—Die Süsswasserbryozoen Böhmens: J. Kafka (Prag).—Archives Italiennes de Biologie, Tome x. Fasc. i. (Turin).—Journal of the Trenton Natural History Society, No. 3 (Trenton, N.J.).—Bulletin de la Société Impériale des Naturalistes de Moscou, No. 2, 1888 (Moscou).—Transactions of the New Zealand Institute, vol. xx., 1887.

CONTENTS.

THURSDAY, AUGUST 16, 1888.

CELTIC HEATHENDOM.

The Origin and Growth of Religion as Illustrated by Celtic Heathendom. The Hibbert Lectures for 1887. By J. Rhŷs. (London: Williams and Norgate, 1888.)

PROF. RHŶS has made an important contribution in this volume, if not to the development of religion in general, at all events to the study of Indo-European mythology. Almost for the first time, the religious legends of the Kelts have been subjected to scientific treatment, and the resources of scientific philology have been called in to explain them. The Keltic languages and mythology have long been a happy hunting-ground for the untrained theorist and charlatan: in the Hibbert Lectures for 1887 we find at last etymologies which can be trusted, and a method of investigation which alone can lead to sound results.

The method employed by Prof. Rhŷs is the comparative method of science. The literature of the Keltic nations does not begin until after the triumph of Christianity; and apart from a few Gaulish inscriptions, and the questionable assertions of Latin or Greek writers, our knowledge of Keltic paganism must be derived from such traces of it as we may detect in a later and hostile literature. These traces consist for the most part of the myths and legends preserved in Irish manuscripts or Welsh romances.

By comparing the Irish and Welsh legends one with another, and analyzing the primitive meaning of the proper names round which they centre, Prof. Rhŷs has attempted to recover their original form and signification, verifying his conclusions not only by an appeal to etymology, but also, wherever it is possible, to the evidence of the Gaulish texts. Without doubt, a considerable number of his conclusions are merely hypothetical, and in some cases his interpretations depend on the exercise of the same Keltic imagination as that which inspired the old story-tellers, but, on the whole, he has laid a broad and solid foundation of fact, which must be the starting-point of all future researches in the same field. He will himself be the first to acknowledge the tentative and theoretical character of much of his work; indeed, the readiness with which he admits in his appendix that he has changed his opinion in regard to certain questions is a witness to his possession of the true scientific spirit, which is always open to conviction.

The lectures appropriately begin with an account of Gaulish religion, so far as it can be gathered from the scanty evidence of the monuments. Then follow chapters on the Zeus of the insular Kelts, as well as on the Culture-hero and on the Sun-hero, the two latter of whom Prof. Rhŷs endeavours to keep apart, though the attempt does not seem to me to be more successful than it has been in the case of other mythologies. The suggestion, indeed, that the Keltic Culture-hero may have been a deified man, like the Norse Woden, the Greek Prometheus, or the Indian Indra, has little in its favour; at all events, if Indra or Prometheus were of human origin, the Sun-god must have been of human origin also. The myths told about "the Culture-hero" are precisely similar in character to those told about "the Sun-hero."

The last lecture is occupied with those figures of Keltic mythology which are not directly connected with either the beginnings of civilization or the adventures of the solar orb. Here Prof. Rhŷs has done important service for the historian by sweeping away the foundations on which the so-called early history of Ireland has been built. The races who have been supposed to have successively effected a settlement in the island belonged to the world of mythology. The Tuatha dé Danann, or "Tribes of the goddess Danu," were long remembered to have been the fairies; the Fomorians, or "submarine" monsters, were supernatural beings whose home was beneath the sea; and a human ancestry is denied even to the Fir-bolgs or "Men of the Bag." I am not sure that Prof. Rhŷs does not sometimes go too far in refusing an historical character to the personages and events recorded in Keltic tradition; the recent revelations of early Greek archæology are a useful warning in this respect, and the Keltic Professor himself is obliged to admit that by the side of the mythical Emrys and Vortigern there were an historical Ambrosius and an historical Vortigern. A story must have a setting in time and place, and the internecine quarrels of the lively Kelt afforded frequent opportunities for attaching an old story to the heroes and circumstances of the day. It is not so many years ago since Atreus and Agamemnon were relegated to the domains of mythology; yet we now know, from archæological exploration, that the legends in which they figured were based on historical fact.

In a book so rich in new ideas and information it is difficult to select anything for special notice. Bearers of the name of Owen, however, will be interested by finding it traced back to the Gaulish agricultural god Esus, whose name is connected by Prof. Rhŷs with the Norse *áss*, "a god," and the Professor is to be congratulated on his discovery of the origin of King Lud, the Lot of the Arthurian romances. Lud is the Welsh Llûdd, in Old Welsh Lodens, who bears the title of Llûdd Llawereint, or "Lud of the Silver Hand." The initial sound of Llûdd, however, is due to that of the epithet so constantly applied to him, the primitive form of the name having been Nûdd, which appears in the Latin inscriptions of Lydney as Nodens or Nudens, a sort of cross between the Roman Mars and Neptune. Nodens, again, was the Irish Sky-god, "Nuada of the Silver Hand," and a myth was current which explained the origin of the title.

Equally worthy of notice is what Prof. Rhŷs has to tell us about "the nine-day week" of the ancient Kelts. He shows that like the Latins they made use of a week of nine nights and eight days, and he points out that traces of a similar mode of reckoning time are to be found in Norse literature. Whether he is right in ascribing the origin of such a week to a habit of counting the fingers of one hand admits of question, and I do not see how the Irish divinity Maine who presided over the day of the week can be the Welsh Menyw, if, as we are told, Maine owes his origin to *secht-main*, itself borrowed from the Latin *septimana* or seven-day week. Prof. Rhŷs believes that he has found a further resemblance between the calendar of the primitive Kelts and Scandinavians, in the fact that the year in both cases began at the end of the autumn. But no argument can be drawn from the fact in favour of the theory which places the primæval seat of the Aryan race within the Arctic Circle, since the civil year of the Jews also began with the ingathering of the harvest at the time of the autumnal equinox, and no one would propose to transfer their forefathers to the distant north.

The points of likeness between the mythologies and religious conceptions of the Kelts and Scandinavians, to which Prof. Rhŷs has drawn attention, are numerous and striking. How many of them go back to an age when the ancestors of the Scandinavians and of the Aryan Kelts still lived together it is impossible to tell, but several of them can most easily be explained as due to borrowing. It is now well established that Norse mythology and religion were influenced not only by Christianity but also by the mythology and religion of the Kelts, with whom the Norsemen came into contact in the Hebrides, in Ireland, and in the Channel Islands, and in a comparison between Keltic and Scandinavian legends this influence must always be allowed for.

I must not part from Prof. Rhŷs's learned and important lectures without exercising the privilege of a reviewer by objecting to certain of his conclusions. These relate to the Keltic allusions to a Deluge, and to the stories of a contest between the gods and the monsters of the lower world. Whatever may be the origin of the Keltic myths which are supposed to refer to such events, they cannot be compared with the Indian legend of the deluge of Manu or with the story of the conflict between the gods of Olympos and the Titans. It has long since been pointed out by Lenormant that the Indian legend was borrowed from Babylonia; and it's hero, Manu, has nothing to do with the Kretan Minôs. Apart from the unlikeness of the vowel in the first syllable of the two names, Minôs seems to be a word of Phœnician origin. The conflict between the gods and the Titans, again, has now been traced to Babylonia. Like the twelve labours of Herakles, the Babylonian epics have been recovered in which the story appears in its earliest form, before it was passed on to the Greeks through the hands of the Phœnicians. The Titans and Herakles were alike figures of Semitic, and not of Aryan, mythology.

I have left myself space to do no more than draw attention to two very interesting questions suggested by Prof. Rhŷs's lectures. It is in Scandinavian rather than in Latin mythology that he finds parallels to the myths and legends of the Kelts. Nevertheless, linguistic science teaches us that the Keltic dialects had most affinity to Latin and not to the Scando-Teutonic languages. Was Latin mythology, then, so profoundly modified by some foreign system of faith, such as the Etruscan, as to have lost a considerable part of its original character even before it passed under the influence of the Greeks? Was it, in fact, Etruscanized before it was Hellenized? The other question relates to the causes which have reduced the gods of a former age to the human kings and princes of later Keltic legend. The same transformation characterizes the traditions of ancient Persia, as it also characterizes Semitic tradition. In the case of Persia, such unconscious euhemerism seems to have been brought about by a change of creed. Was this also the reason why in Keltic story the ancient Sky-god became Nuada of the Silver Hand? If so, the old theology would have remained practically unchanged until the conversion of its adherents to Christianity, and the growth of most of the mythology beneath which Prof. Rhŷs has discovered the forms of dishonoured deities would have taken place in the centuries which immediately followed the fall of the Roman Empire. They are the same centuries, be it remembered, which divide the history of Britain into two portions, separated from one another by a veil of myth.

A. H. SAYCE.

HAND-BOOK OF THE AMARYLLIDEÆ.

Hand-book of the Amaryllideæ. By J. G. Baker, F.R.S. 203 pp. (London: George Bell, 1888.)

SINCE Herbert's "Amaryllidaceæ," published in 1837, there has not been any work brought out containing descriptions of all or approximately all the species of Amaryllidaceous plants until the appearance of this little work. Herbert's volume has long been both rare and out of date, and some such book as the present was a desideratum. Neither could anyone be found who has a better or more extensive knowledge of the bulbous plants than Mr. Baker, whose monographs of the Liliaceæ and Iridaceæ are well known to all lovers of these groups. The work before us is the result of twenty-three years' study, and embodies descriptions drawn up not only from herbarium material, but especially from living plants—some grown at Kew Gardens, others from the conservatories and gardens of professional and amateur cultivators. It is intended as a working hand-book for gardeners and botanists, and as such seems suited for its purpose.

The group of Amaryllideæ is one which has suffered in popularity from the modern rage for Orchids. A glance at the volume will show that many species were introduced into cultivation from fifty to a hundred years ago, and are now quite lost from our gardens. In those days Cape bulbs were very popular; and Masson at the close of the last century, and Cooper and others in later years, introduced many beautiful and curious plants now known to us only by their dried specimens and drawings. Of these the curious South African genus Gethyllis is a striking example, six out of the nine species here described being only known from Masson's sketches and specimens, and this in spite of the numerous careful and energetic collectors we have now at the Cape of Good Hope.

One reason for this disappearance of species is the very narrow limits of their distribution in many cases, although it appears that the individuals are often abundant when the right locality is reached. Witness, for example, the little Tapeinanthus of Spain and Morocco, discovered by Cavanilles in 1794, and lost again till two years ago, when it was re-discovered in profusion by Mr. Maw, who has stocked our gardens with it; and very similar are the cases of the strange green-flowered Narcissus of Gibraltar and the Lapiedra, known to Clusius as early as 1574, and still a great rarity even in herbaria at the present day. When it is remembered that these three plants grow in localities close to our own shores, it is not surprising that many of the more distant South African species figured by Jacquin in his sumptuous works, as well as many Andean and Peruvian species, are still absent from our gardens and houses.

Besides the rarity of some of these plants, they have a habit of entirely disappearing after flowering, and indeed in many cases they will only appear at irregular and long intervals, which also conspires to make them difficult to procure, so that collectors are necessarily anxious to know the time of the year at which they should be looked for in flower, and this the author has where possible added to his description.

The volume includes, besides the typical Amaryllideæ, the Alstrœmeriæ and Agaveæ, but the Hypoxideæ and Vellozieæ are omitted on the grounds that they have been elsewhere fully dealt with. This we think a pity, as it would have made the work more complete to have included these groups; but this will hardly affect cultivators, with whom the Hypoxids are rarely found favourites on account of their comparatively insignificant flowers and general similarity, while the Vellozias, though they would be welcome additions to our stoves on account of their beautiful flowers, yet baffle our gardeners on account of their bulkiness and slow growth.

In the Agaveæ it will be noticed that of many species (in fact, nearly one-third) only the foliage is known. For garden purposes perhaps the form and number of the leaves may be sufficient, at least for identification; but it cannot be considered satisfactory to publish as new species, and endow with scientific names, plants of which the inflorescence is unknown. The author, however, has but done his duty in incorporating these species into his work.

One may hope that the publication of this compendium will stimulate our amateur gardeners to turn their attention more carefully to this comparatively neglected group. Already for some time signs have not been wanting to show that they are rising into favour again to some extent. The Narcissi, Hippeastrums, and Crinums are undergoing elaborate cultivation and hybridization by the best of our gardeners with the highest success, and if this hand-book contributes to the study of this group it will have done its work.

H. N. R.

OUR BOOK SHELF.

Another World; or, The Fourth Dimension. By A. T. Schofield, M.D. (London: Swan Sonnenschein, 1888.)

THIS work consists of seven chapters. The first four—the land of no dimensions, the land of one dimension, the land of two dimensions, and the land of three dimensions —consist of large extracts from "Flatland," with a running commentary upon them, bringing out their salient facts. Indeed, had not "Flatland" been published, the author admits his own book would not have been written. In Chapter V., the land of four dimensions is mathematically considered, and here we have stated, from analogy, the relations of a being in one dimension with that above him and its inhabitants, *e.g.* one in the third dimension (our world) with the fourth; and in Chapter VI. the land of four dimensions is considered in relation to ours of three. Chapter VII. considers generally the land of four dimensions, with facts and analogies. The fourth dimension is not discussed on the lines of Mr. Hinton's "What is the Fourth Dimension?" but after the mathematical side of the question has been considered, our author "further considers the actual facts around us bearing on the question, and compares the deduced laws of the fourth dimension with some of the claims of Christianity as stated in the Bible." Here we must close our notice—as we cannot go into an examination of these topics in our columns—with saying that there is much of interest in the pages before us, and for some readers the speculations of the later chapters may have as much interest as the mathematical certainties of the earlier chapters have for others.

Euclid's Method, or the Proper Way to Treat on Geometry. By A. H. Blunt. (Shepshed: Freeman, 1888.)

THIS booklet consists of an introduction (pp. 3–10), and the method of treating on geometry (pp. 10–23). We let the writer speak for himself:—"In this small work I have attempted to show the proper way to treat on geometry, and which I conceive was the method of Euclid; for it will be seen that the results are right from the way in which they are arrived at, and that they agree with Euclid's results. It is certain, I think none will deny, that when Euclid composed his 'Geometry,' he did everything in it under the guidance of reason and knowledge of what the true method consists in; but since he has not expressed or shown those reasons (and it would not have been proper, nor would it have been necessary to have done so in his 'Geometry'), they therefore appear to have been known but very little to anyone else since his time, as is evident from the expressions and unjust fault-finding made against him in the writings of modern geometers, which greatly betray their own ignorance of the true method. So long as the true method remains unknown, it is not to be wondered at that men should busy themselves in finding faults with Euclid, his work being so complete and perfect as to leave them but little else to do. Not that I would be understood to mean that his works ought to be accepted in blind submission as everything perfect, or that no faults, if there are any in it, ought to be pointed out"; and so on. *Ex pede Herculem!* The author's remarks are made sincerely, and for a certain order of mind his explanations are likely to clear up many points in the Definitions. It is to these only that he confines his attention in pp. 10–23, and he gives good reasons why Euclid should have taken them in the order he has taken them. This was his object: write, then, Q.E.D., and *Vivat* Euclides!

On the Distribution of Rain over the British Isles during the year 1887. Compiled by G. J. Symons, F.R.S. (London: Edward Stanford, 1888.)

MR. SYMONS'S "British Rainfall" is so well known that we need only say of the present issue that it is in no respect inferior to the preceding volumes of the series. The marked characteristic of 1887 was the prevalence of droughts. According to Mr. Symons, the year has had no equal for widespread deficiency of rainfall since 1788. Naturally, therefore, much space is devoted in this volume to the subject of droughts; and in one chapter—on "Historic Droughts"—he has brought together, from a variety of sources, a large amount of information that ought to be as interesting to historians as to meteorologists. In the preface Mr. Symons calls special attention to important additions which have been made to our knowledge of the rainfall of the Lake District. These have resulted from a grant of £42 7*s.* made by the Royal Society from its own funds in 1886.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The "Tamaron" of the Philippine Islands.

A LETTER, which I have just received from our Corresponding Member, the energetic traveller and naturalist, Prof. J. B. Steere, of Ann Arbor, Michigan, U.S.A., announces that he has made a remarkable zoological discovery in the Philippine Islands. In the interior of the little-known Island of Mindoro he has procured specimens of a strange animal, which, although much talked of in the Philippines, is little, if at all, known elsewhere. This is the *Tamaron* of the natives, a wild species of the family Bovidæ, allied to the Anoa of Celebes, which Prof. Steere proposes to call *Anoa mindorensis*. Its general colour is black, the hairs being short and rather fine. A greyish white stripe runs from near the inner corner of the eye towards the base of the horn. There is also a greyish white spot above the hoof on all the feet, and a greyish white patch on the inside of the lower fore-leg. The height of the male at the shoulder is about 3 feet 6 inches, the length from the nose to the base of the tail about 6 feet 8 inches. The horns are about 14 inches long. Prof. Steere obtained two males and one female of this animal, of which his full description will be read at the first meeting of the next session of the Zoological Society. The discovery is of much interest, as giving an additional instance of the similarity between the faunas of Celebes and the Philippines, which was already evident from other well-known cases of parallelism between the natural products of these two countries.

P. L. SCLATER.

Functionless Organs.

IN reference to the Duke of Argyll's letter, I should wish to say that I am not aware of any reason for regarding the electric organ of any Skate as a "prophetic structure," using that term in the sense given to it by the Duke. And I should be very glad if he, instead of confining himself to a simple assertion that it is so, would explain the reasons which lead him to regard it as being so. It might then be possible to combat those reasons.

Further, I think it is only right to say that my own observation of the progress of the doctrine of evolution during the last quarter of a century leads me to a conclusion diametrically opposed to the Duke's in regard to the balance of evidence in favour of, or opposed to, the doctrine of creative design in variations on the one hand, and that of the non-significance of variations on the other hand.

I do not hesitate to say that what may be called "pure" Darwinism—the doctrine of the origin of species by the natural selection in the struggle for existence of non-significant congenital variations—is everywhere being more completely demonstrated by reasoning and observation as the single and sufficient theory of that origin; to the exclusion of Lamarckism, and still more certainly to the exclusion of any vestige of the doctrine of design. E. RAY LANKESTER.

45 Grove End Road, N.W., August 4.

WITH a certain class of thinkers, when endeavouring to disparage the labours of Charles Darwin, no argument appears absurd. Does the Duke of Argyll, in his letter which appeared in your last issue (p. 341), mean to imply by his "prophetic germs" that such cases as the mammæ in the male indicate a time when he will be able to take part with the female in suckling the young, and that the coccyx is prophetic of a tail to the human family, or that a time is approaching when the rudimentary covering of hair on the human body will develop into a warm coat similar to that of the bear or the beaver? For myself, I fail to see how a "functionless organ" can build *itself* up. Perhaps the Duke of Argyll will explain. J. T. HURST.

Raymond Villa, Geraldine Road, Wandsworth, S.W., August 11.

Dr. Romanes's Article in the "Contemporary Review."

ABSENCE from England has hitherto prevented me from seeing Mr. Poulton's letter in your issue of July 26 (p. 295). Having just read it, I am not a little surprised that he should have deemed it necessary to refer me to the titles of two of the most notorious essays in the recent literature of Darwinism. Nor can I fail to wonder that, without a particle of evidence, he should accuse any man of "not making himself acquainted with views which he professes to express."

If I could think it worth while to discuss a somewhat lengthy matter with a critic of this kind, it would be easy enough to justify the incidental remark in my paper to which he has drawn attention. But my only object in noticing his criticism is to observe that, if its tone is due to his supposing that I have not sufficiently appreciated the importance of his own experiments in this connection, he is entirely mistaken. For, although I do not agree with his theoretical interpretation of them, it has always appeared to me that the experiments themselves are among the most valuable which have hitherto been made regarding the causes of variation. But it has also appeared to me that my appreciation of their importance in this respect depends upon what he calls "the Lamarckian conception," *i.e.* a conception which he expressly repudiates. Were it not for the attitude of theory which he thus adopts, of course I should not have alluded to him as a naturalist who concerns himself less with the causes of variation than the other (or Lamarckian) writers whom I had occasion to name. But, as the matter stands, I have merely forestalled the expression of his opinion as stated by himself, where he says in his letter to you, "I agree with Dr. Romanes in the belief that my work does not throw any light upon the causes of variation."

My paper was concerned only with the opinions of others, and I nowhere expressed the "belief" thus attributed to me. In point of fact, "the Lamarckian conception" enables me to hope that work of the kind on which Mr. Poulton is engaged is more calculated than any other to throw light upon the problem in question; and it seems to me a curious corroboration of the remark to which he objects that, on account of his loyalty to the school of Weismann, he is obliged to regard his own experiments as destitute of significance in this respect.

August 9. GEORGE J. ROMANES.

Taxation in China.

NATURE (vol. xxxvii. p. 269), in its review of M. Sîmon's "China: its Social, Political, and Religious Life," represents on that author's authority that in China "taxation is very light—not one-hundredth part of what it is in France," a statement so misleading to publicists, so illusive to economic science, that I take upon myself the task of exposing its fallacy, both as regards direct and indirect taxation.

Taking for illustration the amount of taxation at Ningpo (M. Sîmon was the efficient Consul of his country at that port, where he won golden opinions of foreigners generally, and natives as well), it will be seen that he has been led into egregious errors by incompetent interpreters.

M. Sîmon says that "five francs per *hectare* is the utmost that is paid for the best land."

From municipal archives I tabulate the following relative to the three qualities of rice land:—

Quality of Land.	Relative Quantity.	Taxation per Mou.	Taxation per hectare.	Taxation per acre.
1st	60% ...	$0.35 ...	$5.25 ...	$2.10
2nd	25 ...	0.28 ...	4.20 ...	1.68
3rd	15 ...	0.25 ...	3.75 ...	1.50
Average ...	100	$0.29\frac{1}{3}$	4.40	1.76

Six mou = one acre. Fifteen mou = one hectare.

Hill land, $0.13 per mou. From the second quality only one crop is obtained.

Instead, therefore, of the *best land* being five francs per *hectare*, it is (according to present rate of exchange) about 21 francs, and for the average about 17 francs per *hectare*.

With regard to indirect taxation, that author affirms that the Chinaman has no excise duties to pay. So far from that being the case, his octrois (*likin*) contribute far more to the State demands than the levies on his land; but from lack of trustworthy data, that is altogether an incomputable quantity.

Nevertheless, with such levies, and the salt gabel and so forth, it may be shown that the Chinese are not overburdened with taxation; albeit to imagine that their taxation is "not one-hundredth part of what it is in France" is sheer economic hallucination. D. J. MACGOWAN.

Wenchan, June.

Partial Eclipse of August 7.

THE above eclipse was observed at Cambridge, and the times of contact were estimated as follows:—

	h.	m.	s.	
First contact ...	6	44	50	G.M.T.
Last contact ...	7	7	20	,,

At the time of greatest eclipse, 6h. 56m., a photograph was taken which on being measured gives a magnitude of about

0·021, but this is only a rough approximation. The co-ordinates of the Observatory are—

28·6s. E.
52° 12′ 10″ N.

A. C. CROMMELIN.

Trinity College, Cambridge, August 10.

Macclesfield Observations.

MANY years ago, in studying Rigaud's "Bradley," I was impressed by several references to extensive series of observations with transit and quadrant made at the observatory of Shirbourn Castle, some of which Bradley evidently thought worthy of comparison with his own inaccuracy. It has often occurred to me that these observations, if the records still exist, may well be worthy of as thorough a reduction as has been given to those of other early astronomers. Perhaps some of your readers can tell us something about these records of 1739-89.

CLEVELAND ABBE.

Washington, July 30.

A Lunar Rainbow.

WET Mountain Valley in Colorado is situated some 8000 feet above the sea, and is surrounded by mountains, the Sangre de Cristo Range, on the western side, rising to some 14,000 feet in its highest peaks. For the last few days we have had a succession of thunderstorms—dark clouds pouring forth abundant rain—which have mostly swept along the range, leaving the valley clear, and often in sunshine. Last night, at 9 p.m., there passed just such a storm, while the full moon shone brightly from the east, where it had just risen. The result was a lunar rainbow—part only of the arc, a distinct band of light, in which the several colours were hardly to be observed. The phenomenon, which was new to me and must surely be rare, lasted only about a quarter of an hour, when the storm passed on.

West Cliff, Colorado, July 25. T. D. A. COCKERELL.

GLOBULAR STAR CLUSTERS.

PHYSICAL aggregations of stars may be broadly divided into "globular" and "irregular" clusters. Although, as might have been expected, the line of demarcation between the two classes is by no means sharply drawn, each has its own marked peculiarities. We shall limit our attention, in the present article, to the first kind.

The particles of a drop of water are not in more obvious mutual dependence than the components of these objects—"the most magnificent," in the elder Herschel's opinion, "that can be seen in the heavens." Were there only one such collection in the universe, the probability of its separate organization might be reckoned "infinitely infinite"; and no less than one hundred and eleven globular clusters were enumerated by Sir John Herschel in 1864. It does not, however, follow that the systems thus constituted are of a permanent or stable character; the configuration of most of them, in fact, points to an opposite conclusion.

There may, of course, be an indefinite number of arrangements by which the dynamical equilibrium of a "ball of stars" could be secured; there is only one which the present resources of analysis enable us distinctly to conceive. This was adverted to, many years since, by Sir John Herschel. Equal revolving masses, uniformly distributed throughout a spherical space, would, he showed, be acted upon by a force varying *directly* as the distance from the centre. The ellipses described under its influence would then all have an identical period: whatever their eccentricities, in whatever planes they lay, in whatever direction they were traversed, each would remain invariable; and the harmony of a system, in which no perturbations could possibly arise, should remain unbroken for ever: provided only that the size of the circulating bodies, and the range of their immediate and intense attractions, were insignificant compared with the spatial intervals separating them ("Outlines of Astronomy," 9th ed., p. 636).

But this state of nice adjustment is a mere theoretical possibility. There is no sign that it has an actual existence in Nature. The stipulations, upon compliance with which its realization strictly depends, are certainly disregarded in all stellar groups with which we have any close acquaintance. The components of these are neither equal, nor equally distributed. Central compression, more marked than that due simply to the growth in depth inward of the star strata penetrated by the line of sight, is the rule in globular clusters. The beautiful white and rose-tinted one in Toucan shows three distinct stages of condensation; real crowding intensifies the "blaze" in the middle of the superb group between η and ζ Herculis; in other cases, the presence of what might be called a nuclear mass of stars is apparent. Here, then, the "law of inverse squares" must enter into competition with the "direct" law of attraction, producing results of extraordinary intricacy, and giving rise to problems in celestial mechanics with which no calculus yet invented can pretend to grapple.

Sir John Herschel allowed the extreme difficulty of even imagining the "conditions of conservation of such a system as that of ω Centauri or 47 Toucani, &c., without admitting repulsive forces on the one hand, or an interposed medium on the other, to keep the stars asunder" ("Cape Observations," p. 139). The establishment, however, in such aggregations of a "statical equilibrium," by means of this "interposed medium," is assuredly chimerical. The hypothesis of their rotation *en bloc* is countenanced by no circumstance connected with them. It is decisively negatived by their irregularities of figure. These objects are far from possessing the sharp contours of bodies whirling round an axis. Their streaming edges betray a totally different mode of organization.

Globular clusters commonly present a radiated appearance in their exterior parts. They seem to throw abroad feelers into space. Medusa-like, they are covered with tentacular appendages. The great cluster in Hercules is not singular in the display of "hairy-looking, curvilinear" branches. That in Canes Venatici (M 3) has "rays running out on every side" from a central blaze, in which "several small dark holes" were disclosed by Lord Rosse's powerful reflectors (Trans. Roy. Dublin Society, vol. ii. p. 132, 1880); showing pretty plainly that the spiral tendency, visible in the outer regions, penetrates in reality to the very heart of the system. From a well-known cluster in Aquarius (M 2), "streams of stars branch out, taking the direction of tangents" (Lord Rosse, *loc. cit.* p. 162). That in Ophiuchus (M 12) has stragglers in long lines and branches, noticed by the late Lord Rosse to possess a "slightly spiral arrangement." Herschel and Baily described a remarkable group in Coma Berenices (M 53) as "a fine compressed cluster with curved appendages like the short claws of a crab running out from the main body" (Phil. Trans., vol. cxxiii. p. 458).

We find it difficult to conceive the existence of "streams of stars" that are not *flowing*; and accordingly the persistent radial alignment of the components of clusters inevitably suggests the advance of change, whether in the direction of concentration or of diffusion. Either the tide of movement is setting inward, and the "clustering power" (to use Sir William Herschel's phrase) is still exerting itself to collect stars from surrounding space; or else a centrifugal impulse predominates, by which full-grown orbs are driven from the nursery of suns in which they were reared, to seek their separate fortunes, and lead an independent existence elsewhere. It would be a childish waste of time to attempt at present to arrive at any definite conclusion on so recondite a point; but if the appeal to "final causes" be in any degree admissible, it may be pointed out that mere blank destruction and the eventual collapse of the system would seem to be involved in the first supposition, while the second implies the progressive execution of majestic and profound designs.

After the lapse of some centuries, photographic measurements will perhaps help towards a decision as to whether separatist or aggregationist tendencies prevail in clusters. Allowance will, however, have to be made, in estimating their results, for the possible movements of recession or approach of the entire group relatively to the solar system, by which perspective effects of closing up or of opening out would respectively be produced.

Inequalities of brightness, to the extent of three or four magnitudes, are usually perceptible among the lustrous particles constituting these assemblages. Nor are their gradations devoid of regularity and significance. Generally, if not invariably, the smaller stars are gathered together in the middle, while the bright ones surround and overlay them on every side. Thus, the central portion of the magnificent Sagittarius cluster (M 22) accumulates the light of multitudes of excessively minute stars, and is freely sprinkled over with larger stars. The effect, which probably corresponds with the actual fact, is as if a globe of fifteenth magnitude were inclosed in a shell of eleventh magnitude stars, some of these being naturally projected upon the central aggregation. Sir John Herschel remarked of a cluster in the southern constellation of the Altar ("Gen. Cat." 4467): "The stars are of two magnitudes; the larger run out in lines like crooked radii, the smaller are massed together in and around the middle" ("Cape Observations," p. 119). A similar structure was noted by Webb in clusters in Canes Venatici (M 3), in Libra (M 5), and in Coma Berenices (M 53) ("The Student," vol. i. p. 460). Here, again, we seem to catch a glimpse, from a different point of view, of a law connecting growth in mass and light with increase of tangential velocity—consequently, with retreat from the centre of attraction; and the assumption of an outward drift of *completed* suns gains some degree of plausibility.

Irregularities of distribution in clusters take a form, in some instances, so enigmatical as to excite mere unspeculative wonder. At Parsonstown, in 1850, three "dark lanes," meeting at a point considerably removed from the centre, were perceived to interrupt the brilliancy of the stellar assemblage in Hercules. They were afterwards recognized by Buffham and Webb, and are unmistakable in one (at least) of Mr. Roberts's recent photographs of that grand object. The clusters in Ophiuchus, in Canes Venatici, and in Pegasus ("G. C." 4670) are similarly *tunnelled*. Preconceived ideas as to the mechanism of celestial systems are utterly confounded by appearances not easily reconcilable, so far as we can see, with the prosecution of any orderly scheme of circulatory movement. Even if absolutely vacant, the extensive clearings indicated by the phenomenon of dusky rifts, must of course, in globular clusters, be partially obliterated by the interposed light of the surrounding star-layers. They can hence become perceptible only when their development is most fully pronounced; and, in a less marked shape, may exist in many clusters in which they defy detection.

The apparent diameter of the cluster in Hercules, including most of its branches, is 8′; that of its truly spherical portion may be put at 5′. But since the sine of an angle of 5′ is to radius about as 1 : 687, it follows that the real diameter of this globe of stars is 1/687 of its distance from the earth. Assuming this distance to be such as would correspond to a parallax of 1/20 of a second, we find that the more compact part of the cluster measures 558,000,000,000 miles across. Light occupies about thirty-six days in traversing it. The average brightness of its components may be estimated at the twelfth magnitude; for, although the outlying stars are of the tenth and eleventh ranks, the central ones are, there is reason to believe, much fainter. The sum total of their light, if concentrated into one stellar point, would at any rate very little (if at all) exceed that of a third magnitude star. And one third magnitude star is equivalent to just 4000 stars of the twelfth magnitude. Hence we arrive at the conclusion that the stars in the Hercules cluster number about 4000; and that Sir William Herschel, in estimating them at 14,000, erred considerably on the side of excess.

If, then, 4000 stars be supposed uniformly distributed through a sphere 558,000,000,000 miles in diameter, an interval of 28,365,000,000 miles, or more than ten times the distance from Neptune to the sun, separates each from its nearest neighbour.[1] Under these circumstances, each must shine with about one thousand times the lustre that Sirius displays to us. Since, however, five millions of stars even of this amazing brilliancy would be needed to supply the light we receive from the sun, the general illumination of the cluster can only amount to a soft twilight, excluding, it is true, the possibility of real night on any globe situated near its centre.

At the distance conjecturally assigned to this cluster, our sun would appear as a seven and a half magnitude star; it would shine, that is to say, about sixty-three times as brightly as an average one of the grouped objects. Each of these, accordingly, emits 1/63 of the solar light; and if of the same luminosity, relative to mass, as the sun, it exercises just 1/500 of the solar attractive power. The mass of the entire system of 4000 such bodies is thus equal to that of eight suns. This, however, may be regarded as a minimum estimate. The probabilities are in favour of the cluster being vastly more remote than we have here assumed it to be; hence proportionately more massive, and composed of brighter individual bodies than results from our calculation. Differences of distance are alone adequate to account for the variety of *texture* observable in globular clusters. That in Aquarius, for instance, compared by Sir John Herschel to "a heap of golden sand," might very well be the somewhat coarse-grained Hercules group withdrawn as far again into space. At a still further stage of remoteness, the appearance would presumably be reached of a stellar throng in the Dolphin ("G. C." 4585), which, with low powers, might pass for a planetary nebula, but under stronger optical compulsion assumes the granulated aspect of a true cluster. And many such, their genuine nature rendered impenetrable by excessive distance, are doubtless reduced to the featureless semblance of "irresolvable" nebulæ.

But there are real as well as apparent diversities in these objects. Although smaller and more compressed clusters must, on the whole, be more remote than large, looser ones, yet "this argument," Sir William Herschel remarked, "does not extend so far as to exclude a real difference which there may be in different clusters, not only in the size, but also in the number and arrangement of the stars." There may be globular clusters with components of the actual magnitude of Sirius; others, optically indistinguishable from them, may be aggregated out of self-luminous bodies no larger than Mars, or even than Ceres, or Pallas. There is, indeed, a strong likelihood that clusters and nebulæ form an unbroken series—that swarms of meteorites are connected by such interminable gradations with swarms of suns, as to admit of no impassable barrier being set up between them.[2] The rifted structure, for instance, and truncated spectrum of the Hercules cluster bring it into unmistakable relations with the great nebula in Andromeda; yet it is scarcely doubtful that the one object is an assemblage of orbs each of them, quite possibly, the rival of our sun in lustre; and the other, a collection of what we can only describe as cosmical shreds and particles. Further analogies emerge to view through the reproduction in many nebulæ of the "hairy" appendages of globular clusters, and in the spirality of arrangement characteristic of both classes

[1] See J. E. Gore's similar calculation, based, however, on different data from those assumed above, in Journal Liverpool Astr. Soc. vol. v. p. 169.

[2] See Mr. Lockyer's "Bakerian Lecture," p. 29.

of object. These strange and, at present, unaccountable resemblances will probably be developed and possibly be interpreted by future investigations.

A. M. CLERKE.

TIMBER, AND SOME OF ITS DISEASES.[1]

XI.

IT may possibly be objected that the subject of the present paper cannot properly be brought under the title of these articles, since the disease to be discussed is not a disease of timber *in esse* but only of timber *in posse;* nevertheless, while acknowledging the validity of the objection, I submit that in view of the fact that the malady to be described effects such important damage to the young plants of several of our timber-trees, and that it is a type of a somewhat large class of diseases, the slight impropriety in the wording of the general title may be overlooked.

It has long been known to forest nursery-men that, when the seedling beeches first appear above the ground, large numbers of them die off in a peculiar manner—they are frequently said to "damp off" or to "rot off." A large class of diseases of this kind is only too familiar, in its effects, to cultivators in all parts of the world. Every gardener, probably, knows how crowded seedlings suffer, especially if kept a trifle too damp or too shaded, and I have a distinct recollection of the havoc caused by the "damping off" of young and valuable Cinchona seedlings in Ceylon.

In the vast majority of the cases examined, the "damping off" of seedlings is due to the ravages of fungi belonging to several genera of the same family as the one (*Phytophthora infestans*) which causes the dreaded potato disease—*i.e.* to the family of the Peronosporeæ—and since the particular species (*Phytophthora omnivora*) which causes the wholesale destruction of the seedlings of the beech is widely distributed, and brings disaster to many other plants; and since, moreover, it has been thoroughly examined by various observers, including De Bary, Hartig, Cohn, and others, I propose to describe it as a type of the similar forms scattered all over the world.

It should be premised that, when speaking of this disease, it is not intended to include those cases of literal damping off caused by stagnant water in ill-drained seed-beds, or those cases where insufficient light causes the long-drawn, pale seedlings to perish from want of those nutrient substances which it can only obtain, after a certain stage of germination, by means of the normal activity of its own green cotyledons or leaves, properly exposed to light, air, &c. At the same time, it is not to be forgotten that, *as conditions which favour the spread of the disease to be described*, the above factors and others of equal moment have to be taken into account; which is indeed merely part of a more general statement, viz. that, to understand the cause and progress of a disease, we must learn all we can about the conditions to which the organisms are exposed, as well as the structure, &c., of the organisms themselves.

First, a few words as to the general symptoms of the disease in question. In the seed-beds, it is often first noticeable in that patches of seedlings here and there begin to fall over, as if they had been bitten or cut where the young stem and root join, at the surface of the ground: on pulling up one of the injured seedlings, the "collar," or region common to stem and root, will be found to be blackened, and either rotten or shrivelled, according to the dampness or dryness of the surface of the soil. Sometimes the whole of the young root will be rotting off before the first true leaves have emerged from between the cotyledons; in other cases, the "collar" only is rotten, or shrivelled, and the weight of the parts above ground causes them to fall prostrate on the surface of the soil; in yet others, the lower parts of the stem of the older seedling may be blackened, and dark flecks appear on the cotyledons and young leaves, which may also be turning brown and shrivelling up (Fig. 36).

If the weather is moist—*e.g.* during a rainy May or June—the disease may be observed spreading rapidly from a given centre or centres, in ever-widening circles. It has also been noticed that if a moving body passes across a diseased patch into the neighbouring healthy seedlings, the disease in a few hours is observed spreading in its track. It has also been found that if seeds are again sown in the following season in a seed-bed which had previously contained many of the above diseased seedlings, the new seedlings will inevitably be killed by this "damping off." As we shall see shortly, this is because the resting spores of the fungus remain dormant in the soil after the death of the seedlings.

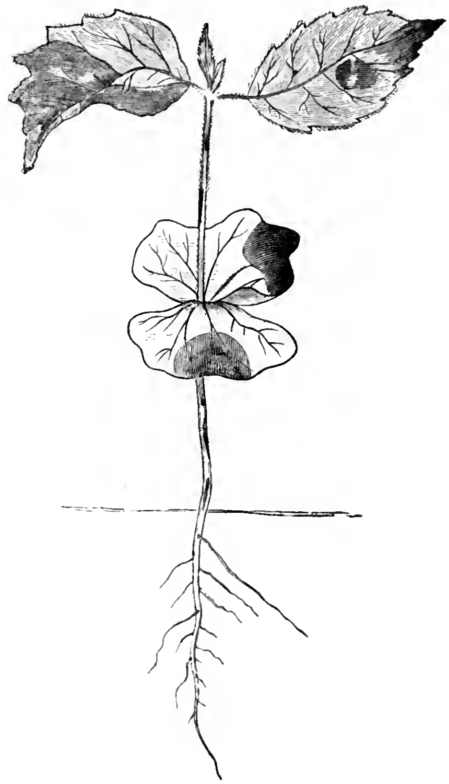


FIG. 35.—A young beech-seedling attacked by *Phytophthora omnivora*: the moribund tissues in the brown and black patches on the young stem, cotyledons, and leaves, are a prey to the fungus, the mycelium of which is spreading from the different centres. The horizontal line denotes the surface of the soil.

In other words, the disease is infectious, and spreads centrifugally from one diseased seedling to another, or from one crop to another: if the weather is moist and warm—"muggy," as it is often termed—such as often occurs in the cloudy days of a wet May or June, the spread of the disease may be so rapid that every plant in the bed is infected in the course of two or three days, and the whole sowing reduced to a putrid mass; in drier seasons and soils, the spread of the infection may be slower, and only a patch here and there die off, the diseased parts shrivelling up rather than rotting.

If a diseased beech seedling is lifted, and thin sections of the injured spots placed under the microscope, it will be found that numerous slender colourless fungus-filaments are running between the cells of the tissues, branching and twisting in all directions. Each of these fungus-fila-

[1] Continued from p. 299.

ments is termed a hypha, and it consists of a sort of fine cylindrical pipe with very thin membranous walls, and filled with watery protoplasm. These hyphæ possess the power of boring their way in and between the cell-walls of the young beech seedling, and of absorbing from the latter certain of the contents of the cells. This is accomplished by the hyphæ putting forth a number of minute organs like suckers into the cells of the seedling, and these suckers take up substances from the latter: this exhaustion process leads to the death of the cells, and it is easy to see how the destruction of the seedling results when thousands of these hyphæ are at work.

At the outer parts of the diseased spots on the cotyledons or leaves of the seedling, the above-named hyphæ are seen to pass to the epidermis, and make their way to the exterior: this they do either by passing out through the openings of the stomata, or by simply boring through the cell-walls (Fig. 37). This process of boring through the cell-walls is due to the action of a solvent substance excreted by the growing tip of the hypha: the

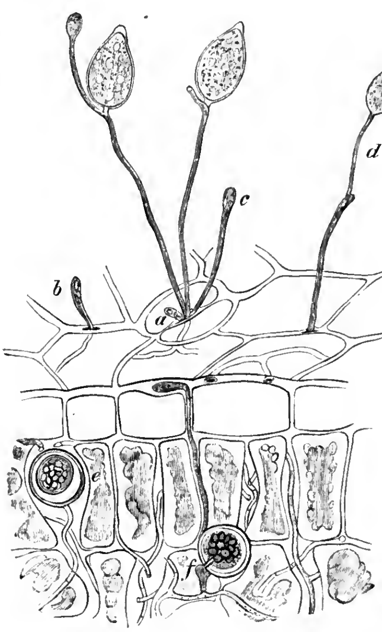


FIG. 37.—Portion of a cotyledon of the beech, infested with *Phytophthora omnivora*: the piece is shown partly in vertical section. The mycelium, spreading between the cells, puts forth aërial hyphæ, which bore between the cells of the epidermis, *b* and *d*, or emerge from the stomata, *a*, and form conidia at their apices: the various stages of development are shown. On other hyphæ, between the cells of the interior, the oospores are formed in oogonia, *e* and *f*. (Highly magnified.)

protoplasm secretes a ferment, which passes out, and enables the tip to corrode or dissolve away the substance of the cell-walls. It is also characteristic of these hyphæ that they make their way in the substance of the cell-walls, in what is known as the "middle lamella": in this, and in what follows, they present many points of resemblance to the potato-disease fungus, which is closely allied to *Phytophthora omnivora*.

The hyphæ which project from the epidermis into the damp air proceed to develop certain spores, known as the *conidia*, which are capable of at once germinating and spreading the disease. These conidia are essentially nothing but the swollen ends of branches of these free hyphæ: the ends swell up and large quantities of protoplasm pass into them, and when they have attained a certain size, the pear-shaped bodies fall off, or are blown or knocked off.

Now the points to be emphasized here are, not so much the details of the spore-formation, as the facts that (1) many thousands of these spores may be formed in the course of a day or two in warm, damp weather; and (2) any spore which is carried by wind, rain, or a passing object to a healthy seedling may infect it (in the way to be described) within a few hours, because the spore is capable of beginning to germinate at once in a drop of rain or dew. A little reflection will show that this explains how it is that the disease is spread in patches from centres, and also why the spread is so rapid in close, damp weather.

When a conidium germinates in a drop of dew for instance, the normal process is as follows. The protoplasm in the interior of the pear-shaped conidium becomes divided up into about twenty or thirty little rounded naked masses, each of which is capable of very rapid swimming movements; then the apex of the conidium bursts, and lets these minute motile zoospores, as they are called, escape (Fig. 38, *a*).

Each zoospore then swims about for from half an hour to several hours in the film of water on the surface of the epidermis, and at length comes to rest somewhere. Let us suppose this to be on a cotyledon, or on the stem or root. In a short time, perhaps half an hour, the little

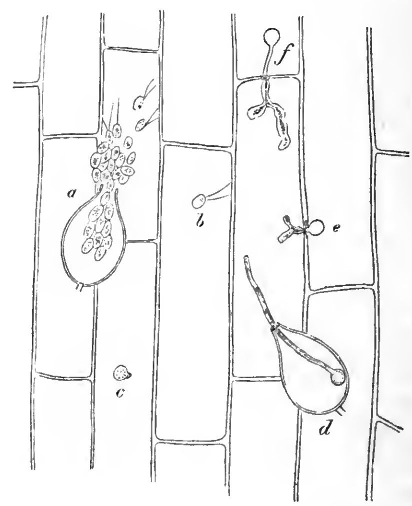


FIG. 38.—Portion of epidermis of a beech-seedling, on which the conidia of the *Phytophthora* have fallen and burst, *a* and *d*, emitting the motile zoospores, *b*, which soon come to rest and germinate, *c*, by putting forth a minute germinal hypha, *c*, *e*, which penetrates between the cells of the epidermis, *e* and *f*, and forms the mycelium in the tissues beneath. At *d* a zoospore has germinated, without escaping from the conidium. (Highly magnified: partly after De Bary and Hartig.)

zoospore begins to grow out at one point—or even at more than one—and the protuberance which grows out simply bores its way directly through the cell-wall of the seedling, and forms a cylindrical hypha inside (Fig. 38, *b*, *c*, *e*, *f*): this hypha then branches, and soon proceeds to destroy the cells and tissues of this seedling. The whole process of germination, and the entrance of the fungus into the tissues, up to the time when it in its turn puts out spore-bearing hyphæ again, only occupies about four days during the moist warm weather in May, June, and early in July.

We are now in a position to make a few remarks which will enable practical people to draw helpful conclusions from what has been stated. Let us suppose a seed-bed several feet long and about three feet wide, and containing some thousands of young beech seedlings: then suppose—by any means whatever—that a single conidium of *Phytophthora omnivora* is carried on to a cotyledon of one of the seedlings. Let us further assume that this occurs one warm evening in May or June. During the night, as the air cools, the cotyledon will be covered with a film or drops of water, and the conidium will germinate, and allow, say, thirty zoospores to escape. Now, the average size of a conidium is about 1/400 of an inch long by about 1/700 of an inch broad, and we may take the zoospore as about 1/2000 of an inch in diameter; thus it is easy to see that the film of moisture on the cotyledon is to a zoospore like a large pond or lake to a minnow, and the tiny zoospores, after flitting about in all directions, come to rest at so many distant points on the cotyledon—or some of them may have travelled abroad along the moist stem, or along a contiguous leaf, &c. Before daylight, each of these thirty zoospores may have put forth a filament which bores between the cells of the cotyledon, and begins to grow and branch in the tissues, destroying those cell-contents which it does not directly absorb, and so producing the discoloured disease-patches referred to. Supposing the weather to remain damp and warm, some of the hyphæ may begin to emerge again from the diseased and dying seedling on the fourth day after infection—or at any rate within the week—and this may go on hour after hour and day after day for several weeks, each hypha producing two or more conidia within a few hours of its emergence; hence hundreds of thousands of conidia may be formed in the course of a few days, and if we reflect how light the conidia are, and how their zoospores can flit about to considerable distances, it is not surprising that many of them are shed on to the surrounding seedlings, to repeat the story. If we further bear in mind that not only every puff of wind, but every drop of rain, every beetle, or fly, or mouse, &c., which shakes the diseased seedling may either shake conidia on to the next nearest

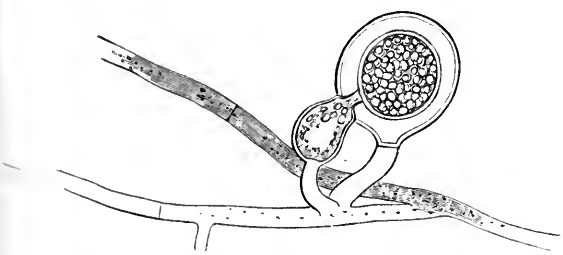


FIG. 39.—An oogonium and antheridium of *Phytophthora omnivora*. The oogonium is the larger rounded body, borne on a branch of the mycelium: it contains an oosphere, in process of being fertilized by the protoplasm of the antheridium (the smaller body applied to the side of the oogonium). The antheridium has pierced the wall of the oogonium, by means of a fertilizing tube, through which the contents pass into the oosphere, converting the latter into an oospore. (Very highly magnified: after De Bary.)

seedlings or even carry them further, it is clearly intelligible how the infection is brought about, and spreads through the seed-bed, gathering strength, as it were, hour by hour.

But, although we have explained the rapid infection from plant to plant, it still remains to see how it is that if we sow the seeds in this bed next year, the seedlings are almost certain to be generally and badly attacked with the disease at a very early stage.

When the fungus-mycelium in the cotyledons and other parts of the diseased seedlings has become fully developed, and has given off thousands of the conidia above described, many of the branches in the dying tissues commence to form another kind of spore altogether, and known as an oospore, or egg-like spore. This spore differs from the conidium in size, shape, and position, as well as in its mode of development and further behaviour, and if it were not that several observers have seen its formation on the same hyphæ as those which give rise to the conidia, it might be doubted by a beginner whether it really belongs to our fungus at all. As it is absolutely certain, however, that the oospore on germination gives rise to the fungus we are considering, the reader may rest satisfied on that point.

The spore in question is formed in a swelling of the free end of a branch of the hypha as follows. The protoplasm in the rounded end of the hypha becomes collected into a ball (the egg-cell or *oosphere*) and then a smaller branch with a distinct origin applies itself to the outside of this rounded swelling and pierces its wall by means of a narrow tube: protoplasm from the smaller branch (*antheridium*) is then poured through the tube into the "egg-cell," which thus becomes a fertilized "egg-spore" or *oospore*. This *oospore* then acquires a very hard coating, and possesses the remarkable peculiarity that it may be kept in a dormant state for months and even a year or more before it need germinate: for this reason it is often called a resting spore. It has been found that about 700,000 oospores may be formed in one cotyledon, and a handful of the infected soil sufficed to kill 8000 seedlings.

Now, when we know this, and reflect that thousands of these *oospores* are formed in the rotting seedlings and are washed into the soil of the seed-bed by the rain, it is intelligible why this seed-bed is infected. If seeds are sown there the next spring, the young seedlings are attacked as soon as they come up. These *oospores* are, in fact, produced in order that the fungus shall not die out as soon as it has exhausted the current year's supply of seedlings; whereas the *conidia*, which soon lose their power of germinating, are the means by which the parasite rapidly extends itself when the conditions are most favourable for its development and well-being.

It has already been mentioned that other plants besides the beech are destroyed by the ravages of this fungus. Not only has it been found to grow on herbaceous plants, such as *Sempervivum*, *Clarkia*, and many others, but it habitually attacks the seedlings of many timber-trees, such as, for instance, those of the spruce and silver firs, the Scotch pine, the Austrian and Weymouth pines, the larch, the maples, and particularly those of the beech.

It is obvious that this makes the question of combating this disease a difficult one, and the matter is by no means simplified when we learn that the fungus can live for a long time in the soil as a saprophyte, and apart from the seedlings. In view of all the facts, let us see, however, if anything can be devised of the nature of precautionary measures. It must at least be conceded that we gain a good deal by knowing so much as we do of the habits of this foe.

In the first place, it will occur to everybody never to use the same seed-bed twice; but it may be added that this precaution need not be taken as applying to anything but seeds and seedlings. Young plants, after the first or second year, are not attacked by the fungus—or rather are attacked in vain, if at all—and so the old beds may be employed for planting purposes. In the event of a patch of diseased seedlings being found in the seed-bed, as in our illustration quoted above, the procedure is as follows: cover the whole patch with soil as quietly and quickly as possible, for obviously this will be safer than lifting and shaking the spore-laden plantlets. If, however, the sharp eye of an intelligent gardener or forester detects one or two isolated seedlings showing the early stages of the disease, it is possible to remove the single specimens and burn them, care being taken that the fingers, &c., do not rub off spores on to other seedlings.

In the last event, the beds must be looked to every day to see that the disease is not spreading. All undue shading must be removed, and light and air allowed free play during part of the day at least; by such precautions, carefully practised in view of the above facts and their consequences, it is quite feasible to eradicate the disease in cases where ignorant or stupid mismanagement would result in the loss of valuable plants and time. In the case of other seedlings also, much may be done by intelligently applying our knowledge of the disease and its cause. It is not our purpose at present to deal with the diseases of garden-plants, &c., but it may be remarked in passing that in the large majority of cases the "damping off" of seedlings is due to the triumphant development of fungi belonging to the same genus as the one we have been considering, or else to the closely allied genus *Pythium*. In illustration of this I will mention one case only.

It is always possible to obtain well-grown specimens of the fungus *Pythium* by sowing cress seed fairly thick, and *keeping the soil well watered and sheltered*. Now what does this mean? Nobody imagines that the fungus arises spontaneously, or is produced in any miraculous manner; and in fact we need not speculate on the matter, for the fact is that by keeping the crowded cress seedlings moist and warm we favour the development of the *Pythium* (spores of which are always there) in somewhat greater proportion than we do the development of the cress. In other words, when the cress is growing normally and happily under proper conditions, it is not because the *Pythium* is absent, but because (under the particular conditions which favour the normal development of healthy cress) it grows and develops spores relatively so slowly that the young cress seedlings have time to grow up out of its reach. The recognition of this struggle for existence on the part of seedlings is of the utmost importance to all who are concerned with the raising of plants.

H. MARSHALL WARD.

NATURAL SELECTION AND ELIMINATION.[1]

MR. DARWIN'S phrase, "natural selection," is applied to such processes, other than those involving the agency of man, as result under Nature in the survival of the fittest. These processes fall under two heads, which have not, I think, been sufficiently distinguished. For the first of these I here retain the word *selection;* for the other I suggest the term *elimination*.

In natural selection the favourable varieties are chosen out for survival: in natural elimination the failures or comparative failures are weeded out. In the one, Nature is employing conscious agents upon the upper or superior end of the scale: in the other, Nature is, through conscious or unconscious agencies, at work on the lower or inferior end of the scale.

Variation is constantly taking place; and the variations may be favourable or unfavourable or neutral. Under selection the favourable variations will be chosen out; the unfavourable and the neutral may go. Under elimination the unfavourable disappear; the favourable *and the neutral* remain. By how much the favourable variations are in excess, by so much will the race tend to advance. I see no reason why neutral variations should be eliminated, except in so far as—in the keen struggle for existence—they become relatively unfavourable.

In the valuable and suggestive paper in which Mr. G. J. Romanes dealt with physiological isolation, he brought forward the inutility of specific characters as one of the three cardinal difficulties in the way of natural selection considered as a theory of the origin of species. So long as we consider selection proper, this objection is valid. But under elimination (by far the more potent of the two) there is no reason why specific features without utilitarian significance should be weeded out. Undoubtedly, in the long run, useful variations will tend more and more to preponderate, since, the longer and keener the struggle, the greater the tendency for neutral variations to become relatively unfavourable. And this conclusion is in harmony with the teachings of biology. For, as Mr. Romanes remarks, "it is not until we advance to the more important distinctions between genera, families, and orders that we begin to find, on any large or general scale, unmistakable evidence of utilitarian meaning."

Natural elimination is intimately associated with the struggle for existence, which may indeed be regarded as the reaction of the organic world called forth by the action of natural elimination. The struggle for existence is the result of a threefold process of elimination (cf. "Origin of Species," chap. iii.). First, elimination by the direct action of surrounding conditions; secondly, elimination by enemies (including parasites); and, thirdly, elimination by competition.

Natural selection (in the narrower sense suggested) is a much rarer process, and one that only comes into play when intelligence, or (since it may be objected that selection is in some cases instinctive) when the mind-element comes definitely upon the scene of life. Perhaps one of the best examples is the selection of flowers and fruits by insects and fruit-eating animals. But even here (at least in the case of flowers) the process of elimination also comes into play: for the visitation of flowers by insects involves cross-fertilization, the advantages of which Mr. Darwin so exquisitely proved. So that we have here the double process at work, the fairest flowers being selected by insects, and those plants which failed to produce such flowers being eliminated as the relatively unfit.

If we turn to the phenomena of what Mr. Darwin termed "sexual selection," we find both selection and elimination brought into play. By the law of battle the weaker and less courageous males are eliminated, so far as the continuation of their kind is concerned. By the individual choice of the females, the finer, bolder, handsomer, and more tuneful wooers are selected.

When we have to consider the evolution of human folk, the principle of elimination is profoundly modified by the principle of selection. Not only are the weaker eliminated by the inexorable pressure of competition, but we select the more fortunate individuals and heap upon them our favours. This enables us also to soften the rigour of the blinder law; to let the full stress of competitive elimination fall upon the worthless, the idle, the profligate, and the vicious; but to lighten its incidence on the deserving but unfortunate.

It is my belief that our views of evolution gain in clearness by the separation of these two processes by which the survival of the fit is brought about. Whether the use of the term "natural elimination" alongside of and in subservience to "natural selection" would be of service to those who are students and teachers of evolution doctrines, I must leave others to judge.

C. LLOYD MORGAN.

[1] Abstract of a Paper read before the Bristol Naturalists' Society.

THE FAUNA AND FLORA OF THE LESSER ANTILLES.

ALTHOUGH much has been done of late years, both in the United States and in Europe, towards the investigation of the fauna and flora of the smaller West Indian Islands, or Lesser Antilles, as it is better to call them, much remains to be effected before we can be deemed to have an accurate knowledge of the natural products of these islands. And it is most important that steps should be taken to remedy this deficiency without further delay. As the tide of civilization advances—more slowly, perhaps, it is true, over these islands than in many other parts of the world's surface—the special peculiarities which each individual island possesses among its animal and vegetable indigens are fast becoming overwhelmed by the more powerful animals and plants that accompany the inroads of civilized man upon the wilderness of Nature. As in other places, where settlers from Europe arrive, rats and mice eat out the indigenous animals, and exotic weeds starve out the native plants. It is therefore most desirable that, while there is yet time, exact information should be obtained of the flora and fauna of these islands, every one of which seems to exhibit features more or less peculiar to itself.

This subject having been brought before the Committee of Section D at the Manchester meeting of the British Association by Mr. Sclater, a grant of £100 was made for the purpose of initiating investigations in this direction. At the instance of the same gentleman, a similar sum was recently obtained out of the Government grant administered by the Royal Society, shortly after which the separate Committees appointed to administer the two grants agreed to combine for the purpose "of reporting on the present state of our knowledge of the zoology and botany of the West India Islands, and of taking steps to investigate ascertained deficiencies in the fauna and flora."

The joint Committee thus formed consists of Prof. Flower, Mr. Carruthers, Mr. Thiselton Dyer, Dr. Günther, Prof. Newton, Mr. Sclater, Dr. Sharpe, Lieut.-Col. Feilden, and Mr. D. Morris. Prof. Flower has been elected Chairman of the Committee; Mr. Thiselton Dyer, Secretary; and Mr. Sclater, Treasurer.

Lieut.-Col. Feilden having accepted a colonial appointment in Barbados will be in future resident at Bridge-Town, where he will act as local Secretary of the Committee, while Dr. H. A. Alford Nicholls, F.L.S., C.M.Z.S., has kindly agreed to assist in the same capacity in Dominica. In order to commence their investigations without delay, the Committee have secured the services of Mr. George A. Ramage, who was lately associated with Mr. Ridley in his expedition to the island of Fernando Noronha, and has since been collecting in Pernambuco. Mr. Ramage arrived in Dominica in March last, and has proceeded to his work with great zeal. In May, after passing five weeks at Laudat, on the right bank of the Roseau River, about 2000 feet above the sea-level, he moved to St. Aroment, an estate belonging to Dr. Nicholls, just above Roseau, which he found to be a better locality for getting his plants dried. At Laudat he met with great difficulty in this matter on account of the extreme wetness of the climate. Writing in May last, Mr. Ramage speaks of having got, besides his plants, "a good lot of insects, lizards, small snakes, and land-molluscs." Besides these, he had also obtained three specimens of *Peripatus*. This is a valuable discovery, as this singular organism was originally discovered in Dominica by Guilding many years ago, and has not been since obtained in the same locality.

After exploring Dominica, Mr. Ramage will probably receive instructions to proceed to the other islands of the Leeward group, some of which are almost entirely unworked as regards their animal and vegetable life. Now that this important investigation has been so fairly started, it is hoped that little difficulty will be experienced in obtaining further assistance from the British Association and the Royal Society. It should, perhaps, be mentioned that complete sets of all the specimens obtained will be placed in the British Museum and Kew Herbarium, the Directors of these two Institutions being themselves both members of the Committee.

SONNET*

TO A YOUNG LADY WITH A CONTRALTO VOICE,

On her singing, on a warm summer's afternoon, without accompaniment, save the music of the birds heard through the open windows of the author's rooms overlooking the beautiful garden of New College, Oxford, the old English ditty,
"Deck not with gems that lovely form for me,"
in which occurs the line,
"I must have loved thee hadst thou not been fair."

THE startled, ambushed, nightingales despair
To match those notes, so tender sweet and low,
That poured through lips where Cupid lays his bow
Had made thee loved e'en hadst thou been less fair.
What need hast thou with gems to deck thy hair,
Of aught of wealth Golconda's mines bestow,
Rubies or pearls rash divers seek below!—
Thou canst in nobler wise thy worth declare.
Oft shall thy votary in his cloistered cell
In deep research of Nature's secret clue
Pause, to bid Memory with her magic spell,
Bring back thy face and sweet girl-form to view,
And in fond fancy hear thy voice anew
Till life to gladness breathes its last farewell.

Athenæum Club, July 25. J. J. S.

* This is the original form of the sonnet, published in the preceding number of NATURE, which, if perhaps superior to this in expression, is open to the reproach from which the original is free, pointed out to the author by his distinguished friend, the great Traveller and Orientalist (the translator, too, of Camoens' sonnets), Sir Richard Burton, of deviating from the Petrarchian model by its sestett having one rhyme in common with the octave. In my "Laws of Verse" (if I remember right) I have compared the octave and sestett of a sonnet to the body and the frame or bed of a carriage respectively. The effect of a rhyme common to the two may be likened to that of driving in a spike, which converts the previous springy connection of the two parts into a fixture. The much more common fault of English sonnets is the reverse of this, viz. that they contain too many distinct rhymes instead of too few. In the form-build of the two sonnets I may be said to have discovered a locket artistically adapted to receive either one of two miniatures, each in its own way equally exquisite, and worthy of ineffable regard and adoration. I left the Subject of this week's sonnet at the door of Magdalen College Chapel to attend the evening service there, and early the next morning, as it now reads, with the exception of changes in three lines only, it was in the hands of her parents.

With regard to the punctuation of this and other of my poetical pieces, I share to a great extent the opinion of the late deeply regretted Matthew Arnold, that in poetical composition the fewer points the better: grammatical or (so to say) choristic points as such should never be introduced except when necessary to prevent ambiguity or obscurity of meaning: consequently there will be many points left out in poetry which would be found in the same piece written in prose. But *per contra* I hold that points are sometimes useful or even necessary in poetry which would not be found in prose, viz. to mark brief pauses or almost insensible musical rests. The pointing I have adopted in the line from last week's sonnet—

Thy flashing, rushing, fingers to indue—

affords an exemplification of this latter principle. The commas on each side of *rushing* are not choristic but melodic, and would not appear in prose.

In law writings no points at all are introduced, and for reasons which in no wise conflict with the principles referred to above.

1°. A law document is expected and ought to be written in such a form as to be insusceptible of an equivocal or doubtful construction.

2°. No one expects a law document (unless maybe it were a marriage certificate or deed of separation by mutual consent) to have much music in its lines.

One of the official readers of the sonnet contained in the last number of NATURE has written to me to say that he cannot see the sense of lines 3 and 4. The answer is, I think, obvious. In the human organism all parts, faculties, and powers are connected and correlated. Consequently a voice whose notes are pure, sweet, and true affords a voucher (I do not say mathematical proof, but presumptive evidence which may be accepted in the absence of rebutting facts) of the character to which it appertains being sweet, pure, and true. But sweetness, purity, and truth are the prime ingredients of goodness. Therefore notes which are pure, sweet, and true vouch for the goodness of the person to whom the voice belongs. Q.E.D.

The argument in the text is put in the form of an enthymeme, the major premise—*All persons whose singing notes are sweet, pure, and true offer a presumption that they are good*—being suppressed. It is notorious that birds instinctively, and therefore on the surest ground, infer the worthiness (or according to their ethical code the goodness) of their partners from their superiority in song. Witness the disticn from a sonnet familiar to many of my readers—

Like foolish bird who in the fowler's cry
Hears her loved mate's soft amorous melody

If I am wrong in supposing so, I hope that Mr. Romanes, or any other biologist (if such there be) of equal skill with him in Darwinian dialectics, will set me right in this point, and inform the readers of NATURE on what other intelligible ground can be explained the recourse had to song by the male bird to win the affections of his mate. If such be the case with birds, why should it not be equally true of the sometimes scarcely less volatile portion of the human race?

NOTES.

NEXT year there will be in Paris what promises to be a splendid Anthropological Exhibition under the auspices of the French Ministry of Public Instruction. It will be organized by Committees representing the Society, the School, and the Laboratory of Anthropology; and an appeal for aid has been addressed to all who are, or have at any time been, connected with one or other of these institutions. The Exhibition will include objects relating to all branches of anthropological science.

CAPTAIN JOHN ERICSSON, who retains much of his vigour and youthful activity, celebrated his eighty-fifth birthday at New York on Tuesday, July 31. The King of Sweden and Norway cabled orders to Consul-General Bors to call upon the eminent engineer, and convey to him on the occasion renewed assurances of His Majesty's esteem. "Consul-General Bors," says the *New York Daily News*, "was only too happy to execute this commission, and when he called at 36 Beach Street to-day to deliver the message he brought with him a beautiful bouquet that delighted the great engineer extremely when he received it. He very willingly granted Consul-General Bors an audience, and thanked him for the courteous message he brought from his Royal master. Captain Ericsson has a wonderful faculty of talking and working out the most exact mechanical drawings at the same time, and Mr. Bors's visit did not interrupt him in his work in the least. He chatted with him cheerily, and listened with an amused smile to the Consul's expressions of wonder at his marvellous health and mental vigour."

THE Congress for the study of tuberculosis, lately held at Paris, was very successful. Numerous and important papers were read, and there was always a large attendance of members. The next meeting will be held in 1890, under M. Villemin's presidency.

THE sixty-first meeting of German men of science and physicians will be held in Cologne from September 18 to 23 next.

MR. JAMES STEVENSON, late Executive Officer of the United States Geological Survey, died at Gilsey House, New York, on July 25. He was born, in 1840, at Maysville, Kentucky.

THE United States Senate has voted to pay the widow of the late Prof. Spencer F. Baird 50,000 dollars in recognition of his services as United States Fish Commissioner.

LAST year, Bedford College sustained a great loss by the death of Mr. Shaen, who had been one of its most active friends since its earliest days; and a wish was then widely expressed that some scheme should be devised which should permanently associate his name with Bedford College. The Council now propose that a building shall be erected on a site immediately behind the College, and that it shall be called the Shaen Wing. In this building there would be good laboratories and class-rooms, and it is believed that the premises could be so arranged as to provide accommodation, at a moderate charge, for a number of students. It would be hard to think of a more suitable memorial of Mr. Shaen, and we have no doubt that the entire amount necessary for the carrying out of the plan (£3000) will soon be subscribed. The proposal that a large part of the fund shall be devoted to science laboratories strikes us as an interesting and hopeful sign of the times. Bedford College has done much to help the movement for supplying women with better opportunities of study. Of the 452 women who have passed the various examinations of the London University, no fewer than 123 have been students of this institution; and about one-third of the present students are working for these examinations. It may be reasonably expected that when the new laboratories are opened the results will be even more satisfactory than those now achieved; for all the present laboratories are adaptations of former class-rooms, and, being deficient in light and space, are but imperfectly fitted for the purposes for which they are used.

IN his Report on the technological examinations of 1888 Sir Philip Magnus says that in the present year there has again been a large increase in the total number of candidates examined. In 1887, 5508 were examined, of whom 3090 passed; in 1888, 6166 were examined, of whom 3510 passed. The increase in the number of candidates is less this year than last year, being 658 as compared with 744. Examinations have been held this year in forty-nine different subjects, in seven of which less than ten candidates presented themselves. The subjects in which the least number of candidates presented themselves are those connected with the chemical industries, and the examiners in these subjects generally remark that few of the candidates are found to possess that combined knowledge of scientific principles and of technical processes which is desirable. The increase in the number of candidates has been most marked in cloth, cotton, linen, and jute manufacture, in plumbers' work, carriage-building, carpentry and joinery, and in brickwork and masonry. The average percentage of failures has fallen from 43·8 to 43·1; and from the separate reports of the examiners it appears that in most subjects there is a distinct improvement in the quality of the candidates' written answers and practical work. Of the 3510 successful candidates, 758, or 21·6 per cent., have passed in the honours grade, as against 21·9 per cent. last year. It appears that 10,404 students have received instruction in 475 registered classes connected with the City and Guilds of London Institute. These classes were in 183 different towns in the United Kingdom. The corresponding numbers for the previous year were 8613 students, 365 classes, and 121 towns. These numbers do not include the students at the Finsbury Technical College, the Yorkshire College, Leeds, and other Colleges the Professors of which do not receive grants on results, and the candidates from which are classed as "external" candidates. Sir Philip Magnus anticipates that with the establishment of new Polytechnic Institutions in different parts of London there will be a large increase in the number of students in the technical classes registered by the Institute and in the number of candidates for examination.

IN the Report, for the year 1886–87, presented by the Board of Managers of the Observatory of Yale University to the President and Fellows, complaint is made that too large a proportion of the clinical thermometers (foreign or American) sent to the Observatory for verification are despatched so soon after their manufacture that the corrections given are liable to change with a year's use. "Physicians," says Mr. Robert Brown, secretary of the Observatory, "would obtain much more exact indications of temperature if, estimating the probable annual breakage, they would provide themselves with two or three years' supply of well-made, well-graduated clinicals, and obtain tables of corrections only after the instruments were *known* to have attained a proper age of, say, one or two years. The comparatively small demand for clinicals whose *age* as well as correction is certified, seems to imply that the medical profession is not yet generally awake to the exactitude that is practicable in ascertaining body temperature."

THE sealer *Jason* has arrived in Norway from the Greenland coast, and reports that the Expedition under Dr. Fridtjof Nansen, which is to cross Greenland from east to west, left that ship on July 17 in latitude 65° 2′ N. An ice-belt about ten English miles in width separated the ship from the shore, but it is believed that the members would have no trouble in crossing this, the floes being large. Dr. Nansen intended to land in the Sermilik Fjord, which is inhabited. Previous attempts at landing had failed on account of rain and fog.

IT is said that the Cincinnati Exposition is the best that has been held in America since the great one at Philadelphia in 1876. We reprint from *Science* the following account of it:—"People who were at New Orleans in 1885 say that this is enormously superior in all the arts, especially upon the mechanical and industrial side. The Exposition covers 15 acres in the very heart of the city, and in every part of this large area one meets evidences of taste, skill, ingenuity, and perseverance in adapting means to ends, which form a series of apparently never-ending surprises as one passes from one exhibit to another. The Government exhibits are all good and all characteristic. The Smithsonian Institution and the Geological Survey exhibits attract crowds. In the latter, Prof. F. W. Clark has some transparent photographic views, represented in colours by some new and as yet undisclosed process. The effect is wonderfully natural and beautiful, and if it is found to be durable it will prove a great discovery. The very fine models of the new classes of naval vessels now building attract crowds daily, as do the various forms of weapons for wholesale slaughter, in case we ever have another war. In close juxtaposition are the ingenious devices, for saving life in cases of shipwreck, of the Life-saving Service. The Fish Commission exhibit is not as yet complete. In such elaborate displays, requiring much preparatory work, more time should have been allowed for preparation. The Post Office Department and the Army exhibits are also incomplete, but a few days will find everything in order."

THE native birds of North America, which were supposed to be rapidly disappearing, reappeared in great numbers during the spring of the present year. This was first noted in the New York papers, and was promptly credited to the liberal destruction of the pugnacious English sparrow, unable to withstand the storm-beating received in the great March blizzard. But counter to this explanation, says *Science*, comes information from Illinois that the attention of all is attracted to the remarkably large number of birds that are to be seen. The groves, the woods, and the meadows in the country, and the many trees in the city, are peopled with these feathered visitors. The oldest inhabitant does not remember to have seen so many and such a variety of birds. And yet the great blizzard did not visit Illinois.

THE vapour-density of hydrofluoric acid has been subjected to a rigorous re-examination at the hands of Prof. Thorpe and Mr. F. J. Hambly. Prof. Mallet some time ago showed that, at a temperature of 30°·5 C., the density of hydrofluoric acid vapour corresponded to a molecule of the composition H_2F_2; consequently we have been accustomed to think of this substance as consisting of ordinary molecules of HF at higher temperatures, and of condensed molecules of H_2F_2 just above its boiling-point. But we have recently seen, from the experiments conducted in Prof. Victor Meyer's laboratory upon the molecular nature of sulphur, and also from the previous investigations concerning the composition of the molecules of the chlorides of aluminium, tin, and iron, that our older ideas as to the formation of condensed molecules, such as S_6 or Fe_2Cl_6, at particular temperatures were erroneous; that these condensed molecules were not capable of existence throughout any notable range of temperature. It therefore became an interesting question whether hydrofluoric acid would not behave in a similar manner. To test the question thoroughly, fourteen vapour-density determinations, at temperatures ranging from 26°·4 to 88°·3 C., have been carried out in the research laboratory of the Normal School of Science by means of an elaborate and expensive platinum apparatus. The first necessity was, of course, the pure anhydrous acid. This was freshly prepared, as required for each experiment, from the now famous double fluoride of hydrogen and potassium; it was afterwards re-distilled from the platinum retort through the density apparatus, which was placed in a bath of glycerol heated to the required temperature. The vessel, of known capacity, in which the hydrofluoric acid was eventually weighed consisted of a platinum cylinder completely closed, with the exception of the entrance and exit tubes, which could be closed at will by means of well-fitting platinum stopcocks of skilful workmanship. The whole operations could thus be conducted in platinum throughout, and are, therefore, of the most trustworthy character. As expected, the values obtained correspond to molecular weights ranging from 51·19 at 26°·4 to 20·58 at 88°·3, thus showing a continuous breaking down of the molecular grouping, until, finally, we arrive at the stage where the whole of the molecules consist simply of HF, corresponding to the normal density of 20. No other molecular condition than this is capable of existing throughout any considerable range of temperature. It is of the highest interest to consider what happens below 26°·4. The natural inference is that the molecular grouping becomes more and more complex, or condensed, until we reach a point where the substance becomes visible—a liquid; while still further condensation eventually brings us face to face with a solid.

THE Report of the Conservator of Forests in Ceylon for the past year says that though Sir Joseph Hooker in 1873 called attention to the rapid destruction of forests in that island, no steps were taken by the Government till 1882. In that year, as a result of a report of Mr. Vincent, of the Indian Forest Department, the Government turned its attention to the subject, and in 1885 the "Forest Ordinance" was issued. The objects of this Ordinance were, briefly, to select suitable areas of forest land and constitute them State forests, to buy off any interests which private individuals might have in those lands, to place them under effective protection, and generally to systematize the forest conservancy. Even now the Crown forests are plundered in a wholesale fashion by organized bands of thieves, but it is hoped in a short time to put an end to this, and make the forests of Ceylon as remunerative, comparatively speaking, as those of India, where they produce a substantial revenue. Ruin has threatened the Ceylon forests, just as it threatened the forests of Jinjira, in Western India, where three-fourths of a vast forest forty miles long, and from fifteen to a hundred miles in breadth, was destroyed, and the remainder with difficulty saved.

IN an interesting paper on ancient tide-lore, reprinted, with some other papers by the same author, from the Transactions of the New Zealand Institute, Mr. W. Colenso, F.R.S., describes the old belief of the Maories as to the ebbing and flowing of the sea. These phenomena, it seems, they attributed to a huge ocean monster, whose home was low down in the depths beyond the horizon. It was supposed to do its work by powerful and regular respiration, or ingurgitation and regurgitation of the water. The monster's name was Parata; and any one overtaken by great misfortune is said to have fallen into Parata's throat. In a myth relating to the first peopling of New Zealand, one of the chief canoes, named the *Arawa*, is represented as being carried into the enormous mouth of the monster, and as being with difficulty extricated by Ngatoroirangi, the courageous and cunning *tohunga* (= priest, or wise man) on board, who recited his powerful charm for the purpose. The words of this charm or spell are still preserved.

IN his Report to the Foreign Office on the agriculture of Yezo, the British Consul at Hakodadi says that though the Ainos are a hunting and fishing people, yet efforts have been made to induce them to cultivate the soil. In 1869 the influx of Japanese to the fishing grounds reduced them to great straits. This appears to have continued till 1882, when attention was drawn to their condition, and sums of money were distributed amongst them to relieve their distress, schools were built, and attempts were made to teach them farming. In 1886 the money gifts were stopped, but the efforts to teach them agriculture continued, and at the end of that year about 803 acres were cultivated by the Ainos. The Consul remarks that it is impossible to tell how many Ainos there are in Japan, from their carelessness or dislike to record the births and deaths; but it is calculated that there are about 3600 houses in Yezo—that is, about 14,000 people. The general impression is that they are gradually disappearing, but obviously the Government of Japan is doing all it can to aid the Ainos, and to foster in them a spirit of self-help.

THE last number (Session 1887-88) of the *Madras Journal of Literature and Science* contains the first part of a treatise by Prof. Oppert, of the Presidency College, Madras, on the original inhabitants of Bharatavarsa, or India, whom he describes as

Ganda-Dravidians. This term the learned writer explains by saying that the two special Ganda-Dravidian terms for mountain are *mala* and *ko*, both being widely used and prevalent throughout India. Those tribes, whose names are derived from *mala*, he calls Dravidians, and those whose names are derived from *ko*, Gandians. In this way the Mallas, Mālas, Malavas, Malayas, &c., and the Kōyis, Kōdulu, Kondas, Gondos, Kuruvas, &c., are classified as Dravidians and Gandians respectively. The presence of the Ganda-Dravidians in India can be proved at a very early period "from the north-west across to the north-east, and from both corners to the furthest south. On the arrival of the Aryans on the north-western frontier, the Ganda-Dravidians are already found in flourishing communities." In the present instalment of his work, Prof. Oppert endeavours to prove the antiquity of this race, especially of the Dravidian branch; in the next he will treat of the Gandians. His own summary of his positions in the concluding section is to this effect: in following the ramifications of the Dravidians throughout the peninsula, he points out the connection which exists between several tribes, apparently widely different from each other; he has identified the so-called pariahs of Southern India with the old Dravidian mountaineers, and establishes their relationship with a number of tribes forming, as it were, the first layer of the ancient Dravidian stratum, and he endeavours to show that various other different tribes are offshoots of the Dravidian race. He thinks also that much that is now considered Aryan in name and origin must be regarded as originally Dravidian. The various Dravidian tribes scattered over India are separately introduced into the discussion in order to establish their mutual kinship. Prof. Oppert, in fact, labours to restore the Dravidian "to those rights and honours of which he has so long been deprived." The spirit in which he has undertaken what is obviously a great work is sufficiently evident from the words with which he concludes the present article:—"My errors, too, may not be without use if, like stranded vessels, they serve to direct the explorer, warning him away from those shoals and rocks that beset the inquirer in his search after truth."

IN the Berlin *Meteorologische Zeitschrift* for July, Dr. E. Brückner discusses the meteorological observations of the German Polar station at Kingua Fjord (Cumberland Sound), and also of the stations in Labrador and South Georgia, in the year 1882-83. The results represent three distinct types of winter climate. Kingua Fjord has a calm, severe winter, and cool summer, the warmest month being August, whereas July is usually the warmest month in Arctic North America. In Labrador the cold is often accompanied by stormy west winds, and although the temperature is higher than at Kingua Fjord, the cold is much more keenly felt. South Georgia naturally partakes of the oceanic character, but the yearly temperature, 34°·5, is much lower than at the neighbouring stations on the coast of South America, in the same latitude (54° 31′ S.), and is the lowest yet known in the southern hemisphere. Dr. P. Schreiber contributes an instructive article on the question of the deduction of true daily means of temperature from three or four observations daily. He gives a series of nine combinations, and their results, as compared with twenty-four hourly observations at Chemnitz. The result shows that the somewhat inconvenient hours of 6 a.m., 2 and 10 p.m., give the nearest true daily temperature. The inquiry is interesting as bearing upon the question of the necessity of continuing the expensive process of continuous records for an unlimited period.

DR. BUYS BALLOT, the Director of the Meteorological Institute of the Netherlands, has published an excerpt paper from the Proceedings of the Amsterdam Academy of Sciences, on the distribution of temperature over the surface of the earth. Instead of representing the temperatures by the usual method of isothermal lines, he gives the departures for each 5° of latitude and longitude from the normal temperature at the equator, by means of figures, using ordinary and thick type to avoid the use of plus and minus signs. The variations of temperature for the typical months of January and July, and for the year, are very clearly shown by this method. The work is also accompanied by maps, connecting by curves all places having the same mean differences of temperature in the summer and winter months.

AMONG the contents of the new number of the *Internationales Archiv für Ethnographie* (Band i. Heft 4) we may note: a Singapore street scene, by Prof. G. Schlegel; a paper, by F. Grabowsky, on certain sacrificial customs in Borneo; another, by J. D. E. Schweltz, on South Sea relics; and various ethnographic notes from Mecca, by G. Snouck Hurgronje. All the articles are admirably illustrated with coloured plates.

A NEW autumn edition of "Walks in Epping Forest," by Percy Lindley, describing portions less known to pedestrians, is in preparation. Prof. Boulger has contributed to the same issue some notes upon the recent extensive tree-felling and "forestry" operations in Epping Forest.

A book on "The General Principles of Agriculture," by A. Larbalétrier (Reinwald), has just been published in Paris.

GEGENBAUER'S "Human Anatomy" is being translated into French. The first quarter of the book was recently published by Reinwald.

THE *Odessa Gazette* reports the discovery of the remains of an ancient town on the right bank of the Volga. These remains are traceable over an area about two miles long, by three-quarters of a mile in width. The place has been visited by a deputation from the Commission of Archives. A very considerable quantity of Arabian, Persian, and Tartar coins has been found there, besides a multitude of other objects which bear witness to the cultivated state of the inhabitants. There were remains of marble blocks, of watercourses, &c.

AN exploring party of eight persons, led by Lieut. Israel, have set out from South Australia to explore the country north-east of Newcastle in Western Australia, and particularly the territory around Lake Moore and Lake Manga. The objects of the expedition are said to be partly scientific and partly commercial, and the funds have been supplied by a number of Australian capitalists.

A CORRESPONDENT—who says that everyone who looks through the series of photographs of lightning in the possession of the Royal Meteorological Society must be struck with the fact that many of the flashes exhibit a ribbon-like structure, while the appearance is totally absent from others—has made some experiments in order to ascertain whether a similar appearance can be produced by interposing a sheet of window-glass between a narrow brightly-illuminated slit and the camera. So far as these experiments have yet gone, he is not in a position to assert that all the peculiar band-like appearances can thus be imitated, but there is no doubt, he asserts, that a photograph of an unribboned flash taken obliquely through a window must exhibit appearances very similar, if not identical.

THE additions to the Zoological Society's Gardens during the past week include a Weeper Capuchin (*Cebus capucinus* ♂) from Brazil, presented by Mr. Haddan; two Common Genets (*Genetta vulgaris*) from West Africa, presented by Mr. Philip Lemberg; three Palm Squirrels (*Sciurus palmarum*) from India, presented by Surgeon-Major W. G. King; an Orange-winged Amazon (*Chrysotis amazonica*) from South America, presented by the Hon. N. L. Melville; two Fulmar Petrels (*Fulmarus glacialis*) from St. Kilda, presented by Mr. W. C. Gilles; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. Underwood; a Macaque Monkey (*Macacus cynomolgus* ♂) from India, an Ocelot (*Felis pardalis*), a Common Rhea (*Rhea americana*) from South America, a Ring-necked Parrakeet

(*Palæornis torquatus*) from India, a Grey-breasted Parrakeet (*Bolborhynchus monachus*) from Monte Video, two White-fronted Amazons (*Chrysotis leucocephalus*) from Cuba, two European Tree Frogs (*Hyla arborea*), European, deposited; a Barraband's Parrakeet (*Polytelis barrabandi*) from New South Wales, purchased; a Mountain Ka-Ka (*Nestor notabilis*) from New Zealand, received in exchange; two Canadian Beavers (*Castor canadensis*), three Gold Pheasants (*Thaumalea picta*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

FURTHER COMETARY DISCOVERIES.—Mr. W. R. Brooks, Smith Observatory, Geneva, New York, discovered a new comet, 1888 *c*, on August 7. The place for 8h. 46m., G.M.T., on August 7 is given as R.A. 10h. 5m., Decl. 44° 30′ N.. It was observed at Vienna on August 9, 9h. 53·5m., in R.A. 10h. 21m. 53s., Decl. 44° 49′ 26″. Faye's comet was picked up by M. Perrotin at the Nice Observatory on August 9, its place at 15h. 19·5m., Nice M.T., being R.A. 5h. 0m. 27·6s., Decl. 20° 0′ 42″ N. There are thus four comets now under observation. The following ephemeris, supplied in the *Dun Echt Circular*, No. 159, is derived from Dr. Krentz's ephemeris for Faye's comet in the *Astr. Nachr.*, No. 2849, the time of perihelion passage having been increased by 2·6 days.

Ephemeris for Berlin Noon.

1888	R.A. h. m.	Decl. ° ′	1888	R.A. h. m.	Decl. ° ′
Aug. 20 ...	5 28·5	19 31 N.	Sept. 5 ...	6 9·5	17 58 N.
24 ...	5 39·0	19 13	9 ...	6 19·2	17 27
28 ...	5 49·4	18 51	13 ...	6 28·6	16 54
Sept. 1 ...	5 59·6	18 26 N.	17 ...	6 37·8	16 18 N.

Dr. Backlund's ephemeris for Encke's comet, given in the last issue of NATURE (p. 350), should also have been given for Berlin noon, and not for midnight. The resulting error of the ephemeris at the time of discovery thus becomes O – C, R.A. + 8s.; Decl. – 1′·3.

The following ephemeris, by Dr. H. Kreutz, for Brooks's comet is for Berlin midnight:—

1888.	R.A. h. m. s.	Decl. ° ′	1888.	R.A. h. m. s.	Decl. ° ′
Aug. 15	11 8 8	44 25·7 N.	Aug. 23	12 5 53	42 14·0 N.
19	11 37 41	43 32·9	27	12 32 21	40 33·4

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *AUGUST* 19-25.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 19

Sun rises, 4h. 54m.; souths, 12h. 3m. 18·2s.; sets, 19h. 12m.: right asc. on meridian, 9h. 56·4m.; decl. 12° 34′ N. Sidereal Time at Sunset, 17h. 6m.

Moon (Full on August 21, 16h.) rises, 18h. 18m.; souths, 22h. 38m.; sets, 3h. 3m.*: right asc. on meridian, 20h. 32·6m.; decl. 19° 20′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	4 21 ...	11 47 ...	19 13 ...	9 40·3 ...	15 45 N.
Venus.....	5 51 ...	12 46 ...	19 41 ...	10 39·3 ...	10 3 N.
Mars......	12 30 ...	16 58 ...	21 26 ...	14 52·2 ...	17 57 S.
Jupiter....	13 26 ...	17 48 ...	22 10 ...	15 41·9 ...	18 58 S.
Saturn....	3 28 ...	11 7 ...	18 46 ...	9 0·0 ...	17 47 N.
Uranus...	9 24 ...	15 1 ...	20 38 ...	12 54·9 ...	5 12 S.
Neptune..	22 23*...	6 10 ..	13 57 ...	4 2·1 ...	18 59 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Aug.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
21 ...	γ Capricorni	... 3½ ...	0 58 ...	2 10 ...	126 314
21 ...	50 Aquarii	... 6 ...	20 17	near approach	162 —
22 ...	ψ³ Aquarii	... 5 ...	21 46 ...	22 30 ...	29 320
22 ...	ψ² Aquarii	... 4½ ...	21 55	near approach	172 —

Aug. h.
24 ... 1 ... Mercury in superior conjunction with the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
Algol	3 0·9 ...	40 31 N. ...	Aug. 23,	0 55	*m*
			,, 25,	21 44	*m*
λ Tauri...	3 54·5 ...	12 10 N. ...	,, 20,	0 57	*m*
			,, 23,	23 49	*m*
T Monocerotis ...	6 19·2 ...	7 9 N. ...	,, 25,	4 0	*M*
R Canis Minoris...	7 2·6 ...	10 12 N. ...	,, 21,		*M*
δ Libræ	14 55·0 ...	8 4 S. ...	,, 23,	22 34	*m*
U Coronæ	15 13·6 ...	32 3 N. ...	,, 22,	21 7	*m*
S Herculis	16 46·8 ...	15 8 N. ...	,, 23,		*M*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,, 19,	2 48	*m*
		and at intervals of		20 8	
W Sagittarii ...	17 57·9 ...	29 35 S. ...	Aug. 23,	20 0	*m*
U Sagittarii... ...	18 25·3 ...	19 12 S. ...	,, 23,	1 0	*m*
S Sagittarii	19 50·9 ...	16 20 N. ..	,, 19,	23 0	*M*
U Cygni	20 16·1 ...	47 33 N. ...	,, 20,		*m*
X Cygni	20 39·0 ...	35 11 N. ...	,, 22,	2 0	*M*
T Vulpeculæ ...	20 46·7 ...	27 50 N. ...	,, 19,	20 0	*M*
			,, 20,	21 0	*m*
R Vulpeculæ ...	20 59·4 ...	23 23 N. ...	,, 21,		*M*
δ Cephei	22 25·0 ...	57 51 N. ...	,, 25,	22 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near γ Camelopardalis...	54 ...	71 N. ...	Swift; streaks.
	290 ...	60 N. ...	Bright and slow; with trains.

GEOGRAPHICAL NOTES.

A WORK of great interest in the history of early European cartography has recently been published by Messrs. Stevens and Sons, of Great Russell Street, and the manner in which it came to be compiled is not a little curious. One of the most famous of the early European cartographers was Johann Schöner, Professor of Mathematics at Nuremberg in the early part of the sixteenth century. He is best known now by a series of terrestrial globes which he prepared, one about 1515, another in 1520, and a third in 1533, all three of which are still preserved at Frankfort, Nuremberg, and Weimar respectively. Here, so far as cartography is concerned, students would have believed Schöner's work to have ceased, were it not for a small Latin pamphlet of four pages which existed amongst his numerous writings, and which was, in substance, a letter to a high ecclesiastical authority of Bamberg descriptive of a globe on which were marked the discoveries made during Magellan's famous circumnavigation of the globe. Only three copies of this pamphlet were known to exist. It was dated 1523, and it obviously did not refer to the globes of 1515 or 1520, for these did not contain any references to the discoveries in question. Hence it was assumed that another globe, between 1520 and 1533 had been prepared by Schöner, but no trace of this could be found, and, if it existed at all, it seemed to be lost for ever. But in 1885 the late well-known bibliographer, Mr. Henry Stevens ("of Vermont") found in the catalogue of a Munich bookseller a facsimile of a globe which he at once recognized as the long-lost work of Schöner. He promptly purchased it, and ultimately it found its way into the remarkable collection of works on early American geography and history made by Mr. Kalbfleisch, of New York, where it still is. But Mr. Stevens, who regarded it as "one of the keys to unlock the many mysteries of early American geography," determined to reproduce Schöner's letter and globe in facsimile, and to append a translation and an introductory sketch of the early historical geography of America. While still labouring at this work he died, but his son took it up, and, aided by Mr. C. H. Coote, of the Map Department of the British Museum, has now succeeded in bringing it to a conclusion. Schöner himself was entirely indebted for his knowledge of the results of Magellan's voyage to a letter written by one Maximilianus Transylvanus, a natural son of the Cardinal Archbishop of Salzburg, and then employed about the Court of the Emperor Charles V., describing for his father the expedition in question. This pamphlet is styled "De Molvccis," and from the descriptions here given, Schöner depicted the new portions of his globe, or, in his own words, "being desirous to make some small addition to this wonderful survey of the earth, so that what appears very extraordinary to the reader may appear more likely when thus illustrated, I have been at the pains to construct this globe." The differences between this and former globes are considerable, and mark a great advance in geographical knowledge. America, instead of being broken up into many islands, as in all earlier globes, is shown as one large continent of tolerably correct shape; Florida is named for the first time in print; "the Moluccas have found a local habitation and their true places, as well as many of the real isles of the sea, while all the monsters and bogus elements of American geography are made to disappear."

THE new volume issued by Mr. Stevens opens with a long, learned, and most interesting introduction by Mr. Coote, on early American geography generally, and especially on the globes and maps of the first part of the sixteenth century. Mr. Coote also narrates the life of Schöner, and furnishes an estimate of his services to geography. One of his discoveries relating to Schöner is that the place-name *Timiripa*, from which he dates some of his letters, and which has hitherto puzzled all students, is merely the translation of part of the name of a small parish of which Schöner was pastor. The introduction is followed by a facsimile of Schöner's letter of dedication of the globe to the Canon of Bamberg, by the letter of Maximilianus, and by translations of both, as well as by a bibliography of Schöner's works. But, next to the introduction, the portion of the book which will receive most attention will be the facsimiles at the end, which are as follows: (1) the famous Hunt-Lenox globe, attributed to 1506–7: (2) the Boulonger globe, supposed to have been executed in 1514–17; (3) Schöner's first globe of 1515; (4) his second globe of 1520; (5) the third globe of 1523, "being the earliest geographical document to delineate the first circumnavigation of the earth by the Spaniards, 1519–22"; (6) the Portuguese so-called Cantino map of 1502. The reproduction of the letters of Schöner and Maximilianus Transylvanus have been done in exact facsimile by the phototypographic process, all the defects and peculiarities of the originals appearing with faithful minuteness. The long-lost globe consists of twelve gores, and its distinguishing feature is a line drawn completely round the circumference, showing the route of Magellan's fleet in the first circumnavigation of the earth.

THE following message from Mr. Joseph Thomson and Mr. Harved Crichton-Browne, transmitted by the Eastern Telegraph Company's cable from Tangier, has been sent to the Royal Society, the Royal Geographical Society, and to the friends of the explorers:—"City of Morocco, July 28.—We returned to Amsmiz across mountains, safe and well, July 24; many interesting geographical and geological notes; so far successful beyond our expectations. We were prevented going direct from Glamoa to Gundaffy by tribal revolt. We shall start on August 6 for third trip across the Atlas, further south-west this time."

THE GASES OF THE BLOOD.[1]

I.

MR. PRESIDENT AND GENTLEMEN,—The subject I have chosen is a consideration of the gaseous constituents of the blood in relation to some of the problems of respiration. This has been selected both because it deals with a province of physiology in which there are many profound problems connected with the molecular phenomena of life, and also because it gives me the opportunity of illustrating some of the methods of physiological research. I purpose to treat the subject chiefly from the physical stand-point, and to demonstrate some of the phenomena as I would endeavour to do to a class of students, believing that this will be of more interest to many of my audience than if I placed before you anything like an encyclopædic account of recent researches. I cannot help adding that as I speak in the class-room of one of the most distinguished physicists of the day, I feel the genius of the place is hovering over me, and I will be impelled to guide you to the borderland of physics and of physiology. It is in this territory that we meet with the most profound questions regarding the nature of vital activity, and it is here that the physiologist and the physicist must join hands in working out their solution.

Respiration may be shortly defined as the function or group of functions by which an interchange occurs between the gases formed in the tissues of a living being and the gases of the medium in which it lives. It is interesting to take a brief survey of the investigations which laid the foundations of our knowledge of this subject, as it illustrates to us the fact taught by the history of all sciences that those truths which we now regard as elementary were at one time unknown, and have been gained only by laborious inquiry.

The oldest writers do not appear to have had any clear notions even as to the necessity for respiration. Hippocrates dimly recognized that during breathing a *spiritus* was communicated to the body. Many of the older anatomists, following Galen, thought that the "very substance of the air got in by the vessels of the lungs to the left ventricle of the heart, not only to temperate heat, but to provide for the generation of spirits." This notion of cooling the blood was held by Descartes (1596–1650) and his followers, and seemed to them to be the chief, if not the sole, use of respiration. In addition, they supposed it aided in the production and modulation of the voice, in coughing, and in the introduction of odours. The celebrated Van Helmont (1577–1664) strongly expresses these views, and attaches particular importance to the necessity for cooling the blood, which otherwise would become too hot for the body.

About the middle of the seventeenth century clearer notions began to prevail. These rested partly on an anatomical and partly on a physical discovery. Malpighi (1621–94) discovered that the minute bronchial tubes end in air vesicles, or membranous cavities, as he termed them, on the walls of which, in the frog, he saw with his simple microscope the blood flowing through capillaries. This pulmonary plexus was for many years termed the "rete mirabile Malpighii." The physical observations were made by the celebrated Robert Boyle (1627–91), who describes in his treatise entitled "New Experiments, Physico-Mechanical, touching the Spring of the Air," published in 1662, numerous experiments as to the behaviour of animals in the exhausted receiver of the air-pump. He showed that the death of the animals "proceeded rather from the want of air than that the air was over-clogged by the steam of their bodies." He also showed that fishes also enjoyed the benefits of the air, for, said he, "there is wont to lurk in the water many little parcels of interspersed air, whereof it seems not impossible that fishes may make some use, either by separating it when they strain the matter thorow their gills, or by some other way."

His conclusion is "that the inspired and expired air may be sometimes very useful by condensing and cooling the blood;" but "I hold that the depuration of the blood in that passage is not only one of the ordinary but one of the principal uses of respiration." Thus, by the use of the air-pump, invented by Otto von Guericke about 1650, Boyle was able to make a contribution of fundamental importance to physiological science.

He also first clearly pointed out the real cause of the influx of air into the lungs. The older anatomists, from Galen downwards, held that the lungs dilated actively, and thus sucked in the air; and there was much controversy as to whether the chest, with the contained lungs, resembled a pair of bellows, which was filled because it was dilated, or whether the lungs resembled a bladder, which is dilated because it is filled. Boyle shows clearly that the cavity of the chest is actively dilated, and that the lungs are distended because the "spring" of the air is then less on their outer than on their inner surface. This simple explanation was not generally accepted, because the minds of Boyle's contemporaries were under the influence of an ancient idea that air existed in the cavity of the chest external to the lungs. This prevented them from seeing the simplicity and accuracy of Boyle's explanation, and to be constantly on the outlook for some mechanism by which the lungs could actively dilate. Such notions were held by Willis, Malpighi, and Erasmus Darwin. The opinion of Darwin is shown by the following passages in the "Zoonomia":—

"By the stimulus of the blood in the right chamber of the heart, the lungs are induced to expand themselves, and the pectoral and intercostal muscles and the diaphragm act at the same time by their associations with them." And, again, "to those increased actions of the air-cells are superadded those of the intercostal muscles and diaphragm, by irritative association."

Boyle's observations were published in 1660, and in 1685 we

[1] Address to the British Medical Association at its annual meeting at Glasgow. Delivered on August 10 in the Natural Philosophy class-room University of Glasgow, by John Gray McKendrick, M.D., LL.D., F.R.SS.L. and E., F.R.C.P.E., Professor of the Institutes of Medicine in the University of Glasgow.

find Borelli (1608–79), in the second portion of his great work "De Motu Animalium," giving expression to very clear notions regarding respiration. Thus in the eighty-second proposition he shows that the lungs are not the effective causes of respiration, but are passively concerned in the movements; and in the eighty-third proposition he states that the efficient cause of inspiration is the muscular force by which the cavity of the chest is increased and permits the lungs to be filled by the elastic force of the air. Borelli was also the first, as shown in the eighty-first proposition of his work, to make an estimate of the quantity of air expelled by a single expiration. At the same time he attributed calm expiration to the elastic resiliency of the ribs, and he pointed out that the deepest expiration could not entirely empty the lungs of air (Propositions 92, 93, and 94). Whilst Borelli thus recognized the air as necessary to animal life, he naturally failed in explaining why this was so, being unacquainted with the composition of the air and of the so-called "fuligineous vapours" (carbonic acid, aqueous vapour, &c.) which were supposed to exist in expired air.

I find, in a work by Swammerdam (1637–80), dated 1667, and entitled "Tractatus Physico-Anatomico-Medicus de Respiratione usuque Pulmonum," at pp. 20, 21, a description of an experiment in which he immersed in a vessel of water a dog having a long tube inserted in the trachea, and he observed the rise and fall of the level of the water during respiration. This was practically the method followed by Borelli, but I am unable to say which experiment was first performed.

Here I may also refer to the curious experiments of Sanctorius, Professor of Medicine in Padua, who flourished from 1561 to 1636, as being probably the first quantitative estimate of substances escaping from the body. Sanctorius constructed a balance by which he weighed himself repeatedly, and observed what he gained by food and what he lost by excretion. The results appeared in his work "Ars de Staticâ Medicinâ," published in 1614, and he states the amount of matter separated by pulmonary exhalation at about half a pound in twenty-four hours. It is not easy to say precisely what these figures represent, and therefore we find the amount, on the authority of Sanctorius, differently stated by writers during the next century. His observations are of interest, however, as being a distinct step in physiological investigation.

Among the contemporaries of Boyle, Pascal, Spinosa, Barrow, Newton, and Leibnitz—all men of the first intellectual rank—was Dr. Robert Hooke, one of the most versatile and able of scientific thinkers. Hooke was born in 1635, and died in 1703. One of the founders of the Royal Society, its early Proceedings show that there was scarcely any department of science at the time to which he did not make important contributions. In particular, he showed a remarkable experiment, in October 1667, to the Royal Society. This experiment, as detailed in Lowthorp's "Abstract of the Philosophical Transactions," vol. iii. p. 67, showed that it was the fresh air, and not any alteration in the capacity of the lungs, which caused the renewal of the heart's beat. It has been said that a similar experiment was performed by Vesalius, but with this difference, that, whilst Vesalius observed the fact, he failed in giving a rational explanation. He supposed that the movements of the lungs affected the movements of the heart, but he did not see, as Hooke did, that the heart moved because it was supplied with blood containing fresh air. Hooke's experiment is one also of great practical importance as being the basis of the modern practice of using artificial respiration in cases of impending asphyxia.

We thus see that the necessity of a continual supply of fresh air was recognized as being essential to life. It was further surmised that the air imparted something to the blood, and received something in return; but no further advance was made in this direction until the researches of Mayow, a name now famous in the early history of chemistry and of physiology. John Mayow was born in 1645, and died at the early age of thirty-four. His principal work was published in Oxford in 1674. In it, by many ingenious experiments, he showed that combustion diminishes the volume of the air and alters its qualities; that respiration also affects the quality of the air; that an animal will die if kept in a confined space full of air—a fact to be explained, according to Mayow, by saying that the animal had used the respirable portion of the air, and that the residue was unfit for life; and, finally, he showed that an animal suffers if placed in an atmosphere the qualities of which have been injured by combustion. Further, he gave the name of "nitro-aërial spiritus" to the "principle" in the air which, he said, had to do with life, muscular action, and combustion. Thus he no doubt came near the discovery of oxygen, made by Priestley nearly a century later. It would be difficult to estimate the enormous influence on theories of combustion and of respiration exerted by the researches of Boyle, Hooke, and Mayow. They prepared the way in physiological science for the next great step—namely, the identification of the gaseous elements contained in respiration. The dependence of progress in physiology on the state of scientific opinion regarding chemical and physical questions could not be better illustrated than in the history of physiological ideas regarding respiration. Thus the researches of Boyle with the air-pump did much to explain the mere mechanism of breathing. Hooke made this even more apparent, and Mayow gave greater precision to the idea that in respiration the blood lost something and gained something. It is difficult to determine precisely, after the lapse of time, the contributions made by each of these distinguished observers, who were contemporaries; but I would venture to say that the germ of the ideas that bore fruit in the minds of Hooke, and more especially of Mayow, may be found in the writings of Robert Boyle.

The researches of Mayow, indicating the existence in the air of a "nitro-aërial spiritus" necessary to life, and the presence in expired air of something deleterious to life, did not immediately produce the fruits one would have expected. At first, his writings attracted considerable attention; they passed through two or three editions, and were translated for Continental readers; but from the beginning of the eighteenth century, nearly twenty years after Mayow's death, they passed almost into oblivion. Thus Hales vaguely refers to him in only two instances, and, as stated by Bostock, "in the discourse delivered by Sir John Pringle before the Royal Society, upon the assignment of Sir Godfrey Copley's medal to Dr. Priestley, which commences with a sketch of the discoveries that had been made in the science of aërology, previous to the period when this philosopher entered upon his experiments, the name of Mayow is not mentioned."

Mayow's writings were first again brought into notice in this country by Reinhold Forster, who gave a summary of Mayow's views in an introduction to his translation of Scheele's essay on "Air and Fire."

As another example of how Mayow's observations were neglected, it may be pointed out that Boerhaave (1668–1738), one of the most learned men of his time, states that he cannot explain the change which the air experiences by respiration; and even Haller, in his great work "Elementa Physiologiæ Corporis Humani," published in 1766, sums up his knowledge regarding expired air by stating that it is combined with a quantity of water and a noxious vapour, and has its elasticity diminished.

The next step in the physiology of respiration was the discovery, in 1754, of carbonic acid, by Joseph Black, then Professor of Medicine and Chemistry in this University. About this time there was much discussion in the medical world as to the use of lime-water in cases of stone and gravel. It was supposed that the lime-water dissolved calculi, and assisted in expelling them from the body. A discussion arose as to the virtues of lime-water produced from different substances. Two Professors in the University of Edinburgh—Alston and Whytt—specially investigated the subject, and Whytt asserted that the lime-water of oyster-shell lime had more power as a solvent than the lime-water of common stone lime. This led Black to examine the question. "I therefore," says he, "conceived hopes that, by trying a greater variety of the alkaline earths, some kinds might be found still more different by their qualities from the common kind, and perhaps yielding a lime-water still more powerful than that of oyster-shell lime."

This led Black to his celebrated investigation on magnesia. He showed that in the case of magnesia alba (carbonate of magnesia) the disappearance of the effervescence on treatment with an acid after heating was accompanied by a loss of weight. The substance thus given off he called "fixed air," or what we now term carbonic acid. This led to an examination of the salts of lime, and in 1757 he made two important physiological discoveries, namely: (1) that the fixed air was injurious to animal life; and (2) that fixed air was produced by the action of respiration. These important observations are thus described in his own words:—"In the same year, however, in which my first account of these experiments was published—namely, 1757—I had discovered that this particular kind of air, attracted by alkaline substances, is deadly to all animals that breathe it by the mouth and nostrils together; but that if the nostrils were kept shut, I was led to think that it might be breathed with safety. I found, for example, that when sparrows died in it in ten or eleven seconds, they would live in it for three or four minutes, when the nostrils were shut by melted suet. And I convinced myself that the change produced on wholesome air by breathing it, consisted chiefly, if not solely, in the conversion of part of it into fixed air. For I found that by blowing through a pipe into lime-water, or a solution of caustic alkali, the lime was precipitated, and the alkali was rendered mild. I was partly led to these experiments by some observations of Dr. Hales, in which he says that breathing through diaphragms of cloth dipped in alkaline solutions made the air last longer for the purposes of life."

Fifteen years afterwards—namely, in 1772—Joseph Priestley examined the chemical effects produced by the burning of candles and the respiration of animals upon ordinary air; and he made the important discovery that, after air had lost its power of supporting combustion, as by the burning of candles, this property might be restored by the agency of plants. Pushing his experiments still further, he found that air, deteriorated by the breathing of animals, might again become suitable for respiration by the action of plants. In these experiments he employed mice for ascertaining how far an air was impure or unfit for respiration. In 1774, Priestley obtained oxygen by heating red precipitate by means of the sun's rays concentrated by a burning-glass. This led to an investigation of the constitution of the atmosphere, and it was shown that it was not a homogeneous elementary body, but consisted of two gases, and that its constitution was remarkably uniform. Priestley showed that by fermentation, combustion, the calcination of metals, and respiration, the air lost a portion of one of its constituents, oxygen.

Thus the chemical researches of Black and Priestley proved that in respiration oxygen was consumed and carbonic acid produced, although the latter fact, owing to the theoretical views of Priestley as to phlogiston, was not fully appreciated by him.

Within a year after Priestley's discovery, a paper on respiration was written by Lavoisier (1743–94), in which he showed that Priestley was correct in stating that the air lost oxygen in breathing, but Lavoisier specially pointed out that it had gained carbonic acid. No doubt Lavoisier was well acquainted with Black's researches, as is shown by the correspondence between these distinguished men. Lavoisier was the first, however, to make a quantitative examination of the changes produced in the air by breathing. In 1780, he performed a remarkable experiment, in which a guinea-pig was confined over mercury in a jar containing 248 cubic inches of gas consisting principally of oxygen. In an hour and a quarter the animal breathed with much difficulty, and, being removed from the apparatus, the state of the air was examined. Its bulk was found to be diminished by 8 cubic inches, and of the remaining 240 inches, 40 were absorbed by caustic potash, and consequently consisted of carbonic acid. Still later, he performed a more accurate experiment, giving quantitative results. During 1789 and 1790, by a special apparatus, Lavoisier and his friend Seguin attempted to measure the changes in the air produced by the breathing of man. These researches are not of value so much for the results they gave as for the method employed. Lavoisier constructed a still more elaborate apparatus, with which he began experiments. This research, however, he never finished, as, in 1794, he fell a victim to the blind fury of Robespierre. It is narrated that he earnestly requested a respite of a few days to give him time to prepare for publication the results of his investigations. This was denied, and thus perished one of the greatest scientific sons of France.

Stephen Hales (1677–1761) attempted to measure the amount of aqueous vapour given off by the lungs by breathing through a flask filled with wood-ashes, which absorbed the moisture, and he estimated the amount at about 20 ounces in twenty-four hours. Similar observations were afterwards made by Menzies and by the eminent surgeon, Mr. Abernethy. Lavoisier also attacked the problem by an indirect method. Thus he determined the quantity of oxygen consumed and of carbonic acid produced, and, assuming that the amount of oxygen was more than sufficient to form the carbonic acid, he came to the conclusion that the excess united with hydrogen in the lungs, and passed off as water. As may be supposed, this method gave widely different results.

Various other attempts were made to estimate the amount of the respiratory changes. In particular, Sir Humphry Davy, in March 1798, investigated the physiological action of nitrous oxide gas. In this research, published in 1800, he began by observations upon animals; and observations as to the effect of the gas on life, on muscular irritability, on the action of the heart, and on the colour of the blood are recorded with great precision. He then passed on to observations on the respiration of hydrogen, and this led him to a repetition of the experiments of Lavoisier and Goodwin. Next he subjected himself and friends to experiment, and recorded a number of interesting physiological and psychical phenomena. This research is of great historical interest as being the first leading to the discovery of a method of producing anæsthesia, or insensibility to pain, by breathing vapours or gases.

Another eminent man who contributed largely to the physiology of respiration was Lazarus Spallanzani, who was born in 1729 and died in 1799. He was educated under the direction of the Jesuits. When about sixteen years of age he went to Bologna, and studied at that University, specially under the tuition of his cousin, Laura Bassa, a woman celebrated in her day for eloquence and scientific knowledge, and who was then a Professor in the University. His biographer, Senebier, says:—"Under the direction of this enlightened guide he learned to prefer the study of Nature to that of her commentators, and to estimate their value by comparing them with the originals they professed to describe. The scholar at once perceived the wisdom of these counsels, and quickly experienced their happy effects. He evinced his gratitude to his instructress in a Latin dissertation published in 1765, which was dedicated to Laura Bassa, and in which he recounted the applauses she received at Modena when, entering the hall, where her pupil, on being appointed a Professor, was defending a thesis, 'De Lapidibus ab Aquâ Resilientibus,' she opposed it with the graces of an amiable woman and the wisdom of a profound philosopher."

Spallanzani became Professor of Logic, Mathematics, and Greek in Reggio in 1754, and about this date he published researches on Infusoria. In 1760, he became Professor in the University of Modena. In 1765, he showed that many microscopic animalcula were true animals, and in 1768 he published his celebrated researches on the reproduction of portions of the body removed from worms, snails, salamanders, and toads. He paid special attention to the great question of spontaneous generation, showing that infusions of animal and vegetable substances exposed to a high temperature, and hermetically sealed, never produced living things. He also investigated respiration, more particularly in invertebrates. He proved that many such animals breathed by means of the skin as well as by the special breathing organs. He placed many animals, but more especially different species of worms, in atmospheres of hydrogen and nitrogen, and found that, even in these circumstances, carbonic acid was produced. He also showed the production of carbonic acid by the dead bodies of such animals, and reasoned from this that the carbonic acid was produced directly from the dead tissues and not from the action of the oxygen of the air. He contrasts the respiration of cold-blooded and warm-blooded animals, and shows the peculiarities of respiration in hibernating animals. Nor were these by any means superficial observations. They were usually quantitative, and by the use of the eudiometer, he analyzed the air before and after respiration. Probably the most important contribution made by Spallanzani to the subject was showing what he states in the following paragraph:—

"I inquire not here why the quantity of carbonic acid gas was greater in azotic and hydrogen gas than in common air. I shall only conclude, from these experiments, that it is clearly proved that the carbonic acid gas produced by the living and dead snails in common air resulted not from atmospheric oxygen, since an equal and even a greater quantity of it was obtained in azotic and hydrogen gas; consequently, in the oxygen gas destroyed by the presence of these animals, its base alone is absorbed by them either during life or after death."

But Spallanzani supposed that the carbonic acid thus produced was formed by digestion in the stomach, passed through the tissues, and was then exhaled. Thus he missed a great step in discovery—namely, that the carbonic acid is produced by the tissues themselves. It was, however, pointed out in 1823, by W. F. Edwards, in his work on the "Influence of Physical Agents on Life," that the amount of carbonic acid produced by animal breathing was too great to be accounted for by the amount of oxygen in their lungs at the beginning of the experiment, or by carbonic acid supposed to be in the stomach. The importance of this observation will be seen when we discuss the phenomena of the breathing of the tissues.

In 1809 the subject of aquatic breathing was investigated with great care by Provençal and Humboldt. They collected and analyzed the gases of water before and after fishes had lived in it for a certain time, and showed that oxygen was consumed and carbonic acid produced by these creatures.

We have now seen how gradually knowledge was arrived at as to the respiratory exchanges. At the beginning of the present century it was recognized that expired air had lost oxygen, gained carbonic acid and aqueous vapour, and had become hotter. Since then many researches have been carried on to determine with accuracy the quantities of these substances. In all of these, as shown in these diagrams,[1] the method followed has been to draw through a chamber containing the animal a steady constant stream of air, the quantity and composition of which is known. Thus, suppose a certain quantity of dry air, free from carbonic acid, and consisting only of oxygen and nitrogen, is passed through such a chamber. In the chamber some of the oxygen is consumed, and a certain amount of carbonic acid and of aqueous vapour is given up by the animal. The air is drawn onwards through bulbs or glass tubes containing sub-tances such as baryta-water, to absorb the carbonic acid, and chloride of calcium or sulphuric acid, to absorb the aqueous vapour. It is evident that the increased weight of these bulbs and tubes, after the experiment has gone on for some time, will give the amounts of carbonic acid and aqueous vapour formed. Thus Andral and Gavarret in 1843, Vierordt in 1845, Regnault and Reiset in 1849, von Pettenkofer in 1860, and Angus Smith in 1862, determined the quantities both by experiments on animals and on human beings.

The results are—first, the expired air, at its own temperature, is saturated with aqueous vapour; secondly, the expired air is less in volume than the inspired air to the extent of about one-fortieth of the volume of the latter; thirdly, the expired air contains about 4 per cent. more carbonic acid and from 4 to 5 per cent. less oxygen than inspired air; fourthly, the total daily excretion of carbonic acid by an average man amounts to 800 grammes in weight, and 406 litres in bulk. This amount of carbonic acid represents 218·1 grammes of carbon and 581·9 grammes of oxygen. The amount of oxygen, however, actually consumed is about 700 grammes; so that nearly 120 grammes of oxygen absorbed are not returned by the lungs, but disappear in the body. It must be remembered, however, that carbonic acid escapes by the skin and other channels. These figures may be taken as averages, and are subject to wide variations depending on nutritional changes.

There is, however, another side to the problem of respiration—namely, a consideration of the chemical changes involved in the process.

According to Lavoisier, respiration was really a slow combustion of carbon and of hydrogen. The air supplied the oxygen, and the blood the combustible materials. The great French chemist, however, did not entirely commit himself to the opinion that the combustion occurred only in the lungs. He says that a portion of the carbonic acid may be formed immediately in the lung, or in the blood-vessels throughout the body, by combination of the oxygen of the air with the carbon of the blood. Lavoisier's opinions were understood correctly by only a few of his contemporaries, and a notion prevailed that, according to him, combustion occurred only in the lungs, and that the changes in these organs were the main sources of animal heat. Such a notion, however, was contrary to the opinion of the great mathematician Lagrange, announced in 1791, a few years after the first publication of Lavoisier's on respiration. Lagrange saw that, if heat were produced in the lungs alone, the temperature of these organs might become so high as to destroy them; and he therefore supposed that the oxygen is simply dissolved in the blood, and in that fluid combined with carbon and hydrogen, forming carbonic acid and aqueous vapour, which were then set free in the lungs. It will be observed that this opinion of Lagrange in 1791 was practically the same as that stated by Lavoisier in 1789.

Now, if the production of carbonic acid in a given time depended upon the amount of oxygen supplied in the same time, these views of Lavoisier and Lagrange would be correct; but Spallanzani had shown that certain animals confined in an atmosphere of nitrogen or of hydrogen exhaled carbonic acid to almost as great an extent as if they had breathed air. He was therefore obliged to say that carbonic acid previously existed in the body, and that its appearance could not be accounted for by the union of oxygen with the carbon of the blood. Spallanzani therefore thought that in the lung there was simply an exhalation of carbonic acid and an absorption of oxygen These views were supported by the experiments of W. Edwards, published in 1824. Edwards showed that animals in an atmosphere of hydrogen produced an amount of carbonic acid not to be accounted for by any oxygen supposed to exist free in the body. In 1830, Collard de Martigny performed many similar experiments, and stated that carbonic acid was secreted in the capillaries and excreted by the lungs. This opinion was supported by Johannes Müller, who repeated the experiments of Spallanzani.

It might thus be said that two theories of respiration were before physiologists—the one, that combustion occurred in the lungs or venous blood, furnishing carbonic acid and aqueous vapour, which were exhaled by the lungs; the other, that there was no such combustion, but that oxygen was absorbed by the lungs and carried to the tissues, whilst in these carbonic acid was secreted, absorbed by the blood, carried to the lungs, and there exhaled. Some writers, soon after Lavoisier, misunderstood, as I have already stated, the opinions of that distinguished man, and taught that in the lungs themselves there was a separation of carbon, which united immediately with the oxygen to form carbonic acid. But this was really not Lavoisier's opinion; and we have to do, therefore, with two theories, which have been well named—the theory of combustion, and the theory of secretion.

The difficulty felt by the older physiologists in accepting the secretion theory was the absence of proof of the existence of free oxygen and carbonic acid in the blood. This difficulty also met those who rejected the notion of combustion occurring in the lungs, and substituted for it the idea that it really occurred in the blood throughout the body, because, if this were true, free gases ought to be found in the blood. Consequently, so long as physiologists had no definite knowledge regarding gases in the blood, the combustion theory, in the most limited sense, held its ground. This theory, although fruitful of many ideas regarding respiration and animal heat, was abandoned in consequence of the evidence afforded by two lines of inquiry—namely, researches regarding the gases of the blood, and researches as to the relative temperature of the blood in the right and left cavities of the heart.

Let me first direct your attention to the gradual development of our knowledge regarding the gases of the blood. The remarkable change in the colour of the blood when it is exposed to, or shaken up with, air was observed so long ago as in 1665 by Fracassati, and is also alluded to by Lower (1631–91), Mayow, Cigna (1773), and Hewson (1774); but Priestley was the first to show that the increased redness was due to the action of the oxygen of the air, and that the blood became purple when agitated with carbonic acid, hydrogen, and nitrogen. The presence of gas in the blood was first observed about 1672 by Mayow. I find in a paper of Leeuwenhoek (1632–1723), entitled "The Author's Experiments and Observations respecting the Quantity of Air contained in Water and other Fluids," published in 1674, a description of a method devised by this ingenious man for detecting the existence of air in certain fluids, and amongst them in the blood. It consisted of a kind of syringe, by which he was able to produce a partial vacuum. He then observed bubbles of gas to escape, and he estimated, in the case of human blood, that the air in the blood amounted to 1/1000 or 2/1000 part of the volume of the blood. He argues, from this interesting observation, against one of the prevalent medical theories of the time, that various diseases were caused by fermentations in the blood. How, said he, was such a theory consistent with the existence of so small a quantity of gas? He made the mistake, from the inefficiency of his apparatus, of stating that blood, when it issues from the veins, contains no air.

Gas was also obtained from the blood in 1799 by Sir Humphry Davy, in 1814 by Vogel, in 1818 by Brand, in 1833 by Hoffmann, and in 1835 by Stevens. On the other hand, John Davy, Bergmann, Johannes Müller, Mitscherlich, Gmelin, and Tiedemann failed in obtaining any gas. The first group of observers, either by heating the blood, or by allowing it to flow into a vacuum, or by passing through it a stream of hydrogen, obtained small quantities of carbonic acid. Sir Humphry Davy was the first to collect a small quantity of oxygen from the blood. John Davy, by an erroneous method of investigation, was led, in 1828, to deny that the blood either absorbed oxygen or gave off carbonic acid. He was shown to be wrong, in 1830, by

[1] Diagrams exhibited on wall.

Christison, who devised a simple method of demonstrating the fact.

So long as the evidence in favour of the existence of gases in the blood was so uncertain, the combustion theory of respiration held its own. At last, in 1836, appeared the researches of Heinrich Gustav Magnus, latterly Professor of Physics and Technology in the University of Berlin. He first attempted to drive off carbonic acid from the blood by a stream of hydrogen, and thus obtained as much as 34 cubic centimetres of carbonic acid from 62·9 cubic centimetres of blood. He then devised a mercurial air-pump, by which it was possible to exhaust a receiver to a much greater extent than could be done by the ordinary air-pump. When blood was introduced into such a vacuum, considerable quantities of carbonic acid, oxygen, and nitrogen were obtained. This research marks an epoch in physiological discovery, as it threw a new light on the function of respiration by demonstrating the existence of gases in the blood.

In order to appreciate the value of this evidence, and the method employed, let me direct your attention to the laws regulating the diffusion of gases. As a mass of gaseous matter has no independent form, like that of a solid body, nor a fixed volume like that of a liquid, but consists of an enormous number of molecules which, in consequence of their mutual repulsions, endeavour more and more to separate from each other, it is easy to see that if two masses of gas are brought into contact, they will mix—that is, their molecules will interpenetrate, until a mixture is formed containing an equal number of the molecules of each gas. The force by which the molecules repel each other, and by which they exercise pressure in all directions, is known as the pressure or tension of the gas. It is evident that the greater the number of gas molecules in a given space, the greater will be the tension of the gas, and from this it follows that the tension of a gas is in the inverse proportion to its volume (this is known as Boyle's law). Suppose now that two gases are separated by a porous partition; the two gases will mix, and the rapidity of the diffusion will vary according to the specific weight of the gases. Thus light gases, like hydrogen or coal-gas, will diffuse more quickly than air, or chlorine, or carbonic acid.

It is important also to note the laws regulating the absorption of gases by fluids. If we allow a little water to come into contact with ammonia gas above mercury, the gas is rapidly absorbed by the water (1 volume of water absorbs 730 volumes NH_3) all the gas above disappears, and in consequence of this the pressure of outer air drives up the mercury in the tube. The higher the temperature of the fluid the less gas it absorbs. At the boiling-point of the fluid its absorption is = 0, because at that temperature, the fluid itself changes into gas. The power of absorption of different fluids for the same gas, and the absorptive power of the same fluid for different gases fluctuates between wide limits. Bunsen defined the coefficient of absorption of a fluid for a gas as that number which represents the volume of gas (reduced to 0° and 760 mm. barometric pressure) which is taken up by 1 volume of the fluid. Thus 1 volume of distilled water takes up the following volumes:—

Temp. Cent.	N.	O.	CO_2.	Air.
0°	0·02	0·041	1·797	0·025
5	0·018	0·036	1·5	0·022
15	0·015	0·03	1·002	0·018
37	—	0·02	0·569	—

Again, 1 volume of distilled water at 0° C. absorbs 0·00193 volumes of hydrogen, while it can take up no less than 1180 volumes of ammonia; again, 1 volume of water at 0° C. absorbs only 0·2563 volumes of olefiant gas, but 1 volume of alcohol, at the same temperature, will take up as much as 3·595 volumes. The volume of gas absorbed is independent of the pressure, and the same volume of gas is always absorbed whatever the pressure may happen to be. But as according to Boyle's law the density of a gas, or in other words the number of molecules in a given space, is in proportion to the pressure, and as the weight is equal to the product of the volume and the density, so while the volume absorbed always remains the same, the quantity or weight of the absorbed gas rises and falls in proportion to the pressure (this is the law of Dalton and Henry). It therefore follows that a gas is to be considered as physically absorbed by a fluid, if it separates from it not in volumes but in quantities, the weights of which are in proportion to the fall of pressure.

When two or more gases form an atmosphere above a fluid, the absorption takes place in proportion to the pressure which each of the constituents of the mixture would exercise if it were alone in the space occupied by the mixture of gases, because, according to Dalton's law, one gas does not exercise any pressure on another gas intermingled with it, but a space filled with one gas must be considered, so far as a second gas is concerned, as a space containing no gas, or in other words a vacuum. This pressure, which determines the absorption of the constituents of a gaseous mixture, is termed, according to Bunsen, the partial pressure of the gas. The partial pressure of each single gas in a mixture of gases depends, then, on the volume of the gas in question in the mixture. Suppose atmospheric air to be under a pressure of 760 mm. of mercury, then, as the air consists of 21 volumes per cent. of O and 79 volumes per cent. of N, $\frac{760 \times 21}{100} = 159·6$ mm. of mercury, will be the partial pressure under which the oxygen gas is absorbed, while the absorption of nitrogen will take place under a pressure of $\frac{760 \times 79}{100} = 600$ mm. of mercury. Suppose, again, that above the fluid containing a gas, say carbonic acid, which has been absorbed, there is an atmosphere of another gas, say atmospheric air, then as carbonic acid exists in the air only in traces, its tension is equal to zero, and carbonic acid will escape from the fluid until the difference of tension between the carbonic acid in the water and the carbonic acid in the air above it has been balanced—that is, until the carbonic acid which has escaped into the air has reached a tension equal to that of the gas still absorbed by the fluid. By the phrase "tension of the gas in a fluid" is understood the partial pressure in millimetres of mercury which the gas in question has to exercise in the atmosphere, when no diffusion between the gas in the fluid and the gas in the atmosphere takes place.

The method followed by Magnus will now be understood. By allowing the blood to flow into an exhausted receiver surrounded by hot water, gases were set free. These were found to be oxygen, carbonic acid, and nitrogen. He further made the important observation that both arterial and venous blood contained the gases, the difference being that in arterial blood there was more oxygen and less carbonic acid than in venous blood. Magnus concluded that the gases were simply dissolved in the blood, and that respiration was a simple process of diffusion, carbonic acid passing out and oxygen passing in, according to the law of pressures I have just explained.

Let us apply the explanation of Magnus to what occurs in pulmonary respiration. Venous blood, containing a certain amount of carbonic acid at the temperature of the blood and under a certain pressure, is brought to the capillaries, which are distributed on the walls of the air-vesicles in the lungs. In these air-vesicles, we have an atmosphere at a certain temperature and subject to a certain pressure. Setting temperature aside, as it may be assumed to be the same in the blood and in the air-cells, let us consider the question of pressure. If the pressure of the carbonic acid in the blood be greater than that of the carbonic acid in the air-cells, carbonic acid will escape until an equilibrium is established between the pressure of the gas in the blood and the pressure of the gas in the air-cells. Again, if the pressure or tension of the oxygen in the air-cells be greater than that of the oxygen in the venous blood, oxygen will be absorbed until the tensions become equal. This theory has no doubt the merit of simplicity, but it will be observed that it depends entirely on the assumption that the gases are simply dissolved in the blood. It was pointed out by Liebig that, according to the experiments of Regnault and Reiset, animals used the same amount of oxygen when breathing an atmosphere composed of that gas alone as when they breathed ordinary air, and that the vital processes are not much affected by breathing the atmosphere of high altitudes where the amount of oxygen taken in is only about two-thirds of that existing at the sea level. It was also shown at a much later date, by Ludwig and W. Müller, that animals breathing in a confined space of air will use up the whole of the oxygen in the space, and it is clear that as the oxygen is used up the partial pressure of the oxygen remaining must be steadily falling. Liebig urged the view that the gases were not simply dissolved in the blood, but existed in a state of loose chemical combination which could be dissolved by the diminished pressure in the vacuum, or by the action of other gases. He also pointed out the necessity of accurately determining the coefficient of absorption of blood for the gases—that is, the amount absorbed under a pressure of 760 mm. of mercury

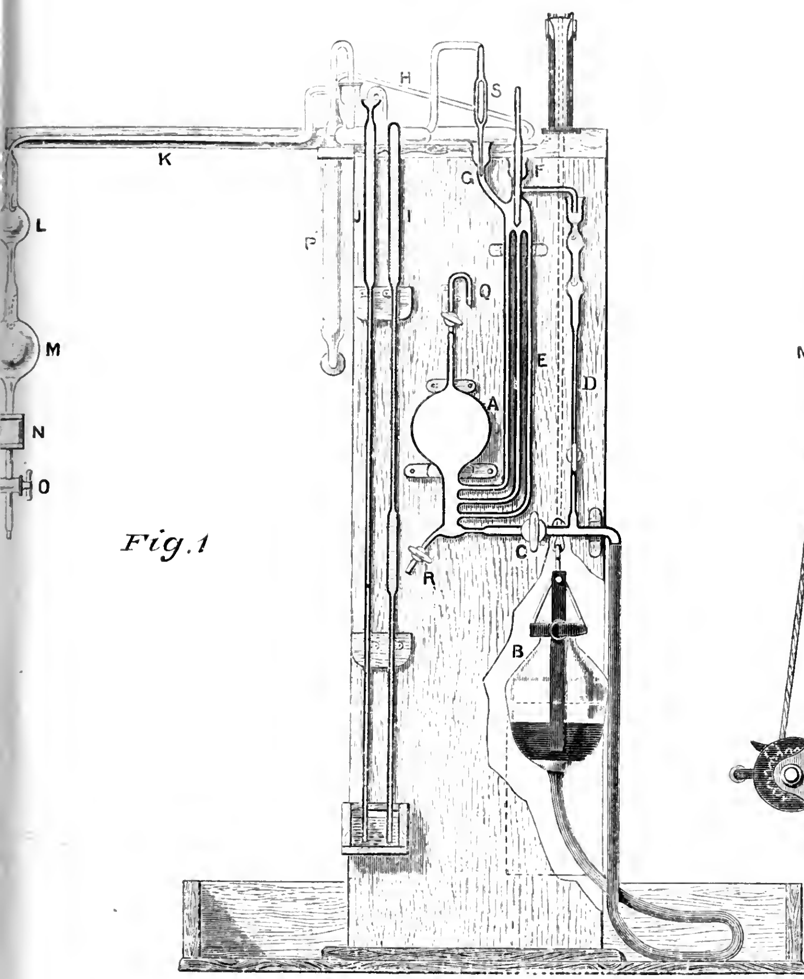


Fig. 1

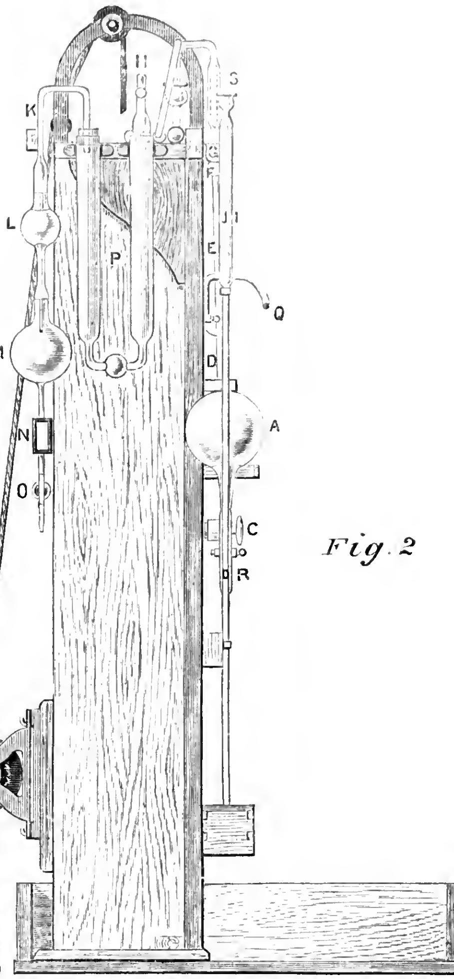


Fig. 2

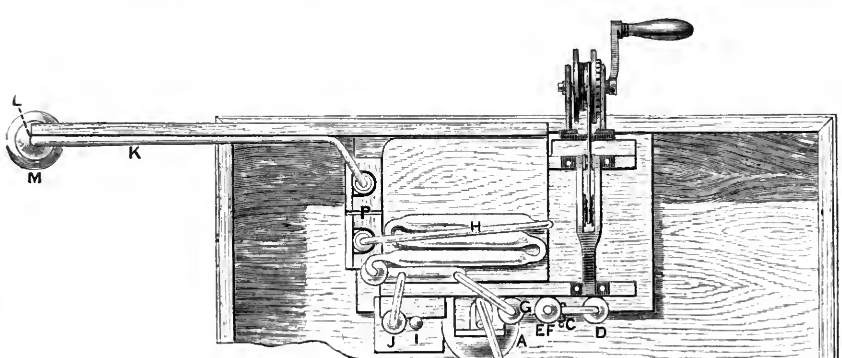


Fig. 3

Description of Figures.

Figs. 1, 2, and 3.—Views of a gas pump constructed for the purpose of extracting and collecting the gases of the blood and suitable for the physiological lecture table. These views have been correctly drawn on the scale of 1 to 10 by my friend the Rev. A. Hanns Geyer.[1] Fig. 1, front view: A, glass bulb connected by horizontal glass tube with bulb B; this tube guarded by stopcock C. By elevating B, A is filled with mercury, stopcock of delivery tube Q is closed, and B is lowered; A is thus exhausted and air is drawn into it by tubes E, connected by G with drying apparatus and blood chamber. I, permanent barometer; J, barometer gauge tube connected with part of instrument to be exhausted. Both I and J dip into mercury trough seen below; S, a glass float to prevent mercury from running into drying apparatus when B is raised. After A and the drying apparatus and the blood chamber have been well exhausted, B is raised and mercury may be allowed to pass up D, and then the apparatus acts as a Sprengel pump by the three tubes E. Fig. 2, side view of apparatus: same references. Fig. 3, drying apparatus, placed on a shelf at the top of the pump, consisting of H, tubes containing solid phosphoric acid, and U-tube P, seen in Fig. 2, containing sulphuric acid. The tube K passes to receiver. In the drawing it is seen to be connected with an apparatus suitable for projecting the spectrum of oxy-hæmoglobin by lime or electric light on screen; then exhausting the blood of oxygen and showing the spectrum of reduced hæmoglobin. L and M, froth chambers with traps; N, parallel-sided chamber for blood; O, stopcock. The whole pump is modelled on one I obtained about ten years ago from Messrs. Mawson and Swan, of Newcastle, but it has been much altered and added to so as to make it suitable for physiological demonstration. It is evident that the gases can be readily obtained for analysis by driving out of A by delivery tube Q. A rough demonstration of the gases can be made in from five to ten minutes.

[1] The pump can be obtained from Mr. W. Potter, glass-blower, Physical and Physiological Laboratories, University of Glasgow, who will give information as to cost.

by one volume of the gas at the temperature of the observation. The next important observations were those of Fernet, published in 1855 and 1857. He expelled the greater part of the gas of the blood (dog) by passing through it a stream of hydrogen and then submitting it to the action of the air-pump. He then introduced into the apparatus the gas under a given pressure, the absorption coefficient of which he had to determine. He then estimated the amount of gas absorbed, under different pressures, and found in the case of oxygen that the amount absorbed with gradually decreasing increments of pressure was greater than what would have been the case had it been in accordance with Dalton's law of pressures. The oxygen was not then simply dissolved in the blood. Further, Fernet arrived at the conclusion that the greater portion of the oxygen was in a state of combination, whilst a small amount was simply dissolved according to Dalton's law.

It is evident, then, that while the amount of oxygen absorbed varies with the pressure, it does not do so according to Dalton's law. The amount decreases slowly with pressures below atmospheric pressure, and it increases very rapidly with pressures above it. It is when the pressure in the vacuum is as low as one-thirtieth of an atmosphere that the oxygen is given up, and this will be about the pressure of the aqueous vapour in the apparatus at the temperature of the room, when the experiment is made. The view that something in the blood is chemically united to the oxygen is strengthened by the fact that serum does not absorb much more oxygen than water can absorb, so that blood at a temperature of 30° C. would contain only about 2 volumes per cent. of oxygen gas were the latter simply dissolved in the fluid. It can also be shown that defibrinated blood takes up oxygen independently of the pressure, and that the quantity of oxygen taken up by defibrinated blood is about equal to the quantity absorbed by a solution of pure hæmoglobin containing as much of that substance as exists in the same volume of blood.

By similar experiments made with carbonic acid, Fernet determined that the greater portion of it was in a state of loose chemical combination, whilst a small amount was simply dissolved according to the law of pressures. Experiments with blood serum showed similar results as regards carbonic acid, with the difference that the coefficient of absorption for oxygen was much less than with ordinary blood. He therefore concluded that nearly the whole of the carbonic acid was chemically retained in the fluid of the blood, whilst nearly the whole of the oxygen was combined with the red blood corpuscles. He then proceeded to investigate whether or not the three principal salts of the blood, carbonate of soda, phosphate of soda, and chloride of sodium, in any way influenced the absorption coefficient of carbonic acid. He found (1) that the addition of these salts to distilled water in the proportion in which they exist in the serum slightly diminishes the absorption coefficient; (2) that chloride of sodium has no influence on the absorption coefficient; and (3) that carbonic acid combines with the carbonate and phosphate of soda.

In the same year (1855) Lothar Meyer published the results of a series of researches of the same nature. Under the direction of Bunsen, the blood was diluted with ten times its bulk of water, and the gases were collected by boiling the liquid *in vacuo* at a very gentle heat; a certain amount of gas was thus obtained. He also found that blood absorbs a much larger quantity of carbonic acid than pure water at the same temperature, and stated that when blood was exposed to oxygen at various pressures the quantity of that gas taken up might be regarded as consisting of two portions, one following Dalton's law and the other independent of it.

Further researches of a similar kind have been carried out by Setschenow, Ludwig, Alexander Schmidt, Bert, Pflüger, and others, and ingenious methods of collecting and of analyzing the gases have been devised. To Prof. Pflüger and his pupils, in particular, are we indebted for the most complete series of gas analyses on record. The result has been to enable us to give the average composition of the gases of the blood as follows. From 100 volumes of dog's blood there may be obtained—

	Oxygen.	Carbonic Acid.	Nitrogen.
Arterial	18·4 to 22·6, mean 20	30 to 40	1·8 to 2
Venous	Mean 11·9	43 to 48	1·8 to 2

the gases being measured at 0° C. and 760 mm. pressure. The venous blood of many organs may contain less than 11·9 per cent. of carbonic acid, and the blood of asphyxia may contain as little as 1 volume per cent. It is clear, then, that the gases of the blood do not exist in a state of simple solution, but that they are largely combined with certain constituents of the blood. Take, for example, the case of oxygen. Berzelius showed long ago that 100 volumes of water will absorb, at a given temperature and pressure, 2·9 volumes of oxygen; while, in the same circumstances, 100 volumes of serum will absorb 3·1 volumes, and 100 volumes of blood will absorb 9·6 volumes. Something in the blood must have the power of taking up a large amount of oxygen.

(*To be continued.*)

THE BATH MEETING OF THE BRITISH ASSOCIATION.

THE arrangements for the Bath meeting of the British Association are now practically completed. The Reception Room, adjoining the Assembly Rooms, will be opened on Monday, September 3, at 1 p.m., and on each succeeding week-day till Thursday, September 13, at 8 a.m. precisely; on Sunday, September 9, from 8 to 10 a.m., and from 3 to 6 p.m. In this building will be the offices of the General and Local Secretaries and Treasurers, a post office, telegraph office, telephone, ticket office, lodgings, inquiry, excursion, and lost property offices, and offices for the supply of all official papers and programmes. There will also be lavatories, cloak-rooms, &c., &c. The Council of the Association will meet in the Guildhall.

In the Reception Room there will be offices for supplying information regarding the proceedings of the meeting. The tickets contain a map of Bath, and particulars as to the rooms appointed for the Sectional and other meetings. A list of lodgings, or apartments, with prices, &c., and also information concerning hotels, and other similar matters, will be furnished by the Lodgings Clerk between the hours of 9 a.m. and 6 p.m. daily, at No. 13 Old Bond Street, up to 1 p.m. on Monday, September 3, and after that time at the Reception Room between the same hours daily.

The places of meeting, &c., will be in the Assembly Rooms, the Drill Hall, and the Guildhall. The Secretaries of Sections will be lodged at the White Lion Hotel. The following are the Section Rooms:—A, Mathematics, St. James's Hall; B, Chemistry, Friends' Meeting House; C, Geology, Mineral Water Hospital; D, Biology, Mineral Water Hospital; E, Geography, Guildhall; F, Statistics, Christ Church Hall; G, Mechanics, Masonic Hall; H, Anthropology, Grammar School; Sub-Sections C and D, Blue-Coat School.

By the courtesy and liberality of the Directors of the Western Counties and South Wales Telephone Company, the whole of the Section Rooms will be telephonically connected with the Reception Room, and, through the Telephone Exchange, with all important places in the neighbourhood, free of any expense to the Local Executive Committee, or members and associates, for the meeting.

The first general meeting will be held on Wednesday, September 5, at 8 p.m. precisely, in the Drill Hall, when Sir H. E. Roscoe, M.P., F.R.S., will resign the chair, and Sir Frederick Bramwell, F.R.S., President-Elect, will assume the Presidency, and deliver an address. According to the *Times*, Sir Frederick is sure to deal pretty largely with progress in the department with which his name is so eminently connected. With regard to the addresses of the Presidents of Sections the *Times* makes the following statement:—In Section A (Mathematics and Physics), Prof. Fitzgerald is President, and the subject of his address will probably be connected with Clerk-Maxwell's theory that electric and magnetic forces are produced by the same medium that propagates light, and some recent experimental proofs of that theory. In Section B (Chemistry), Prof. W. A. Tilden, of Birmingham, is President, and his address will be concerned with the history of the teaching of chemistry practically, and will review the existing provision for efficient teaching of chemistry in this country. This will be followed by some discussion of the methods actually used or proposed for teaching chemistry either as a constituent part of a liberal education or for technical purposes, together with an endeavour to trace the causes of the unproductiveness of the English schools in respect to advanced studies, and especially in regard to the results of original research. Prof. Boyd Dawkins is President of Section C (Geology). Among other points which he is likely to discuss will be the following:—That in the history of life on the earth the more complex forms have changed more swiftly than the simpler, because they are more susceptible to changes in their environment. That in the Tertiary age the highest of all, or the placental mammals, are the only forms which have changed with sufficient swiftness to mark the subdivisions of the Tertiary period. They alone are *en pleine évolution*. The borderland between geology and history will be discussed, and the present series of events shown to belong to the Tertiary period. The place of man in the geological record will be considered (pre-glacial). The impossibility of fixing historic dates for geological events will also be discussed. Outside the written record a sequence of events can alone be made out, in which we are ignorant of the length of the intervals. In Section D (Biology), of which Mr. Thiselton Dyer, Director of Kew Gardens, is President, no doubt we may expect some of those discussions on subjects of general biological interest which have been so marked a feature of the Section since Prof. Ray Lankester was its President at Southport. Colonel Sir Charles Wilson presides over Section E (Geography), and his address will deal largely with the commercial aspects of geography. In Section F (Economics), of which Lord Bramwell is President, the Presidential address is likely to be brief, and will deal with the general principles of political economy, and with socialism in particular. Mr. W. H. Preece, of the Telegraph Department, will preside over Section G (Mechanical Science). In his address he will pass under review the various practical applications of electricity, with the introduction of nearly all of which Mr. Preece has been more or less associated. He will also probably say something about the present views of the theory of electricity, about which practical electricians and pure physicists are at entire variance. Finally, in Section H (Anthropology), the address of the President, General Pitt-Rivers, is, like Lord Bramwell's, likely to be short.

Discourses will be delivered in the Drill Hall—on Friday evening, September 7, by Prof. W. E. Ayrton, F.R.S., on "The Electrical Transmission of Power"; on Saturday Evening, September 8 (to "the operative classes"), by Sir John Lubbock, M.P., F.R.S., on "The Customs and Ideas of Savage Races"; on Monday evening, September 10, by Prof. T. G. Bonney, F.R.S., on "The Foundation Stones of the Earth's Crust."

The Mayor of Bath invites the members and associates to a *conversazione* in the Assembly Rooms on Thursday, September 6, at 8.30 p.m. The Chairman and members of the Local Executive Committee invite the members and associates to a *conversazione* at the Assembly Rooms, on Tuesday, September 11, at 8.30 p.m. On this occasion the Bath Microscopical Society, assisted by the Bristol Microscopical Society, have arranged for a display of objects in the various departments of natural history, &c. No special cards of invitation will be issued to these *conversaziones*, but all members and associates will be admitted on presentation of their tickets.

The concluding general meeting will be held on Wednesday, the 12th of September, at 2.30 p.m.

On Wednesday and Thursday, the 5th and 6th of September, there will be an exhibition of fruits, flowers, &c., in the Sydney Gardens; to this exhibition all members and associates will be admitted on presentation of their tickets. On the 12th and 13th of September there will be a horse show in Bath; but on this occasion the members and associates will have no special advantages.

The following are the proposed excursions, arrangements for which are in active progress:—

Saturday, September 8.—Stanton Bury, Stanton Drew, Maes Knoll: Bannerdown, Sodbury Camp, Dyrham, Lansdown: Box Quarries, Corsham, Lacock Abbey: Bradford, Farleigh Castle, Wraxall: Cirencester, Museum and College: Tytherington and Thornbury: Swindon, G.W. Works: Berkeley Castle: Wells, *viâ* Maesbury and Shepton Mallet, Ebbor, Wookey Hole: Barry Docks and Cardiff.

Thursday, September 13.—Stonehenge, Salisbury, Wilton: Silbury, Avebury, Bowood, Wansdyke, Beckhampton: Stourton, Pen Pits, White Sheet, Longleat: Frome Valley, Nunney Whateley: Maesbury, Wells, Glastonbury, Street: Sandford and Banwell, Churchill, Dolbury, Rowberrow, Burrington, the two Charterhouses, Mendip Gorge, Cheddar Cliffs: Severn Tunnel, Chepstow, Tintern, Wyndcliffe: Radstock, Wellow, Littleton.

SOCIETIES AND ACADEMIES.

London.

Entomological Society, August 1.—Dr. D. Sharp, President, in the chair.—Mr. F. D. Godman, F.R.S., exhibited a large number of species of Lepidoptera and Diptera recently collected for him in Mexico by Mr. Herbert Smith.—Mr. White exhibited parasites bred from *Bombyx neustria*, and a living example of *Heterodes guyoni*, found at Dartford, and believed to have been introduced with Esparto grass from Tunis.—Mr. Enock exhibited a stem of barley, showing the appearance of the plant under an attack of Hessian fly.—Mr. Stevens exhibited a number of galls collected at Byfleet in July last; also a specimen of *Coleophora solitariella*, with ichneumons bred from it.—Mr. E. Saunders exhibited a specimen of *Catephia alchymista*, captured at St. Leonards, in June last. He also exhibited specimens of a rare ant (*Anochetus ghiliani*), taken at Tangier by Mr. G. Lewis. One of these he had submitted to Dr. Emery, of Bologna, who thought that, although ocelli were present, the specimen was probably intermediate between a worker and a female, and that possibly the true female did not exist.—Mr. Pascoe exhibited a number of species of Coleoptera recently collected in Germany and the Jura Mountains, and read a note correcting the synonymy of certain species of *Brachycerus* recently described by him in the Transactions of the Society. He stated that the corrections had been suggested by MM. Peringuey and Aurivillius.—Prof. Westwood communicated a paper entitled "A List of the Diurnal Lepidoptera collected in Northern Celebes by Dr. Sydney Hickson, with descriptions of new species."

Edinburgh.

Royal Society, July 16.—Rev. Prof. Flint, Vice-President, in the chair.—Dr. Traquair read an obituary notice of Mr. Robert Gray, Vice-President.—A paper by Prof. C. G. Knott, Tokio University, on some relations between magnetism and twist in iron and nickel, was submitted.—Mr. R. Kidston communicated a paper on the fossil plants in the Ravenhead collection in the Liverpool Museum.—Prof. Crum Brown submitted an investigation by Mr. Alex. Johnstone on the action of carbonic acid water on olivine.—In a paper discussing the question, Is Talbot's law true for very short stimuli? Dr. G. N. Stewart, Owen's College, describes experiments designed to test whether it is possible to make the luminous stimuli so short that the separate effects cannot be summed. He was able, by means of a rotating mirror, to reduce the length of each stimulus to something like 1/8,000,000 sec. Up to this limit he could detect no variation from the law.—Another paper by Dr. Stewart, on some colour phenomena observed with intermittent stimulation with white light, was communicated. When light of moderate intensity is used, and the rate of stimulation gradually increased, the colour is seen to change regularly in a manner which can be explained on the assumption that the curves representing the course of the excitation in the three hypothetical fibre-groups run in such a way that with a certain length of stimulation time the violet fibres are proportionally more stimulated than the others; with a shorter time of stimulation the green fibres are more stimulated; with a still shorter time, the red.—Dr. H. R. Mill, Scottish Marine Station, discussed the specific gravity of the water in the Firth of Forth and the Clyde sea-area.—Dr. J. Macdonald Brown read a paper on arrested twin development.—The Chairman made some remarks in closing the session.

Paris.

Academy of Sciences, July 30.—M. Janssen, President, in the chair.—On the relations of atmospheric nitrogen to vegetable soil, by M. Th. Schlœsing. The conclusion already arrived at from previous researches (see *Comptes rendus* for March 19 and 26, 1888) is fully confirmed by the results of the subsequent series of experiments here described. Whether exposed to renewed contact with the air, or kept in closed vessels with a confined but oxygenated atmosphere, the soil with which the experiments have been made has in no case fixed any appreciable quantity of gaseous nitrogen. The author supplements this communication with some remarks on the quantitative analysis of the carbon and nitrogen in vegetable earths. The main object of these remarks is to enable chemists to judge for themselves as to the degree of confidence his conclusions are entitled to.—On the density of chlorine and on the vapour density of ferric chloride, by MM. C. Friedel and J. M. Crafts. For chlorine the mean at 21° C. is here determined at 2·471, and at 440° C. 2·448, while between 321° and 442° C. the perchloride of iron is shown to have a somewhat constant density corresponding to the formula Fe_2Cl_6.—On the vapour density of the perchloride of gallium, by MM. C. Friedel and J. M. Crafts. According to Lecoq de Boisbaudran's determinations the perchloride of gallium (Ga_2Cl_6) melts at 75°·5 and boils at 215° to 220°. Here the density at 237° and 307° is found to be 11·73 and 10·61 respectively, or somewhat less than the theoretic density. Above 307° it diminishes considerably, falling to 8°·5 at 357°, and 6°·6 at 440°.—On the gigantic dimensions of some fossil mammals, by M. Albert Gaudry. These remarks are made in connection with the accurate measurements of the St. Petersburg mammoth (*Elephas primigenius*) supplied by Tilesius. The skeleton, a photograph of which has recently been taken by M. Strauch, is 3·42 metres high to the top of the head, as compared with the 4·22 of the Durfort skeleton (*Elephas meridionalis*) in the new gallery of the Paris Museum. Comparing these with the remains of *Dinotherium giganteum* and other monsters of the Upper Miocene and later epochs, the author groups the larger extinct mammals according to their dimensions in five classes, as follows: (1) *Dinotherium giganteum* of the Upper Miocene, Attica; (2) *Elephas antiquus* of the Quaternary, neighbourhood of Paris; (3) *Elephas meridionalis* of the Upper Pliocene, Durfort (Gard); (4) *Mastodon americanus*, of the Quaternary, United States; (5) *Elephas primigenius*, of the Quaternary, Siberia, this last being about the same size as the living elephants.—Observations of the comet 1888 *a*, by M. Cruls. These observations were made at the Imperial Observatory of Rio Janeiro for the period from February 24 to April 2.—Positions of the comet 1888 I., measured with the 8-inch equatorial of the Observatory of Besançon, by M. Gruey. The positions of the comet and comparison stars are given for the period from June 7 to June 19.—An isochronous regulator, by M. Baudot. The object of this apparatus is to maintain at a uniform velocity the rotation of the distributor employed by the inventor in his multiple printing telegraph system, despite the variations of the motor power and those of the resisting force caused by the action of the several parts of the instrument, or by any other disturbing element. Its action consists in introducing into the motor mechanism a resistance varying automatically whenever necessary, thus maintaining a perfect equilibrium between the total motor and resisting forces.—On a telephone with closed magnetic field, and plaque with equal concentric cylindrical sections, by M. Krebs. With the appliance here described the vibrations preserve a large degree of amplitude, while the section is saturated at no point of the magnetic circuit. These dispositions greatly facilitate the construction of powerful instruments of all sizes.—Magnetic charts of the West Mediterranean basin, by M. Th. Moureaux. The magnetic charts which the author now presents to the Academy have been mainly prepared from the data supplied by the series of observations described in the last number of the *Comptes rendus*. They comprise, besides the chief islands, the whole of the European seaboard from Cadiz to the Strait of Messina, and the North African coast between Tangier and Tripoli.—The storage of electricity and thermodynamics, by M. Gouy. In this paper the author endeavours to connect the principle of the preservation of electricity with the general laws of thermodynamics, taking as his experimental starting-point the first law of electric actions.—On the electric conductibility of mixtures of salts in solution, by MM. E. Bouty and L. Poincaré. In the present communication the authors deal mainly with the special case of the nitrates of potassa and soda, their object being to ascertain whether it be possible to deduce the electric conductibility of a mixture of saline solutions, without chemical action, from the conductibility of each, assuming this to be a known quantity.—On the production of ozone by electric shocks, by MM. Bichat and Guntz. Here the authors propose to study the various circumstances which influence the production of ozone by means of explosive discharges. The results obtained show that the formation of ozone is primarily connected with the greater or less elevation of the temperature of the oxygen under the action of the electric shocks.—Notes follow, by M. A. Carnot, on the lithine present in mineral waters; by M. J. Ribau, on a method of analyzing and separating zinc; by M. de Forcrand, on the glycol-alcoholate of soda; by M. J. Meunier, on a dibenzoic ether derived from mannite; by M. E. Gley, on the comparative toxic properties of wabaine and strophanthine; and by M. Prillieux, on an efficaceous treatment of black rot, a disease of the vine which has spread from America to France.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Speaking Parrots, Part 4: Dr. K. Russ (L. U. Gill).—British Dogs, No. 22: H. Dalziel (L. U. Gill).—*Challenger* Expedition Reports—Zoology, vol. xxvi. (Eyre and Spottiswoode).—Contributions to the Natural History of Alaska, No. 2: L. M. Turner (Washington).—A New Theory of Parallels; C. L. Dodgson (Macmillan).—Atlantic Weather Charts, Part 4 (Eyre and Spottiswoode).—Arithmetical Exercises and Examination Papers: H. S. Hall and S. R. Knight (Macmillan).—Entomology for Beginners: Dr. A. S. Packard (Holt, New York).—Catalog der Conchylien-Sammlung, Liefg. 8: F. R. Paetel (Berlin).—The Structure and Classification of the Mesozoic Mammalia: H. F. Osborn (Philadelphia).—Insect Life (Washington).—Il Terremoto nel Vallo Cosentino del 3 Dicembre, 1887: G. Agamennone (Roma).—Morphologisches Jahrbuch, Band 14, Heft 1 (Williams and Norgate).—Annalen der Physik und Chemie, 1888. No. 9 (Leipzig).—Verhandlungen des Naturhistorischen Vereines, 5 Jahrg. Erste Hälfte (Bonn).—Annual Report of the American Museum of Natural History, Central Park, New York, for the Year 1887–88.

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THURSDAY, AUGUST 23, 1888.

BRITISH PETROGRAPHY.

British Petrography: with Special Reference to the Igneous Rocks. By J. J. Harris Teall, M.A., F.G.S. With Forty-seven Plates. (London: Dulau and Co., 1888.)

THIS handsome volume, with its beautifully chromolithographed plates, supplies a want that has long been felt in English scientific literature. It was scarcely fitting that in this country, where the application of the microscope to the study of thin sections of rock was first suggested and practically carried out, there should exist no comprehensive work dealing with the chief varieties of our native rocks, as illustrated by their microscopic characters.

In its general appearance, plan, and scope, this volume reminds one so closely of the "Minéralogie Micrographique: Introduction à l'Étude des Roches Éruptives Françaises," of MM. Fouqué and Michel Lévy, that it is scarcely possible to avoid a comparison between the two works. Artistically, the forty-seven plates of the English treatise may perhaps even claim superiority over the fifty-five plates in the French work; though in the exact presentation of minute but characteristic details, and in the accuracy of tints employed, the palm must in some cases be awarded to the latter. There are some plates in the volume before us, however, in which truthful delineation of details has been so admirably combined with a general beauty of effect as practically to leave nothing to be desired in work of this class.

Like his French predecessors, the author of this volume has found it desirable to go outside of the country illustrated for a few of his types of igneous rock. A striking testimony, however, to the variety as well as the beauty of our native rocks is found in the circumstance that it has been possible to present so complete a selection of the chief types of igneous materials without going beyond the limits of the British Isles except in two instances,—those, namely, of the Lherzolite of the Ariège, and of the Lencitic rock of the Eifel. Nor are the varieties of British igneous rocks by any means exhausted in the illustrations of the work before us. The rhyolites, which perhaps are less adequately represented than some other groups, might have had their more crystalline varieties (Nevadites) well illustrated by the beautiful rocks of Tardree, Co. Antrim, while examples of trachyte of graphic-granite, and of various types of granulites and "trap-granulites," might have been easily obtained from Scotland. On the whole, however, we think the author has shown excellent judgment in his selection of types, and he is to be heartily congratulated upon his success in, securing accurate drawings, and exact reproductions of those drawings by the process of chromolithography—results which we are assured could not have been attained without much labour and extreme care.

Although the book is one which is especially noteworthy for the beauty of its illustrations, it would be a mistake to suppose that it belongs to that class of works in which everything else is sacrificed to showy plates, and scientific accuracy is regarded as merely a secondary object. On the contrary, the author has clearly devoted great pains to the perfecting of his text, which constitutes in itself an excellent introduction to the study of petrography. Some of the rocks chosen for illustration have already been described by other authors, and in these cases Mr. Teall, while doing full justice to the labours of his predecessors and contemporaries, has not unfrequently been able to extend, supplement, or correct their results by the light of more recent researches; in the case of rocks which have not been previously described, the author has himself investigated their chemical and microscopical characters, in some instances in a very complete and exhaustive manner. In all cases he has earned the gratitude of students by the copiousness of his references to the ever-growing mass of literature which deals with the question of the minute structure of minerals and rocks.

While MM. Fouqué and Lévy have devoted the text of their work to a systematic description of the various species of rock-forming minerals, and especially of those characters which enable us to recognize them when seen in thin sections under the microscope, the author has aimed rather at describing the rocks themselves, incidentally discussing the characters of each species of mineral as it presents itself in the different groups of rocks. This plan, while attended with certain advantages, may perhaps be objected to on the ground that it is only possible to gather the whole of the conclusions of the author upon any particular mineral after consulting different and widely-separated portions of the book. This is rendered more easy, however, by the very full index which is supplied.

The work, we are informed in the preface, was commenced as a serial publication, and to this cause probably must be ascribed its most serious defect as a means of instruction: this is the absence of references and cross-references between the text and the atlas of plates; these, indeed, constituting two practically independent works. Had all the plates been before the author during the time that he was preparing the text, he would frequently have been able to illustrate his remarks upon the minerals and structures in the rocks he is describing by references to his own admirable drawings. To the same cause, too, we must ascribe the only other serious blemish we have detected in the book—a rather large proportion of misprints, which, though usually obvious enough to the initiated, may occasion considerable embarrassment to the student.

However much the beginner, taking up this attractive volume, may be delighted with the mode of study of which it aims at giving an exposition, he will scarcely be led into the fatal error of supposing that everything necessary to a person seeking to employ the method is a microscope and some rock-sections. The author makes it perfectly clear that unless the student is prepared to go through a certain amount of preliminary training, the microscopic examination of a rock is more likely to lead to error rather than to truth. So much knowledge of crystallography as will enable the observer to appreciate the position of any section with respect to the axes of the crystal, and such an acquaintance with the principles of physical optics as will suffice to guide him in interpreting the chief phenomena revealed, when either plane or convergent polarized light are employed, are absolutely indispensable. But in addition to these there is a vast mass of knowledge, which has been gradually acquired and is ever increasing, concerning the internal peculiarities of minerals, especially such as appear in the varieties that constitute rocks, and with respect to the wonderful series of changes which they undergo when exposed to different conditions; and the more of this kind of knowledge the student can bring to the investigation of a rock the less liable will he be to fall into error. In this branch of science, as in every other, the experience which can only be obtained by long-continued study of the subject must always supplement, and may sometimes even supersede, the results obtained by the application of rigid rules of of procedure.

As a suggestion has recently been made in the pages of NATURE that all which is required to secure a uniform and uniformly-acceptable classification and nomenclature of rocks is that some master of the modern methods of research should bring in a sweeping "reform bill" on the subject, it may be well to quote the author's views upon petrographic notation and classification. Writing after the two years of careful labour devoted to the preparation of this work, he remarks:—

"As regards the classification of rocks, I am sorry to say that increasing knowledge has not tended to bring about any clearness of view. The more rocks are studied the less they seem to me to adapt themselves to any classification at all comparable in definiteness with the classifications of organic bodies and mineral substances. Rock-masses often vary so much in composition and structure that any scheme of classification based on work done in the laboratory is unsuitable for the expression of broad geological facts. It is absolutely impossible to map the different varieties recognized by modern petrographers. The conclusion at which I have arrived is that the necessity for giving names to rocks arises rather from work done in the field than from work done in the laboratory. Rock specimens are mineral-aggregates, and may be described as such. Rock-masses are integral portions of the earth's crust, and possess a certain amount of individuality in virtue of their mode of occurrence."

With these remarks we very cordially agree. Systematic mineralogy is a branch of natural-history science; for, in their crystalline forms and chemical constitution, minerals supply safe criteria which enable us to define species and varieties, and also permit us to group these into larger divisions. But most petrographical classifications seem to be of value only so long as we confine our attention to the selected fragments that fill the cases in a petrographical museum. In the field one type is often found passing into another which the mere petrographer may have placed in a totally different class.

There is perhaps just now a danger of our exaggerating the importance of the microscopic method as applied to the study of rocks. That the method has already done much in enabling us to follow out and trace the effects of the slow processes of change within the earth's crust, and that it will do still more in the future, no one can doubt. But when it is sought to make the microscope a "court of final appeal" in geological questions, and in doing so to disregard the importance of field-observation, we perceive the same source of danger as is now perhaps being experienced in connection with almost every branch of natural-history research. It must be remembered that, while the microscope enables us to see a little more than the naked eye or the pocket lens, yet nevertheless, between what is actually seen by the very highest powers of our microscopes and the molecular groupings and reactions which give rise to the varied phenomena of the mineral kingdom, there is room for almost infinite possibilities. We accept the teaching of the microscope with all thankfulness, but we recognize the fact at the same time that it has enabled us to get only a very little nearer to the heart of those great physical problems which we aim at solving.

In congratulating the author upon the completion and publication of a book which, as we learn from his preface, has occasioned him no little anxiety as well as so much labour, we may express the hope that his project of treating the aqueous and metamorphic rocks in the same attractive and thorough fashion may be realized. We cannot conclude this notice without a word of commendation for the excellent glossary of terms used in describing rocks, which has been supplied by Dr. F. H. Hatch, and will, we are assured, prove of the greatest service to students. JOHN W. JUDD.

SILKWORMS.

Silkworms. ("Young Collector Series.") By E. A. Butler, B.A., B.Sc., Author of "Pond Life: Insects," &c. (London: Swan Sonnenschein, Lowrey, and Co, 1888.)

THE silkworm is so familiar an insect to everyone, and is interesting from so many points of view, that we gladly welcome this small volume from the pen of a well-known writer on popular natural history. The space which can be allotted to this subject in works on general zoology, or even on general entomology, is necessarily small; and when we consider that a whole library could be written on the history and structure of any single insect, a book dealing almost exclusively with *Bombyx mori* should be a useful addition to our entomological literature. The present work is fairly comprehensive in its scope, and is written in such a manner as to be intelligible to everyone, however ignorant of natural history. Numerous woodcuts are added, wherever they seem to be required to elucidate the text.

Mr. Butler appears to be adequately acquainted with his subject, and we have glanced through his book without noticing any very serious errors, or meeting with many statements which we felt disposed to question. But we can hardly accept the inconceivable narrative which Mr. Butler has copied from the *Entomologist* on pp. 78 and 79, about a male and female moth being developed upside down in a single pupa formed by a single larva. Until more instances of a similar nature are recorded, we fancy that most charitably-disposed people will be inclined to imagine that some extraordinary error must have occurred. In this case, and in a few others, Mr. Butler quotes his authorities. Although it would be unfair to expect the author of a work like the present to quote authorities throughout, we think that it would have been more satisfactory to Mr. Butler's readers, especially to those who may wish to go further into the subject, if he had indicated in a brief preface the chief sources from whence he had derived his information, and how far portions of it were based upon his own observations.

We must take exception to one statement (on p. 79) as rather too sweeping. "Silk-producing *Lepidoptera* belong exclusively to two families, the *Bombyridæ* and the *Saturniidæ*." All, or very nearly all, *Lepidoptera* produce more or less silk; but even if we understand Mr. Butler to mean "all *Lepidoptera* which produce silk of economic value," he would still have spoken too positively, for we believe that various species belonging to the *Lasiocampidæ*, and perhaps to other families of Bombyces, have been used as silk-producers in various countries; as, for example, *Libethra cajani* in Madagascar.

Mr. Butler has divided his work into six chapters. The first treats of "The History of Silk Culture," and contains a sketch of the gradual progress of silk-culture and manufacture, and of the introduction of these industries into one country after another, from their commencement in China, according to tradition, about 2600 B.C., to the present time. One point seems to have been overlooked, viz. the modern origin of the name Morea for the Peloponnesus, and its derivation from the mulberry-tree.

The second chapter, "The Silkworm: its Form and Life-History," deals with the metamorphoses, and the external structure and changes of the insect in its various stages. The mode of denuding the wings to examine the neuration; parthenogenesis, and other incidental matters, are likewise noticed. Mr. Butler objects to the term "nervures" as applied to the branching tubes which traverse the wings of butterflies and moths; but we may be permitted to point out that such terms, when used in a purely technical and conventional manner, though frequently incorrect in themselves, rarely mislead anyone.

Chapter III., "The Silkworm: its Internal Structure," treats, of course, of internal anatomy. Detailed directions are given for dissecting silkworms. The chapter closes with remarks on Lyonnet's great work on the anatomy of the larva of the goat-moth, and with a detailed explanation of the position of *Bombyx mori* in the system of Nature.

Chapter IV., "The Silkworm: its Rearing and Management," notices some of the principal races of silkworms, the manner of rearing them, and the mode of preparing the silk. The last paragraph briefly alludes to some allied species of true *Bombyx*.

Chapter V. deals with "The Silkworm: its Diseases and Imperfections." The three most serious diseases, flaquerie, muscardine and pebrine, are discussed rather fully, as well as M. Pasteur's method of combating pebrine by microscopic examination of brood females.

In the concluding chapter (VI.), the author discusses "Wild Silkworms," many of which he figures. His treatment of this part of the subject is necessarily somewhat brief, but this is the less to be regretted, as those who wish for further information will probably find much of what they require in Mr. Wardle's "Hand-book of the Collection illustrative of the Wild Silks of India, in the Indian Section of the South Kensington Museum." This book was published by the Science and Art Department of the Committee of Council on Education in 1881; and though earlier in date, it will be found a most useful appendix to Mr. Butler's work.

Mr. Butler himself may fairly be congratulated on his success in compressing so large an amount of useful matter as his book contains into the moderate compass of just 100 pages.

W. F. KIRBY.

OUR BOOK SHELF.

Allgemeine Geologie. Von Dr. Karl von Fritsch, Professor an der Universität in Halle. (Stuttgart: J. Engelhorn, 1888.)

THIS is one of a very useful series of volumes which is appearing under the editorship of Dr. Friedrich Ratzel, with the title of "Library of Geographical Handbooks." As the subjects of glaciers and of volcanoes and earthquakes have had special volumes of the series devoted to their discussion, while many other problems of geological interest are treated of in separate monographs, such as those which deal with the geography of the ocean, and the morphology of the earth's surface, Dr. von Fritsch has been able to limit the scope of the work now before us to certain definite lines of inquiry. The first division of the book is devoted to "Geophysiography," or a discussion of the features of the earth as a member of the solar system, and of the relations of the atmosphere and ocean to the lithosphere or solid crust of the globe. The second division, "Geotektonik," deals with the forms and relations of the rock-masses that build up the solid crust, and is treated with considerable fullness, the illustrations being for the most part new, and not of the kind which find a place in the ordinary text-books of geology. In the third part, "Geochemistry," or chemical geology, we have a short sketch of the present state of petrography, or the description of rocks, followed by remarks on petrogeny, or the theory of their origin. It would be unfair to expect, in the 175 pages at the author's disposal, anything like a complete treatment of the numerous and difficult problems presented by petrological science at the present day, but it is certainly possible to conceive of a bolder and more masterly treatment of the whole question than is found in the present work. "Geomechanik," or physical geology, treats of the questions usually grouped by English writers under the head of dynamical geology; and the fifth and concluding portion of the work is devoted to "Geogenie," or a general sketch of historical geology. The work is of interest to English students and teachers of geological science, as illustrating the general methods of treatment of the subject which prevail in Germany. Without aiming at the comprehensive character which belongs to the well-known treatises of Credner and Gümbel, this book forms an admirable sketch of the chief facts and theories of geological science, which are presented always in an attractive and sometimes in a somewhat novel manner.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Functionless Organs.

IN an interesting letter which appeared in NATURE (p. 341), under the above title, the Duke of Argyll brings forward a "doctrine of prophetic germs" as explanatory of certain rudimentary structures. He refers particularly to the electrical organ of the skate, which he regards as an example of such a germ. The doctrine is that these functionless organs are not structures which have been useful, but are endowed with "utilities yet to be."

In the lecture which I gave at the Royal Institution, on "Electrical Fishes," in May 1887, I pointed out, in discussing the particular instance referred to, that the difficulty suggested by your distinguished correspondent was familiar to Mr. Darwin, and that it was dealt with by him in the sixth chapter of the "Origin," in what seemed to me to be the only way which was then, or is now, possible. We should learn to understand it, he said, by observing "by what graduated steps" [electrical organs] "have been developed in each separate group of fishes." By this I understand him to have meant that what we require to know is, under what conditions the development of electrical organs has actually taken place.

On morphological grounds, we know that a striped muscular fibre taken together with its nerve, and the electrical disk of the organ of the skate taken together with its nerve, are homologous structures—that is, that they are made up of corresponding parts, and have corresponding places in the normal order of development; so that they are in collateral, not in sequential, relation to each other. In other words, both spring from the same origin, not one from the other; and the development of one is quite as *normal* as of the other. An electrical organ is no more an abnormal muscle, than a muscle a misdeveloped electrical organ.

In accordance with Mr. Darwin's teaching, external conditions, whether antecedent or collateral, influence development only in accordance with morphological laws—that is, with the normal order of development. In the present instance we have some knowledge of the order, but the conditions are unknown; and what we have to do is to ascertain what conditions of existence have given predominance to one order rather than to the other, so as, in certain cases, to determine the development of apparatus for producing electrical discharges in place of apparatus for doing mechanical work.

This is the problem, and it will take a long time to investigate it. We know a great deal more now than Mr. Darwin did twenty-five years ago about the structure, development, and mode of working of the electrical organ, but scarcely more than he did about the "why" of its existence in such animals as the skate. Nor shall we be able to give any better account of it until time and opportunity have been afforded for the examination and comparison of a much larger number of instances than are at present accessible to us.

I need only add a word as to his Grace's suggestion that the electrical organ of the skate may be regarded as a "prophetic germ." I would observe that, although in some species of skate the organ is imperfect, it shows no sign of incompleteness in others, and therefore cannot be properly designated a germ. As to the organ being prophetic, I am not sure that I understand what the word means. If the prophecy is such as might encourage the present race of skates to hope to be provided at some future period with more efficient apparatus, I am afraid that any such expectation on their part would be illusory.

Oxford, August 15. J. BURDON-SANDERSON.

ON the part of, I believe, a very large class of unprofessional students of science and theology, I should like to express the profound dissatisfaction, not unmingled with irritation, with which we have read the Duke of Argyll's recent contributions to the subject of evolution. The complete collapse of the grave charges made against the advocates of evolution in the article entitled "A Great Lesson" in the September (1887) number of the *Nineteenth Century*, is too well known to need comment.

The letter on "Functionless Organs" affords another instance of the illogical and dogmatic style with which we are too familiar. Passing over any notice of the absolute inconceivability of any *cause* for the development of "prophetic structures," the Duke of Argyll once more repeats the exploded notion that "the element of fortuity is inseparable from the idea of natural selection," whereas, as has been proved over and over again, the ideas of fortuity and of evolution, of which process natural selection is so integral a part, are absolutely incompatible. But perhaps the climax is reached in the following quotation: "Hitherto I have never yet met with a case in which an expert interprets functionless organs as structures on the way to use." Having at last found a solitary case which, it is thought, by one expert, may be interpreted against the Darwinian conception of evolution, he immediately jumps to the conclusion that "everywhere, in reasoning and observation, it is breaking down."

Apropos of Mr. J. G. Hurst's pertinent queries on p. 364 of your last issue, it may be well to recall the Duke of Argyll's dictum given in the "Reign of Law," *i.e.* that in man's structure "there is no aborted member. Every part is put to its highest use." SAMUEL F. WILSON.

Warsop, August 18.

Lamarckism *versus* Darwinism.

IT is to be regretted that Dr. Romanes has not written anything which can be considered as a reply to my letter. Although Prof. Weismann's essays, to which I referred, are certainly "two of the most notorious essays in the recent literature of Darwinism," it is nevertheless equally certain that a large and important part of their contents is devoted to the consideration of the causes of variation. This being the case, I may safely leave the evidence in support of the statement in my first letter to anyone who will take the trouble to read p. 841 of the June number of the *Contemporary Review*. As it is probable that many people have already read the article in question, and that others may be induced to do so as a result of this correspondence, I think that on this account it may be worth while for Dr. Romanes to notice the criticism, and if possible to show that his remark about Prof. Weismann is intended to bear some other than its obvious meaning.

I need hardly make any further reference to the second and third paragraphs of Dr. Romanes's letter, for I have already explained my position in my first letter. I need only reassert that I was in no way influenced by Dr. Romanes's remarks or opinions about myself; nor am I concerned to allude to the personal references contained in his letter, except to express regret if anything in the form as apart from the substance of my first letter should have caused the annoyance which Dr. Romanes takes no pains to conceal.

In conclusion, it may be worth while to draw attention to the curious coincidence which brings into the same number of NATURE a letter from Prof. E. Ray Lankester, containing an expression of opinion diametrically opposed to that of Dr. Romanes upon the interesting question of Lamarck *versus* Darwin. EDWARD B. POULTON.

Oxford, August 17.

WITH reference to the recent revival of what may be considered as "pure" Lamarckism, it appears to me of importance that those who have followed the course of biological work and thought in this direction should at the present juncture declare their views with respect to the interpretation of such results as those obtained by Mr. Poulton, and referred to by Dr. Romanes in his letter of August 9 (p. 364). I am glad of the present opportunity of discussing this matter, because Mr. Poulton's work is to a large extent an expansion and experimental confirmation of views to which I gave expression in a paper published in 1873 (Proc. Zool. Soc., p. 159). I have no desire to enter into the personal question as to whether Dr. Romanes has or has not made himself acquainted with Weismann's essays, but I must express my disappointment that he has not given us a more explicit statement concerning the precise manner in which he interprets the experiments in the Lamarckian sense. For my own part I may add that I have had opportunities of witnessing Mr. Poulton's experiments at intervals during their progress, and of discussing their bearings with him, and I must confess that I am at present completely at a loss to see how they can by any means be interpreted in the manner Dr. Romanes suggests.

The conclusions at which I arrived in the paper referred to may be very briefly summarized. We find in many species of insects, &c., a variability in colour which is distinctly of an *adaptive* character, enabling the insect to become adapted to a variable environment, and thus being obviously advantageous to the possessors of such a faculty. From this it seemed but a natural conclusion that such a power of adaptability should have been conferred by the usual operation of the law of the survival of the fittest. This conclusion I ventured to draw in 1873, after carefully considering all the cases which I could collect. But in thus grouping what I called at the time "variable protective colouring" among the biological phenomena capable of being regarded as the result of the action of natural selection, I was careful to point out that the precise mechanism of the process by which this adaptability was brought about remained to be investigated for each case. This is the work which has been so admirably carried out by Mr. Poulton for certain Lepidopterous larvæ, pupæ, and cocoons, and the results which he has obtained go far to show that this adaptability in colour is possessed by a much larger number of species than was formerly suspected, and that the modification is invariably in the direction of protection. The experiments prove also that the stimulus prompting the colour change is given by the colour of the surroundings, but the precise means by which the stimulus is conveyed to the pigment-secreting cells has not yet been made out. This part of the work is no doubt the most difficult to deal with from the experimental side, but any objection to the Darwinian explanation which may be urged from the point of view of our ignorance of the nature of this correlation between an external stimulus and the power of secreting a particular colour applies with equal or greater force to the theory of "direct action" upon which so much stress is laid by the new Lamarckian school. The difficulty in the way of completing the explanation of this kind of action is of precisely the same nature as that which meets us when we attempt to explain the power of colour adaptability in a frog or fish as depending upon a colour stimulus, which in these cases is known to be conveyed through the eye. All that is contended for is that the power of adaptation has been conferred by natural selection, an agency capable of dealing with complex physiological relationships in precisely the same way that it deals with all other kinds of variations. In these cases of variable protective colouring we are concerned with the *origin* of the initial variations only in the same manner that we are concerned with their origin in ordinary cases of protective resemblance. Why the colour variability should always be restricted to the limits of protective shades is perfectly intelligible from the purely Darwinian stand-point, but is, as it appears to me, absolutely devoid of meaning if we accept the theory of "direct action." R. MELDOLA.

August 18.

MODERN VIEWS OF ELECTRICITY.[1]

PART IV.—RADIATION.

IX.

So far as we have been able to understand and explain electrical phenomena, it has been by assuming the existence of a medium endowed with certain mechanical or *quasi*-mechanical properties, such as mobility, incompressibility or infinite elasticity of volume, combined with a certain amount of plasticity or finite elasticity of shape. We also imagined the medium as composed of two opposite constituents, which we called positive and negative electricity respectively, and which were connected in such a way that whatever one did the other tended to do the precise opposite. Further, we were led to endow each of these constituents with a certain amount of inertia, and we recognized something of the nature of friction between each constituent and ordinary matter.

Broadly speaking we may say—

(1) That *friction* makes itself conspicuous in the discussion of current-electricity or the properties of conductors, and that the laws of it are summarized in the statement known by the name of Ohm, viz. that the current through a given conductor is proportional to the force that drives it, or that the opposition force exerted by a conductor upon a current is simply proportional to the strength of that current.

(2) That *elasticity* is recognized as necessary when studying the facts of electrostatics or the properties of insulators—electric displacement and recoil, or charge and discharge: the laws having been studied by Faraday, and the relative pliability (or shearability if there were such a word) of the medium in different substances being measured and stated in terms of that of air as their specific inductive capacity, K.

(3) That *inertia* is brought into prominence by the facts of magnetism, studied chiefly perhaps by Thomson, who has called the relative density of the medium in different substances their magnetic permeability or magnetic inductive capacity; the ratio of its value for any substance to its value for common air being called μ.

(4) That the *doubleness of constitution* of the medium —its being composed of two precisely opposite entities—is suggested by the facts of electrolysis, by the absence of mechanical momentum in currents and magnets, and by the difficulty of otherwise conceiving a medium endowed with rigidity which yet is perfectly fluid to masses of matter moving through it.

With the hypothesis of doubleness of constitution this difficulty disappears. The ether as a whole may be perfectly fluid and allow bodies to pass through it without resistance, while its two components may be elastically attached together and may resist any forces tending to separate them with any required rigidity. It is like the difference between passing one's hand through water, and chemically decomposing it; it is like the difference between waving a piece of canvas about, and tearing it into its constituent threads.

To put the matter boldly and baldly: we are familiar with the conceptions of matter and of ether, and it is known that the two things react on each other in some way, so that although matter appears to move freely through a free portion of the ether, yet another portion appears to move with matter as if bound to it. This mode of regarding the facts is as old as Fresnel. We now proceed a step further, and analyze the ether into two constituents —two equal opposite constituents—each endowed with inertia, and each connected to the other by elastic ties: ties which the presence of gross matter in general weakens and in some cases dissolves. The two constituents are called positive and negative electricity respectively, and of these two electricities we imagine the ether to be composed. The tie between them is dissolved in metals, it is relaxed or made less rigid in ordinary insulators. The specific inductive capacity of a substance means the reciprocal of the rigidity of its doubly constituted ether. Let us call this rigidity k, so that $k = \frac{1}{K}$.

The neighbourhood of gross matter seems also to render ether more *dense*. It is difficult to suppose that it can really condense an incompressible fluid, but it may load it or otherwise modify it so as to produce the effect of increased density. In iron this density reaches its highest known value, and in all substances the density or inertia per unit volume of their ether may be denoted by μ, and called their magnetic permeability.

Let it be understood what we are doing. In Part I. we discussed effects very analogous to those which would be produced by an elastic incompressible medium (roughly like india-rubber or jelly). In Parts II. and III. we discussed effects suggesting, and more or less necessitating, the idea of a property of the medium very analogous to inertia; and we were also led to postulate a doubleness of constitution for the medium, so that shearing strains may go on in it and yet it be perfectly fluid as a whole. We are now pushing these analogies and ideas into greater definiteness and baldness of statement. We already know of a continuous incompressible fluid filling all space, and we call it the ether. Let us suppose that it is composed of, and by electromotive force analyzable into, two constituents; let these constituents cling together with a certain tenacity, so that the medium shall have an electromotive elasticity, though mechanically quite fluid; and let each constituent possess inertia, or something so like inertia as to produce similar effects. Making this hypothesis, electrical effects are to a certain extent explained. Not ultimately indeed—few things can be explained ultimately—not even as ultimately as could be wished; for the nature of the connection between the two constituents of the ether and between the ether and gross matter—the nature of the force, that is, and the nature of the inertia—remains untouched. This is a limitation to be clearly admitted; but if that were the only one—if all else in the hypothesis were true—we should do well, and a distinct step would have been gained. It is hardly to be hoped that this is so—hardly to be expected that the bald statement

[1] Continued from vol. xxxvii. p. 368.

above is more than a kind of parody of the truth; nevertheless, supposing it only a parody, supposing what we call electromotive elasticity and inertia are things capable of clearer conception and more adequate statement, yet, inasmuch as they correspond to and represent a real analogy, and inasmuch as we find that a medium so constructed would behave in a very electrical manner, and might in conjunction with matter be capable of giving rise to all known electrical phenomena, we are bound to follow out the conception into other regions, and see whether any other abstruse phenomena, not commonly recognized as electrical, will not also fall into the dominion of this hypothetical substance and be equally explained by it. This is what we shall now proceed to do.

Before beginning, however, let me just say what I mean by "electromotive elasticity." It might be called chemical elasticity, or molecular elasticity. There is a well-known distinction between electromotive force and ordinary matter-moving force. The one acts upon electricity, straining or moving or, in general, "displacing" it; the other acts upon matter, displacing it. The nature of neither force can be considered known, but crudely we may say that as electricity is to matter so is electromotive force to common mechanical force: so also is electromotive elasticity to the common shape-elasticity or rigidity of ordinary matter: so perhaps, once more, may electrical inertia be to ordinary inertia.

Inertia is defined as the ratio of force to acceleration; similarly electric inertia is the ratio of electromotive force to the acceleration of electric displacement. It is quite possible that electric inertia and ordinary inertia are the same thing, just as electric energy is the same with mechanical energy. If this were known to be so, it would be a step upward towards a mechanical explanation; but it is by no means necessarily or certainly so; and, whether it be so or not, the analogy undoubtedly holds, and may be fruitfully pursued.

And as to "electromotive elasticity," one may say that pure water or gas is electromotively elastic, though mechanically limpid; each resists electric forces up to a certain limit of tenacity, beyond which it is broken; and it recoils when they are withdrawn. Glass acts in the same way, but that happens to be mechanically elastic too. Its mechanical elasticity and tenacity have, however, nothing to do with its electric elasticity and tenacity.

One perceives in a general way why fluids can be electrically, or chemically, or molecularly elastic: it is because their molecules are doubly or multiply composed, and the constituent atoms cling together, while the several molecules are free of one another. Mechanical forces deal with the molecule as a whole, and to them the substance is fluid; electrical or chemical forces deal with the constituents of the molecule, setting up between them a shearing strain and endeavouring to tear them asunder. To such forces, therefore, the fluid is elastic and tenacious up to a certain limit. Extend this view of things to the constitution of the ether, and one has at least a definite position whence to further proceed.

It may be convenient and not impertinent here to say that a student might find it a help to re-read Parts I. and II. in the light of what has just been said: remembering that, for the sake of simplicity, only the simple fact of an elastic medium was at first contemplated and insisted on; no attempt being made to devise a mechanism for its elasticity by considering it as composed of two constituents. Hence the manifest artificiality of such figures as Fig. 6 (NATURE, vol. xxxvi. p. 559), where fixed beams are introduced to serve as the support of the elastic connections. But it is pretty obvious now, and it has been said in Part III., that a closer analogy will be obtained by considering two sets of beads arranged in alternate parallel rows connected by elastic threads, and displaced simultaneously in opposite directions.

Recovery of the Medium from Strain.

We have now to consider the behaviour of a medium endowed with an elastic rigidity, k, and a density, μ, subject to displacements or strains. One obvious fact is that when the distorting force is removed the medium will spring back to its old position, overshoot it on the other side, spring back again, and thus continue oscillating till the original energy is rubbed away by viscosity or internal friction. If the viscosity is very considerable, it will not be able so to oscillate; it will then merely slide back in a dead-beat manner towards its unstrained state, taking a theoretically infinite time to get completely back, but practically restoring itself to something very near its original state in what may be quite a short time. The recovery may in fact be either a brisk recoil or a leak of any degree of slowness, according to the amount of viscosity as compared with the inertia and elasticity.

The matter is one of simple mechanics. It is a case of simple harmonic motion modified by a friction proportional to the speed. The electrical case is simpler than any mechanical one, for two reasons: first, because so long as capacity is constant (and no variation has yet been discovered) Hooke's law will be accurately obeyed—restoring force will be accurately proportional to displacement; secondly, because for all conductors which obey Ohm's law (and no true conductor is known to disobey it) the friction force is accurately proportional to the first power of velocity.

There are two, or perhaps one may say three, main cases. First, where the friction is great. In that case the recovery is of the nature of a slow leak, according to a decreasing geometrical progression or a logarithmic curve; the logarithmic decrement being independent of the inertia, and being equal to the quotient of the elasticity and the resistance coefficients.

As the resistance is made less, the recovery becomes quicker and quicker until inertia begins to prominently assert its effect and to once more lengthen out the time of final recovery by carrying the recoiling matter beyond its natural position, and so prolonging the disturbance by oscillations. The quickest recovery possible is obtained just before these oscillations begin; and it can be shown that this is when the resistance coefficient is equal to twice the geometric mean of the elasticity and the inertia. One may consider this to be the second main case.

The third principal case is when the resistance is quite small, and when the recovery is therefore distinctly oscillatory. If the viscosity were really zero, the motion would be simply harmonic for ever, unless some other mode of dissipating energy were provided; but if some such mode were provided, or if the viscosity had a finite value, then the vibrations would be simply harmonic with a dying out amplitude, the extremities of all the swings lying on a logarithmic curve. In such a case as this, the rate of swing is practically independent of friction; it depends only on elasticity and inertia; and, as is well known for simple harmonic motion, the time of a complete swing is 2π times the square root of the ratio of inertia and elasticity coefficients.

Making the statement more electrically concrete, we may consider a circuit with a certain amount of stored-up potential energy or electrical strain in it: for instance, a charged Leyden jar provided with a nearly complete discharge circuit. The main elastic coefficient here is the reciprocal of the capacity of the jar: the more capacious the jar the more "pliable" it is—the less force of recoil for a given displacement,—so that capacity is the inverse of rigidity. The main inertia coefficient is that which is known electrically as the "self-induction" of the circuit: it involves the inertia of all the displaced matter and ether, of everything which will be moved or disturbed when the jar is discharged. It is not a very simple thing to calculate its value in any given case; still it can be done, and the general idea is plain enough without understanding the exact function and importance of every portion of the surrounding space.

Corresponding, then, to the well-known simple harmonic $T = 2\pi\sqrt{\frac{m}{k}}$, we have, writing L for the self-induction or inertia of the circuit, and S for its capacity or inverse rigidity constant,

$$T = 2\pi\sqrt{LS},$$

This, therefore, is the time of a complete swing. Directly the jar is discharged, these oscillations begin, and they continue like the vibration of a tuning-fork until they are damped out of existence by viscosity and other modes of dissipation of energy.

But now just consider a tuning-fork. Suppose its substance were absolutely unviscous, would it go on vibrating for ever? In a vacuum it might: in air it certainly would not. And why not? Because it is surrounded by a medium capable of taking up vibrations and of propagating them outwards without limit. The existence of a vibrating body in a suitable medium means the carving of that medium into a succession of waves and the transmission of these waves away into space or into absorbing obstacles. It means, therefore, the conveyance away of the energy of the vibrating body, and its subsequent appearance in some other form wherever the radiating waves are quenched.

The laws of this kind of wave-propagation are well known; the rate at which waves travel through the medium depends not at all on any properties of the original vibrating body, the source of the disturbance; it depends solely on the properties of the medium. They travel at a rate precisely equal to the square root of the ratio of its elasticity to its density.

Although the speed of travel is thus fixed independently of the source, the length of the individual waves is not so independent. The length of the waves depends both on the rate at which they travel and on the rate at which the source vibrates. It is well known and immediately obvious that the length of each wave is simply equal to the product of the speed of travel into the time of one vibration.

But not every medium is able to convey every kind of vibration. It may be that the mode of vibration of a body is entirely other than that which the medium surrounding it can convey: in that case no dissipation of energy by wave-propagation can result, no radiation will be excited. The only kind of radiation which common fluids are mechanically able to transmit is well known: it is that which appeals to our ears as sound. The elasticity concerned in such disturbance as this is mere volume elasticity or incompressibility. But electrical experiments (the Cavendish experiment,[1] and Faraday's ice-pail experiment) prove the ether to be enormously —perhaps absolutely—incompressible; and if so, such vibrations as these would travel with infinite speed and not carve proper waves at all.

Conceivably (I should like to say probably) *gravitation* is transmitted by such longitudinal impulses or thrusts, and in that case it is nearly or quite instantaneous; and the rate at which it travels, if finite, can be determined by a still more accurate repetition of the Cavendish experiment than has yet been made; but true radiation transmitted by the ether cannot be of this longitudinal character. The elasticity possessed by the ether is of the nature of rigidity: it has to do with shears and distortions; not mechanical stresses, indeed—to them it is quite limpid and resistless—but electromotive stresses: it has an electrical rigidity, and it is this which must be used in the transmission of wave-motion.

But the oscillatory discharge of a Leyden jar is precisely competent to apply to the ether these electromotive vibrations: it will shake it in the mode suitable for it to transmit; and accordingly, from a discharging circuit, waves of electrical distortion, or transverse waves, will spread in all directions at a pace depending on the properties of the medium.

Thus, then, even with a circuit of perfect conductivity the continuance of the discharge would be limited, the energy would be dissipated; not by friction, indeed—there would in such a circuit be no direct production of heat—it would be dissipated by radiation, dissipated in the same way as a hot body cooling, in the same way as a vibrating tuning-fork mounted on its resonant box. The energy of the vibrating body would be transferred gradually to the medium, and would by this be conveyed out and away, its final destination being a separate question, and depending on the nature and position of the material obstacles it meets with.

Velocity of Electrical Radiation.

The pace at which these radiation-waves travel depends, as we have said, solely on the properties of the medium, solely on the relation between its elasticity and its density. The elasticity considered must be of the kind concerned in the vibrations; but the vibrations are in this case electrical, and so electrical elasticity is the pertinent kind. This kind of elasticity is the only one the ether possesses of finite value, and its value can be measured by electrostatic experiments. Not absolutely, unfortunately: only the relative elasticity of the ether as modified by the proximity of gross substances has yet been measured: its reciprocal being called their specific inductive capacity, or dielectric constant, K. The absolute value of the quantity K is at present unknown, and so a convention has arisen whereby in air it is called 1. This convention is the basis of the artificial electrostatic system of units. No one supposes, or at least no one has a right to suppose, that its value is really 1. The only rational guess at its value is one by Sir William Thomson,[1] viz. $\frac{1}{842 \cdot 8}$. Whether known or not, the absolute value of the dielectric constant is manifestly a legitimate problem which may any year be solved.

The other thing on which the speed of radiation waves depends is the medium's density—its electric density, if so it must be distinguished. Here, again, we do not know its absolute value. Its relative or apparent amount inside different substances is measured by magnetic experiments, and called their specific magnetic capacity, or permeability, and is denoted by μ.

Being unknown, another convention has arisen, quite incompatible with the other convention just mentioned, that its value in air shall be called 1. This convention is the basis of the artificial electro-magnetic system of units —volts, ohms, amperes, farads, and the like. Both of these conventions cannot be true: no one has the least right to suppose either true. The only rational guess at ethereal free density is one by Sir William Thomson, viz. $9 \cdot 36 \times 10^{-19}$.

Very well, then; it being clearly understood that these two great ethereal constants, k or $\frac{1}{K}$, and μ, are neither of them at present known, but are both of them quite knowable, and may at any time become known, it remains to express the speed of wave transmission in terms of them. But it is well known that this speed is simply the square root of the ratio of elasticity to density, or

$$v = \sqrt{\frac{k}{\mu}}, \text{ or } \frac{1}{\sqrt{(K\mu)}}.$$

This then is the speed with which waves leave the discharging Leyden jar circuit, or any other circuit conveying alternating or varying currents, and travel out into space.

Not knowing either k or μ, we cannot calculate this

[1] See Maxwell's "Electrical Researches of Cavendish," p. 104; see also p. 417.

[1] Trans. R. S. Edin., xxi. 60; see also article "Ether," in the "Encyc. Brit."

speed directly, but we can try to observe it experimentally.

The first and crudest way of making the attempt would be to arrange a secondary circuit near our oscillating primary circuit, and see how soon the disturbance reached it. For instance, we might take a nearly closed loop, make it face a Leyden jar circuit across a measured distance, and then look for any interval of time between the spark of the primary discharge and the induced spark of the secondary circuit, using a revolving mirror or what we please. But in this way we should hardly be able to detect any time at all: the propagation is too quick.

We might next make use of the principle of the electric telegraph, viz. the propagation of a disturbance round a single circuit from any one point of origin. Consider a large closed circuit, either conveying or not conveying a current: introduce at any one point a sudden change—a sudden E.M.F., for instance, or a sudden resistance if there be a current already. Out from that point a disturbance will spread into the ether, just as happens in air when a blow is struck or gun-cotton fired. A regular succession of disturbances would carve the ether into waves: a single disturbance will merely cause a pulse or shock; but the rate of transmission is the same in either case, and we may watch for the reception of the pulse at a distant station. If the station has to be very distant in order to give an appreciable lapse of time, a speaking-tube is desirable to prevent spreading out in all directions—to concentrate the disturbance at the desired spot. What a speaking-tube is to sound, that is the wire of the circuit—the telegraph wire—to ethereal pulses.

It is a curious function, this of the telegraph wire: it does not *convey* the pulses, it directs them. They are conveyed wholly by the ether, at a pace determined by the properties of the ether, modified as it may be by the neighbourhood of gross matter. Any disturbance which enters the wires is rapidly dissipated into heat, and gets no further; it is the insulating medium round it which transmits the pulses to the distant station.

All this was mentioned in Part III., and an attempt was made to explain the mechanism of the process, and to illustrate in an analogical way what is going on.

The point of the matter is that currents are not propelled by end-thrusts, like water in a pipe or air in a speaking-tube, but by lateral propulsion, as by a series of rotating wheels with their axes all at right angles to the wire surrounding it as a central core, and slipping with more or less friction at its surface. This is characteristic of ether modes in general: it does not convey longitudinal waves or end-thrust pulses, like sound, but it conveys transverse vibrations or lateral pulses, like light.

Without recapitulating further, we can perceive, then, that the transmission of the pulse round the circuit to its most distant parts depends mainly on the medium surrounding it. The process is somewhat as follows:—Consider two long straight parallel wires, freely suspended, and at some great distance joined together. At the near end of each, start equal opposite electromotive impulses, as by suddenly applying to them the poles of a battery; or apply a succession of such pulses by means of an alternating machine. Out spread the pulses into space, starting in opposite phases from the two wires, so that at a distance from the wires the opposite pulses interfere with each other, and are practically non-existent, just as but little sound is audible at a distance from the two prongs of a freely suspended tuning-fork. But near the wires, and especially between them, the disturbance may be considerable. To each wire it spreads and is dissipated, and so a fresh supply of energy goes on continually arriving at the wires, always flowing in from outside, to make up the deficiency. If the wires are long enough hardly any energy may remain by the time their distant ends are reached; but whatever there is will still be crowding in upon the wires and getting dissipated, unless by some mechanism it be diverted and utilized to effect some visible or audible or chemical change, and so to give the desired signal.

Now the pace at which this transmission of energy goes on in the direction of the wires is pretty much the same as in free space. There are various circumstances which can retard it; there are none which can accelerate it. The circumstances which can retard it are, first, constriction of the medium by too great proximity of the two conducting wires: as, for instance, if they consisted of two flat ribbons close together with a mere film of dielectric between, or if one were a small-bore tube and the other its central axis or core. In such cases as this the general body of ether takes no part in the process, the energy has all to be transmitted by the constricted portion of dielectric, and the free propagation of ethereal pulses is interfered with: the propagation is no longer a true wave-propagation at all, but approximates more or less closely to a mere diffusion creep, rapid it may be, and yet without definite velocity, like the conduction of heat or the diffusion of a salt into water. One well-known effect of this is to merge successive disturbances into one another, so that their individuality, and consequently the distinctness of signalling, is lost.

Another circumstance which can modify rate of transmission of the pulses is ethereal inertia in the substance of the conducting wires, especally extra great inertia, as, for instance, if they are made of iron. For the dissipation of energy does not go on accurately at their outer surface; it has usually to penetrate to a certain depth, and until it is dissipated the fresh influx of energy from behind does not fully occur. Now, so long as the value of μ for the substance of the wires is the same as that of air or free space, no important retardation is thus caused, unless the wires are very thick; but directly the inertia in the substance of the wires is one or two hundred times as big as that outside, it stands to reason that more time is required to get up the needful magnetic spin in its outer layers, and so the propagation of pulses is more or less retarded. At the same time this circumstance does not alter the character of the propagation, it does not change it from true wave velocity to a diffusion, it leaves its character unaltered; and so the signals, though longer in coming, may arrive quite clear, independent, and distinct. It is much the same, indeed, as if the density of the surrounding medium had been slightly increased.

These, then, are the main circumstances which affect the rate of transmission of a pulse from one part of a closed circuit to another: extra inertia or so-called magnetic susceptibility in the conducting substance, especially in its outer layers; and undue constriction or throttling of the medium through which the disturbance really has to go. Both these circumstances diminish rate of transmission, and one (the last mentioned) modifies the law and tends to obliterate individual features and to destroy distinctness.

Of course, besides these, the nature of the insulating medium will have an effect on the rate of propagation, but that is obvious all along; it is precisely the rate at which any given medium transmits pulses that we want to know, and on which we are thinking of making experiments. If we use gutta-percha (more accurately the ether inside gutta-percha) as our transmitting medium in an experiment, we are not to go and pretend that we have obtained a result for air.

The circumstances we have considered as modifying the rate of transmission are both of them adventitious circumstances, independent of the nature of the medium, and they are entirely at our own disposal. If we like to throttle our medium, or to use thick iron wires, we can do so, but there is no compulsion: and if we wish to make the experiment in the simplest manner, we shall do no such thing. We shall use thin copper wires (the thinner the better), arranged parallel to one another a fair distance apart, and we shall then observe the time which an electromotive impulse communicated at one end takes to travel to the other. Instead of using two wires, we may if we like use what comes to much the same thing, viz. a single wire suspended at a reasonable height above the ground, as in a common land telegraph. Such a case as this is much the same as if two wires were used at a distance apart equal to about twice the height above the ground.

The experiment, if it could be accurately made, would result in the observation of a speed of propagation equal to 3×10^{10} centimetres per second. The actual speed in practice may be less than this, by reason of the various circumstances mentioned, but it can never be greater. This, then, is the rate of transmission of transverse impulses, and therefore of transverse waves, through ether as free as it can be easily obtained.

There are many methods known to physicists by which an indirect experimental determination of this velocity can be made. These methods are more easily practicable than the one described: they directly determine the ratio k/μ, or, what is the same thing, the product $K\mu$, and it is left to theory to say that this is really the velocity of electrical pulses in free ether. It is unnecessary to say more about them here. OLIVER J. LODGE.

(*To be continued.*)

A HISTORY OF THE AUGUST METEORS.

THE August meteor-shower has been more frequently observed than any other with which we are acquainted, and the modern history of this remarkable system includes many interesting circumstances. It has not, in recent times, given us displays equal in grandeur to periodical swarms like the Leonids of November 13 and Andromedes of November 27, being decidedly less rich in point of numbers. But what this stream lacks in this respect is compensated for by the *annual* visibility of the shower and by the intense brilliancy of some of its individual members. Every year the August meteors present a conspicuous appearance on the night following St. Lawrence's Day, and fire-balls of excessive lustre are now and then interspersed with the smallest perceptible shooting-stars of the system. The Leonids and Andromedes, which have rendered the month of November so famous in meteoric annals, can only reappear abundantly at intervals of thirty-three and (probably) thirteen years, whereas the Perseids of August are unfailing in their regular apparitions as the epoch comes round each year. On the night of the 10th the most casual observer will not fail to notice the surprising frequency of shooting-stars, and must remark their occasional brilliancy and the persistency of the phosphorescent after-glows which they generate during their rapid flights amongst the fixed stars.

The early history of the August meteors is vague and meagre in the extreme. Ancient writings are significantly mute as to the scientific aspect of meteor-showers. Doubtless in olden times these phenomena were equally as plentiful as at present, but amid the ignorance and superstition which prevailed they were little regarded. The prominent part which meteors play in the solar system was not suspected, hence no importance was attached to their appearance. They were supposed to be mere exhalations uncontrolled by fixed laws, and it is entirely due to modern science that their true character has been revealed, and that they have been raised to the dignity of bodies having a celestial origin, and probably also an extensive influence throughout the wide range of astronomical physics.

But former records, if void of particulars possessing a scientific utility, are yet often useful in supplying *dates*. Many old references to meteor-showers, though very imperfect in description, are, by the accordance of epoch, justly assumed to have been early exhibitions of the very same systems as those which have furnished some of the most imposing displays of recent years. In the catalogue of 315 meteoric showers compiled by Quetelet, a considerable proportion are probably identical with the August Perseids, and below we give the dates, up to a century ago, of these:—

Year.	Date.	Year.	Date.
811	July 25	926	July 27–30
820	,, 25–30	933	,, 25–30
824	,, 26–28	1243	Aug. 2
830	,, 26	1451	,, 7
833	,, 27	1709	,, 8
835	,, 26	1779	,, 9 and 10
841	,, 25–30	1781	,, 8
924	,, 27–30	1784	,, 6
925	,, 27–30	1789	,, 10

The dates in the ninth and tenth centuries are somewhat different from those in later years, but this does not negative the assumed relation, because they are brought nearly into agreement when the change of style in 1752 is allowed for. This proves the showers to have really occurred at a period early in August according to present reckoning. There may also be a slight alteration in the epoch of the swarm due to a shifting of the node, which, in its cumulative amount after many ages, might reach a considerable value. For the reasons assigned, the celebrated shower of Leonids which now takes place on November 13 was observed in October 902, and again on October 19, 1202, October 22, 1366, &c.

Muschenbroek, in 1762, announced the general fact that he had observed shooting-stars to be more plentiful in August than in any other month of the year. Further towards the close of the century this was in part confirmed by the apparition of many meteors on August 8 and 9. In 1806 and 1812, Dr. Forster, of Clapton, recorded in his "Calendar" that these phenomena were unusually abundant on August 10, and in the latter year he particularly noted the extraordinary length and phosphorescent aspect of the trains left in their wake. Subsequently the same epoch was amply corroborated; and in 1835, Quetelet definitely mentioned the 9th and 10th of August as the date of maximum annual display.

On August 9, 1837, M. Wartman, of Geneva, observed 82 of these meteors between 9 p.m. and midnight. In the following year, on August 10, observations were made at Geneva and at Planchettes, a village 62 miles north-east of Geneva, with the view of determining the heights and velocities of the meteors. A discussion of the results showed that the average elevation above the ground was 550 miles, and the velocity 220 miles, but these figures are now known to have been enormously in excess of the true values.

From 20 meteors observed in August 1863, Prof. A. S. Herschel determined the mean height as 81·6 miles at first appearance and 57·7 miles at disappearance, and the velocity was found to be 34·4 miles per second. From 27 meteors similarly observed in Italy between August 5 and 10, 1864, Secchi derived limiting heights of 76·6 and 49·7 miles; and, averaging these with the results obtained by Prof. Herschel in the preceding year, we get 78 to 54 miles, which may be adopted as representative values for the normal heights not only of the Perseids, but of shooting-stars generally.

Heis, Schmidt, Greg, and Herschel were amongst the first to methodically observe the August meteor-shower and determine its radiant point in the northern region of Perseus. In 1863, August 10, an unusual display was witnessed, for on this occasion the stream seems to have attained a degree of intensity not recorded either before or subsequently to that year. In 1871 there was also a very pronounced and abundant appearance of these meteors. In NATURE, vol. xx. p. 457 (September 11, 1879), will be found some details as to the relative number of August meteors counted in different years.

But the epoch of 1866 is perhaps the most eventful and interesting of all in the history of this notable group. Signor Schiaparelli, of Milan, in the course of some observations of the Perseids, was led to take up the investigation of the theory of shooting-stars. Cautiously sifting the available materials, and forming deductions from facts indicated by the best authorities on the subject, he was induced to the belief that meteors were small particles composing cosmical clouds. These clouds were, by the action of gravitation, spread out into streams, and their orbits formed, like those of comets, elongated conic sections. From a method explained by Prof. Erman, he computed the orbital elements of the August meteors and of certain other streams, and, comparing them with the orbits of comets, discovered two remarkable coincidences between the system of Perseids and Comet III. 1862, and the Leonids and Comet I. 1866. In each case the paths of the meteor group and comet were identical, and every circumstance favoured the inference that the two phenomena were physically identical, the meteors forming the dispersed material of the comet. The period of the Leonids (November 13), viz. $33\frac{1}{4}$ years, agreed precisely with that of their supposed parent comet. The period of the August display, however, remained doubtful, the ellipse being more elongated; but Schiaparelli adopted a cycle of rather more than 100 years, as best satisfying the observations, though the exact period is still doubtful.

Computation showed that the radiant point of meteoric particles following the track of Comet III. 1862 would be seen, on August 10, at R.A. 43°, Decl. $57\frac{1}{2}$° N. In 1863, on August 10, Prof. Herschel had observed the meteors, and fixed their radiant at R.A. 44°, Decl. 56° N., a wonderfully close agreement, considering the difficulties attached to such observations. This, and other coincidences of orbit, removed all doubts as to the affinity of meteors and comets; and later evidence, especially that afforded by Biela's comet and the splendid meteor-showers of November 27, 1872 and 1885, has afforded convincing proofs as to the validity of the theory enunciated by the Italian astronomer.

Some interesting features in connection with the August meteors still, however, awaited further investigation. The visible duration of the shower was unknown. The radiant was thought to be diffused over a region extending from Perseus to Cassiopœia. Mr. R. P. Greg, in his "Table of Radiants" (*Monthly Notices*, 1872, p. 353), places it over the area from R.A. 50°–25°, Decl. 44° N., to R.A. 50°–65°, Decl. 56° N.; and Serpieri gave R.A. 50°–30°, Decl. 49°–64°. Mr. J. E. Clark, in 1874, undertook the projection of the tracks of about 2000 Perseids described in the "Luminous Meteor Reports" of the British Association, with the object of detecting motion in the radiant centre on successive days or hours of the night, but without definite success, though the observations suggested a progressive motion on succeeding nights similar to that noticed by Prof. Twining in 1859. In 1877 the shower was watched by the writer at Bristol on several nights, and the radiant was distinctly seen to take up a fresh position with every change of date. It moved from R.A. 40°, Decl. 56°, on August 5, to R.A. 60°, Decl. 59° N., on August 16. The fact was first announced in NATURE for August 30, 1877 (vol. xvi. p. 362), and many observations in subsequent years at the same station have fully confirmed the shifting of the radiant, and indicated the long duration of the shower. In the following table will be seen the position of the radiant at intervals of five days:—

July 8	3° + 49°	August 2... ...	36° + 55°
13...	11 + 50	7... ...	42 + 57
19...	19 + 51	12... ...	50 + 57
23...	25 + 52	17... ...	60 + 58
28...	31 + 54		

The whole duration extends, very probably, over the forty-five days from July 8 to August 22, and in the interval the radiant moves from 3° + 49° to 77° + 57°.

This cluster is evidently one of enormous width, and has doubtless undergone distortion by the effect of planetary perturbation. Some interesting facts in connection with this and other cometary meteor systems will be found in the *Sidereal Messenger* for April and May 1886. With regard to the August meteor-shower, it appears that a certain change in the position of the radiant ought theoretically to occur every night, but the observed displacement does not well accord with computation. On July 26 the Perseid radiant is about 4°, and on August 19 about 9°, from the radiant of its derivative comet (III. 1862); and these differences are doubtless to be referred to the disturbances exercised upon the original stream by the attraction of the earth. At every return of the group a vast number of the particles must obviously pass very near to us without being dissipated by the action of our atmosphere, and the paths of these will be affected to an extent that must alter the elements of their orbits.

Though the period of the August meteors has not yet been precisely ascertained, there is no question that the shower exhibits fluctuations from year to year as regards intensity, and that, like the two great systems of November, a certain cycle regulates its most brilliant displays. Future observations will determine the precise form of the orbit. The return of Comet III. 1862, or a recurrence of the very rich shower of August 1863, will decide the matter, but as the orbit is one of considerable eccentricity, several generations may yet elapse before the period is accurately ascertained. It is certain that many of the supposed variations in the perennial intensity of the display are more apparent than real, because the successive returns are witnessed under different conditions. Cloudy or misty weather sometimes interrupts observation; moonlight offers another impediment; occasionally, also, the maximum is attained in daylight, and passes unheeded. The same observer is not always enabled to maintain an outlook from positions equally favourable; and there are other circumstances which, with those mentioned, prove the difficulty of securing a series of observations fairly comparable with each other. Usually about 40 or 50 meteors per hour may be counted by one observer before midnight on August 10, but in the early morning hours of August 11 as many as 80 or 90, perhaps more, will be seen, as the radiant is then higher and better placed for the visible distribution of its meteors.

"The August meteors," though a general term capable of being applied to any showers observed in the month of August, is commonly employed in special reference to the Perseids of August 10. There are large numbers of minor displays visible in the same month, the radiant points of which are scattered profusely over the firmament. There are certainly more than 100 showers in contemporaneous action with the Perseids, and many of these are now pretty well known, a mass of observations having accumulated for this particular epoch.

In the present year the great August shower has not been especially brilliant, though many of its meteors have appeared under their customary aspect. At Bristol, on August 2, 42 shooting-stars were counted during the $2\frac{1}{2}$ hours between 10h. 50m. and 13h. 21m., and 14 of these were Perseids from a centre at 35° + 54°. On August 5, 31 meteors were seen in a similar interval, including 11 Perseids. On August 8, in 3 hours from 10h. to 13h., 36 meteors were observed, and among these were 20 Perseids. The radiant, both on the 2nd and 5th, seemed to be at 42° + 57°. The few subsequent nights were overcast, but on the 13th a clear sky permitted watching, and during the $3\frac{1}{2}$ hours from 10h. to 13h. 30m. 49 meteors were seen, of which 13 were Perseids from a radiant at $52° + 57°$. On August 14, between 10h. and 13h., 25 meteors were noted, but there were only two Perseids amongst them.

On August 8, Mr. Booth, at Leeds, watched the eastern sky for $4\frac{1}{2}$ hours, and saw 45 meteors, including 25 Perseids. The radiant was at about $42° + 57\frac{1}{2}°$, and it will be observed that the proportion of Perseids to the total number of meteors observed was the same as noted at Bristol on that date. On August 13, Mr. Booth recorded 13 Perseids from a radiant at $51\frac{1}{2}° + 56°$, thus confirming the displacement observed at Bristol.

On August 10, Mr. G. T. Davis, of Theale, near Reading, reports the sky was clear and many meteors were visible between 9.30 and 11 p.m., the majority being Perseids. The same observer recorded a number of paths on August 5 and 8, and a comparison of his results with similar observations at Bristol show that 7 meteors were doubly observed at the two stations. Their heights, &c., were computed by the writer as follows :—

Date. 1888. Aug.	Hour. G.M.T. h. m.	Mag.	Height at appearance Miles.	Height at disappearance Miles.	Length of real path. Miles.	Radiant point. ° °	Inclination to horizon. °
5	10 19	1–3	69	50	37	50 + 55	$27\frac{1}{2}$
5	10 30	3–4	69	48	38	39 + 57	34
5	10 42	3–4	68	48	24	43 + 51	29
8	10 6	3–5	70	59	28	66 + 56	23
8	10 10	3–4	65	52	38	319 − 13	20
8	10 21	3–3	43	28	26	40 + 60	$35\frac{1}{2}$
8	10 28	4–4	68	48	24	42 + 57	$33\frac{1}{2}$

The close agreement in the heights of these meteors (except in the case of No. 6 in the list, which was much nearer the earth's surface than usual) will be noticed. They were, with the exception of No. 6, which belonged to a radiant in Aquarius, all members of the August meteor system, though in several cases, notably that of No. 4, the path, as observed at Reading, was not exactly conformable to the radiant point of this shower.

The recent display has furnished us with a splendid fire-ball. It appeared on August 13 at 11h. 33m., and was seen by Mr. Booth at Leeds, by Mr. Monck at Dublin, by the writer at Bristol, and by several observers at Birmingham and other places. When near its disappearance the fire-ball acquired such brilliancy that it lit up the firmament like a vivid flash of lightning, and in the latter portion of its path there remained a comet-like streak which at Leeds and Birmingham continued visible for three minutes. The descriptions of this exceptionally fine meteor are in good agreement. It traversed a course above Yorkshire at normal heights ; its brilliant streak had a mean elevation of 53 miles and length of 18 miles. No detonation appears to have been heard.

W. F. DENNING.

NOTES.

IT is proposed by the Organizing Committee of Section B that in the course of the approaching meeting of the British Association there shall be a discussion in that Section upon the subject of "Valency." Prof. Armstrong will open the debate, and it is hoped that several other eminent chemists will take part. In the immediate neighbourhood of Bath there are no industries specially interesting to chemists, but arrangements are in progress by which it is hoped that members will be admitted to some of the works in and about Bristol, which is only ten miles away.

THE autumnal meeting of the Iron and Steel Institute was opened in the University, Edinburgh, on Tuesday. A hearty reception was given to the members in the Senate Hall by the Lord Provost (Sir Thomas Clark), Sir William Muir (Principal of the University), Prof. Armstrong (the honorary secretary of the Reception Committee), and other dignitaries and officials of the University. The members having adjourned to the Examination Hall of the University to begin the business of the meeting, the President, Mr. Daniel Adamson, announced that Sir James Kitson had been nominated by the Council as the President for the next two years, and he hoped that that would meet with the approval of the members. The Institute had intended, he said, to go to America for their next autumnal gathering, but the visit had been postponed until 1890, as that was considered a more suitable time, especially as a kind invitation had been given them to visit Paris next year, when the Exhibition was on. They would thus have an opportunity of entertaining their American friends. Sir Lowthian Bell took the chair while a paper on a lever-testing machine, prepared by the President, was discussed. It described in detail a horizontal compound lever-testing machine. Mr. Wickslead (Leeds), Mr. G. C. Hemming (Yale and Towne Manufacturing Company, U.S.), Mr. Brown (of Brown Brothers, Leith), M. Gautier (Paris), Mr. Nursey (London), and Sir Lowthian Bell took part in the discussion. A paper on manganese steel, by Mr. R. C. Hadfield (Sheffield), proved specially interesting, as it formed a guide to the exhibits of this metal at the Glasgow Exhibition.

THE third International Congress of Inland Navigation was opened at Frankfort-on-the-Main on Monday. It began with a speech from the President, Herr von Bötticher, Minister of State, who greeted those present in the name of the German Emperor. The Congress is divided into three sections. The first studies the improvement of river navigability, the best kind of boat for river navigation, and the best means of propulsion for boats. The second section occupies itself with the economic advantages of ship canals penetrating into the interior from river mouths, their navigability, and keeping in good order. The third deals with the reform of the statistics of interior navigation, and with the relations between agriculture and navigation.

ON Monday a paper by Dr. Gamaleia, of Odessa, on the cure of cholera by inoculation, was read to the Paris Academy of Sciences by M. Pasteur. The following information on the subject is given by the Paris Correspondent of the *Times*. It appears that in 1886 Dr. Gamaleia came to Paris as delegate of the Odessa doctors, and studied the Pasteur method, with which he made himself thoroughly acquainted. On his return to Russia various institutions were founded under his care for the cure of hydrophobia, which have proved very valuable. Five years ago M. Pasteur endeavoured to discover a means of curing cholera by inoculation. At his request a mission was sent by the French Government to Alexandria while cholera prevailed there, to study the subject. Dr. Lhuiller, one of the mission, died of cholera, and M. Pasteur did not press the continuance of the investigations. The subject, however, was taken up by Dr. Gamaleia, who has discovered a method similar to that of M. Pasteur, by which it is believed cholera can be cured by the inoculation of the cholera virus. As yet experiments have only been made on animals, but no doubt is entertained that it will be possible to apply in a short time the same process to man. After reading the paper, M. Pasteur stated that Dr. Gamaleia had expressed his readiness to repeat the experiments at Paris in presence of a committee of the Academy of Sciences, and to try on himself the inoffensive and sufficient dose for human vaccination. He is ready to undertake a journey into countries where cholera prevails to prove the efficacy of his method. M. Pasteur added that he need scarcely say that he accepted with the greatest satisfaction the offer made by Dr. Gamaleia to conduct the experiments in his laboratory. The letter was referred to the committee, which has a prize of 100,000 francs in its hands for a cure for cholera, and it was arranged that the experiments should be postponed till November.

We have already called attention (p. 359) to the address delivered by M. Janssen on July 23, at the French Academy of Sciences, on the late Jules Henri Debray. M. Debray was born at Amiens in 1827, and entered the Normal School in 1847. There he became the collaborator of the illustrious Sainte-Claire Deville, with whom his name will always be intimately associated. As M. Janssen said, it is by his researches on dissociation, in which he developed M. Deville's ideas, that M. Debray will be chiefly remembered. He succeeded M. Deville at the Paris Faculty of Sciences, and at the Normal School. M. Debray was also assayer to the "Garantie" of Paris, Vice-President of the Society for the Encouragement of National Industry, and a member of the Higher Council of Public Instruction and of the Consulting Committee of Arts and Manufactures. He was considered one of the most active and distinguished members of the Academy of Sciences. After a short illness he died on July 19.

We regret to record the death of Mr. William H. Baily, Acting Palæontologist of the Geological Survey of Ireland. He was born at Bristol in 1819. In 1844, having held for some years an appointment in the Bristol Museum, Mr. Baily was attached by the late Sir Henry de la Beche to the Geological Survey of England. He acted first as a draughtsman, and afterwards as assistant naturalist under Edward Forbes and subsequently under Prof. Huxley. In 1857, Mr. Baily was transferred to the Irish branch of the Geological Survey as Palæontologist, and this office he held until his death. He was also Demonstrator in Palæontology to the Royal College of Science, Dublin. Mr. Baily often contributed to the Proceedings of the Royal Irish Academy, of the Linnean and Geological Societies of London, of the Royal Geological Society of Dublin, and of various kindred Societies in Europe and the United States. His most important work was his "Characteristic British Fossils," which was incomplete at the time of his death.

Mr. Seth Green, whose death from paralysis of the brain is announced from New York, made a great reputation in connection with fish culture in the United States. He died at the age of seventy-one. Mr. Green was appointed in 1868 one of the Fish Commissioners of New York, and soon afterwards was made Superintendent of Fisheries in that State. He was decorated with two gold medals by the Société d'Acclimatation of Paris. Mr. Green was the author of "Trout Culture," 1870, and "Fish Hatching and Fish Catching," 1879.

Neutral chloride of platinum has been obtained in fine permanent crystals of the composition $PtCl_4 . 4H_2O$ by M. Engel (*Bulletin de la Soc. Chim.*). The universally-employed chloride of platinum is, as is well known, in reality a chloroplatinate, $PtCl_4 . 2HCl . 6H_2O$; and the neutral chloride cannot be obtained from it by merely raising its temperature, which causes it to part with a portion of its chlorine in addition to the hydrochloric acid, leaving the lower chloride, $PtCl_2$. Some time ago, however, a neutral salt was prepared by Norton, who assigned to it the formula $PtCl_4 . 5H_2O$. Norton's method of preparation consisted in the addition of silver nitrate to the ordinary commercial chloride of platinum in the proportion of two molecules of the former to one of the latter. The composition of the precipitate appears never to have been thoroughly cleared up, but the filtered liquid was found to deposit crystals of the neutral chloride. As the whole subject appeared involved in a certain amount of doubt, Engel has repeated Norton's work, and finds that the neutral chloride *is* obtained under these conditions, but that the crystals contain only four molecules of water of crystallization. The reaction, moreover, is shown to proceed in the following manner:—

$$PtCl_4 . 2HCl + 2AgNO_3 = 2AgCl + PtCl_4 + 2HNO_3.$$

The best mode of preparing the neutral chloride of platinum, according to Engel, consists in dissolving in a solution of the chloroplatinate the necessary quantity of oxide of platinum, prepared by Fremy's method, in order to neutralize the excess of hydrochloric acid. The filtered liquid, on evaporation, then deposits beautiful crystals of $PtCl_4 . 4H_2O$, permanent in the air, and not at all deliquescent like the chloroplatinate. The composition of these crystals was determined both by weighing the metallic platinum left on calcination of a known weight, and by estimation of the chlorine by fusion of the crystals with carbonate of potash and precipitation with silver nitrate. The water was, of course, given by difference. In spite of the stability of the chloroplatinate, it is a somewhat curious fact that the powdered crystals of the neutral chloride do not take up hydrochloric acid gas at ordinary temperatures. At about 50° C., however, the chloride partially liquefies under the influence of a dry current of the gas, forming the chloroplatinate. As might be expected from its non-deliquescence, the new chloride is very much less soluble in water than is the ordinary chloroplatinate.

The Portuguese Government has given notice that from August 1 meteorological signals will be established at six semaphore stations along its coast, between the River Douro and Cape St. Vincent, and shown to passing vessels requiring information as to the state of the weather in the Bay of Biscay, at Gibraltar, and at Madeira. Each notice will indicate the time to which the information refers, the locality to which it has reference, and the direction and force of the wind, together with any other particulars which the Lisbon Observatory may consider it expedient to give. The signals will usually be made by flags, of the International Code of Signals, or by semaphore, when colours of flags would not be easily distinguished. This useful information is at present only to be obtained from very few countries.

In the new number of the Journal of the Anthropological Institute there is an interesting note, by Mr. Basil Hall Chamberlain, on the Japanese "go-hei," or paper offerings to the Shinto gods. It has been thought by some European travellers that the Japanese, prompted by equal frugality and irreverence, offer paper to their gods because it is the cheapest article at hand. Mr. Chamberlain suggests a more reasonable explanation. Though paper is now used in the ceremonies of the Shinto religion, this was not so in days preceding the eighth century of the Christian era. The offerings then were made of two kinds of cloth—a white kind made of the paper-mulberry (*Broussonetia papyrifera*), and a blue kind made of hemp. Such cloth was the most precious article in the possession of a population to whom luxury and art were unknown. Later on, when Chinese civilization had brought a variety of manufactures in its train, hempen cloth ceased to be regarded as a treasure worthy of the divine acceptance; and, frugality perhaps helping, and partly also in accordance with that law of progress from the actual to the symbolical which characterizes all religions, paper began to be used instead. Mr. Chamberlain is unable to determine the date of the change, Shinto having suffered such an eclipse from the eighth to the seventeenth century that little regarding its mediæval history has been preserved. During all that time, Buddhism reigned supreme. Speaking of the general character of Shinto as a national religion, Mr. Chamberlain says that even native commentators, over anxious as they are to magnify everything Japanese at the expense of everything foreign, acknowledge that it has no moral system, no body of views of any kind save worship of the gods who were the ancestors of the Imperial House. For this reason Shinto collapsed utterly at the touch of Buddhism, and it fails to support itself now, when an attempt is being made to revive it for political purposes. It has nothing in it that appeals to the religious instincts of the people.

Messrs. George Philip and Son announce that they have made arrangements for the publication in December next of

"The Educational Annual," a handy reference volume of about 200 crown octavo pages on educational subjects, which is likely to prove a convenience to school managers, teachers, and others interested in the promotion of national education. It is proposed to review elementary education, technical education, agricultural education, industrial, reformatory, truant, and ragged schools, secondary education, and, generally, the purpose and work of the Education Department, the Science and Art Department, the training of teachers, and the teachers' organizations.

MESSRS. SONNENSCHEIN AND CO. will issue shortly a translation of Moritz Hauptmann's "Nature of Harmony and Metre." The work consists of three parts. The first part considers the evolution of harmony from acoustics, taking as basis the Hegelian theory of sound. In the second part the author discusses metre and rhythm, which are respectively analogous to harmony and melody. The last part of the book is concerned with the union of metre and harmony—that is, harmony and melody in concrete combination with metre and rhythm.

A SPECIMEN of the golden mullet (*Mugil auratus*, Risso), 320 mm. in length, has been caught at Stromstad, on the south-west coast of Sweden. Only once before has a specimen of this fish been caught on the Swedish coast.

THE authorities of the Mason Science College, Birmingham, have issued the syllabus of day classes to be held during the session 1888-89.

ACCORDING to the *American Naturalist*, the proposed site of the National Zoological Park at Washington is one of great beauty, and even grandeur. It is in the valley of Rock Creek, just beyond the city limits, and at two points walls of rock rise to a height of over 80 feet. The Rock Creek will afford what the *American Naturalist* describes as "unrivalled facilities" for the care of aquatic mammals and birds of all kinds. Nearly the whole tract is covered by a fine growth of forest trees.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented Mr. William Norman; a Lesser White-nosed Monkey (*Cercopithecus petaurista* ♂) from West Africa, presented by Mr. W. Blandford Griffith; a Tiger (*Felis tigris* ♂) from India, presented by Sir E. C. Buck, C.M.Z.S.; a Bengal Cat (*Felis bengalensis*) from India, presented by Mr. W. L. Sclater, F.Z.S.; a Black-backed Piping Crow (*Gymnorhina leuconota*), two Leadbeater's Cockatoos (*Cacatua leadbeateri*) from Australia, a Common Magpie (*Pica rustica*), four Common Herons (*Ardea cinerea*), British, two Himalayan Monauls (*Lophophorus impeyanus* ♂ ♂) from the Himalayas, two Gold Pheasants (*Thaumalea picta* ♂ ♀), two Silver Pheasants (*Euplocamus nycthemerus* ♂ ♀), two Mandarin Ducks (*Æx galericulata*) from China, a Javan Pea-fowl (*Pavo spicifer* ♂) from Java, two Common Pea-fowls (*Pavo cristatus* ♂ ♀) from India, a Rose-crested Cockatoo (*Cacatua moluccensis*) from Moluccas, a Hyacinthine Macaw (*Ara hyacinthina*), a Blue and Yellow Macaw (*Ara ararauna*) from South America, a Great Eagle Owl (*Bubo maximus*), European, presented by Mr. Charles Clifton, F.Z.S.; a Bare-eyed Cockatoo (*Cacatua gymnopis*) from North Australia, presented by Mrs. Fishlock; an Imperial Eagle (*Aquila imperalis*) from Morocco, presented by Mrs. Ernest H. Forwood; two American Box Tortoises (*Terrapene carinata*), two Alligator Terrapins (*Chelydra serpentina*), a Speckled Terrapin (*Clemmys guttata*), four Sculptured Terrapins (*Clemmys insculpta*) from North America, presented by Prof. O. C. Marsh, C.M.Z.S.; a Horned Lizard (*Phrynosoma cornutum*) from North America, presented by Master Howard Sexton; six Guinea Pigs (*Cavia porcellus*, var.), presented by Mr. R. F. Bennett; a Common Kingfisher (*Alcedo ispida*), British, deposited; a New Zealand Parrakeet (*Cyanorhamphus novæ-zealandiæ*) from New Zealand, purchased; two Chinchillas (*Chinchilla lanigera*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

COMET 1888 *c* (BROOKS).—Dr. H. Kreutz (*Astr. Nachr.*, No. 2853) has computed the following elements and ephemeris for this comet from observations made at Vienna on August 9, and at Strassburg on August 10 and 11. The middle place was represented closely.

T = 1888 July 16, 1982, Berlin M.T.

$$\left.\begin{aligned} \omega &= 34^\circ\ 36'{\cdot}90 \\ \Omega &= 94\ \ 59{\cdot}69 \\ \iota &= 71\ \ 25{\cdot}07 \end{aligned}\right\} \text{Mean Eq. 1888·0.}$$

$$\log q = 9{\cdot}92444$$

$$\begin{aligned} x &= [9{\cdot}51743]\ r \,.\, \sin (v + 229^\circ\ 57'{\cdot}12) \\ y &= [9{\cdot}99943]\ r \,.\, \sin (v + 148\ \ 23{\cdot}72) \\ z &= [9{\cdot}97573]\ r \,.\, \sin (v + \ \ 59\ \ 24{\cdot}32) \end{aligned}$$

Ephemeris for Berlin Midnight.

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Aug. 23	12 5 53	42 14·0 N.	0·0390	0·2201	0·74
25	12 19 20	41 26·1			
27	12 32 20	40 33·4	0·0568	0·2254	0·67
29	12 44 50	39 36·5			
31	12 56 49	38 36·1	0·0746	0·2326	0·60
Sept. 2	13 8 17	37 32·8			
4	13 19 16	36 27·2	0·0921	0·2413	0·53
6	13 29 45	35 19·9			
8	13 39 46	34 11·3 N.	0·1094	0·2514	0·47

The brightness on August 9 is taken as unity.

YALE COLLEGE OBSERVATORY.—The Reports of this Observatory for the last two years have recently been published. That for the year 1886-87 notes the retirement of Mr. Orray T. Sherman, who had charge of the Thermometric Bureau up to the date of his resignation in November 1886, and the renewal of subscriptions for the support of the work with the heliometer for another period of three years. Prof. Loomis had borne the expense of printing and distributing Dr. Elkin's memoir upon the Pleiades, and a second grant of 600 dollars had been made from the Bache Fund to enable Mr. Asaph Hall, Jun., the Assistant Astronomer at the Observatory, to carry on his observations of Titan for the determination of the mass of Saturn. Dr. Elkin had continued his heliometer measures for the determination of the mean parallax of the first magnitude stars; and the Report for 1887-88 records the completion of this work, and gives the results for the ten stars observed. These are as follows:—

Aldebaran	+ 0″·116 ± 0″·029	α Leonis	+ 0″·093 ± 0″·048
Capella	+ 0·107 ± 0·047	Arcturus	+ 0·018 ± 0·022
α Orionis	− 0·009 ± 0·049	α Lyræ	+ 0·034 ± 0·045
Procyon	+ 0·266 ± 0·047	α Aquilæ	+ 0·199 ± 0·047
Pollux	+ 0·068 ± 0·047	α Cygni	− 0·042 ± 0·047

The probable errors include an estimation of the probable systematic error of the measures, and are not as usual confined to the mere casual error of observation.

The results for Procyon and α Aquilæ are in close accord with those obtained by Auwers and Wagner for the first star, and by W. Struve for the second; and that for Aldebaran agrees with Prof. Asaph Hall's value; the value found by O. Struve—viz. + 0″·516—would appear, therefore, to be erroneous. But Dr. Elkin's parallax for α Lyræ is much smaller than the results which have been hitherto obtained by other observers, and which give in the mean a parallax quite five times as great as he has found. But the most remarkable result is that obtained for Arcturus, the practically insensible parallax of which seems in such strong contrast to its large proper motion. Dr. Elkin is well satisfied that the parallax of this star is extremely small, for his value depends upon eighty-nine observations and on five pairs of comparison stars, all in reasonable agreement.

The mean of the ten parallaxes gives for the mean parallax of a first magnitude star—

$$+\ 0''{\cdot}089 \pm 0''{\cdot}015,$$

a result according well with the values deduced by Gyldén (0″·084) and Peters (0″·102).

The heliometer is at present engaged on a triangulation of stars near the North Pole for Prof. Pickering, but the last three months of the present year it is to be employed in the determination of the solar parallax during the extremely favourable opposition of Iris. Measures of the diameters of the sun and of Mars, measures of certain double stars, the investigation of the parallaxes of 6 B Cygni, and of 18115/22 Lalande, are amongst the other labours of the Observatory. Mr. Hall has nearly completed the reduction of his measures of Titan.

GRAVITATION IN THE STELLAR SYSTEMS.—Prof. Asaph Hall supplies an interesting paper on "The Extension of the Law of Gravitation to Stellar Systems," in *Gould's Astronomical Journal*, No. 177, to which Dr. Elkin's new value of the parallax of Arcturus might afford a most striking illustration. Prof. Hall shows that there is a theoretical difficulty in proving the law of Newton for double stars which we cannot overcome, though the probability of the existence of this law can be increased as more double star orbits, and those very differently situated, are determined. Still, even then, before the universality of the law can be inferred, there remains the difficulty of the so-called "runaway" stars, like Groombridge 1830, stars moving through space with the speed of a comet at perihelion, and yet with no visible attracting body near them. Of these Prof. Hall supplies a list. But if Dr. Elkin's value of the parallax of Arcturus be accepted, that star would outstrip any of those given in this table. For its speed in the direction at right angles to the line of sight would be 373 miles per second, a speed compared with which its speed in the line of sight, as given by Dr. Huggins, 55 miles per second, becomes small. Prof. Hall concludes, therefore, that though Newton's law is one of the greatest generalizations of science, it is better and safer "to await further knowledge before we proceed, as Kant has done, to construct the universe according to this law."

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 AUGUST 26—SEPTEMBER 1.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 26

Sun rises, 5h. 5m.; souths, 12h. 1m. 28·9s.; sets, 18h. 57m.: right asc. on meridian, 10h. 22·1m.; decl. 10° 11′ N. Sidereal Time at Sunset, 17h. 19m.

Moon (at Last Quarter August 29, 14h.) rises, 20h. 54m.*; souths, 3h. 22m.; sets, 10h. 2m.: right asc. on meridian, 1h. 41·6m.; decl. 5° 0′ N.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	5 14 ...	12 13 ..	19 12 ...	10 33·8 ...	10 52 N.
Venus ...	6 14 ...	12 51 ...	19 28 ...	11 11·5 ...	6 42 N.
Mars ...	12 27 ...	16 48 ...	21 9 ...	15 9·6 ...	19 12 S.
Jupiter ...	13 2 ...	17 23 ...	21 44 ...	15 44·5 ...	19 8 S.
Saturn ...	3 6 ...	10 43 ...	18 20 ...	9 3·5 ...	17 33 N.
Uranus...	8 59 ...	14 35 ...	20 11 ...	12 56·1 ...	5 20 S.
Neptune..	21 56*...	5 43 ...	13 30 ...	4 2·3 ...	18 59 N.

* Indicates that the rising is that of the preceding evening.

Variable Stars.

Star.	R.A.	Decl.			
	h. m.	° ′		h. m.	
R Arietis	2 9·8 ...	24 32 N. ...	Aug. 26,		*M*
R Ceti	2 20·3 ...	0 41 S. ...	,, 27,		*M*
λ Tauri...	3 54·5 ...	12 10 N. ...	,, 27,	22 41	*m*
			,, 31,	21 33	*m*
U Monocerotis ...	7 25·5 ...	9 33 S. ...	,, 27,		*m*
R Virginis	12 32·8 ...	7 36 N. ...	,, 31,		*M*
S Boötis	14 19·1 ...	54 19 N. ...	,, 27,		*M*
δ Libræ	14 55·0 ...	8 4 S. ...	,, 30,	22 8	*m*
W Scorpii	16 5·2 ...	19 51 S. ...	,, 28,		*M*
W Ophiuchi ...	16 15·4 ...	7 26 S. ...	,, 28,		*M*
R Draconis	16 32·4 ...	67 0 N. ...	,, 30,		*M*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,, 30,	0 28	*m*
			,, 30,	20 36	*m*
W Sagittarii ...	17 57·9 ...	29 35 S. ...	,, 27,	0 0	*M*
Z Sagittarii... ...	18 14·8 ...	18 55 S. ...	,, 27,	1 0	*M*
			,, 31,	0 0	*m*
U Sagittarii... ...	18 25·3 ...	19 12 S. ...	,, 26,	0 0	*M*
β Lyræ...	18 46·0 ...	33 14 N. ...	,, 26,	1 0	m_2
R Aquilæ	19 1·0 ...	8 4 N. ...	,, 29,		*m*
X Cygni	20 39·0 ...	35 11 N. ...	Sept. 1,	3 0	*m*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Occultation of Star by the Moon (visible at Greenwich).

Aug.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
26 ...	ξ^2 Ceti	4 ...	23 20 ...	0 22† ...	98 238

† Occurs on the following morning.

Meteor-Showers.

	R.A.	Decl.	
	6° ...	11° N. ...	Swift.
Near β Trianguli	30 ...	35 N.	
,, 33 Cygni	305 ...	54 N. ...	Swift, bright. Sept. 1.
,, δ Cephei	336 ...	58 N. ...	Swift.

GEOGRAPHICAL NOTES.

THE *Times* printed on Tuesday the substance of communications received from Mr. Joseph Thomson, dated from the city of Morocco, July 22. Mr. Thomson writes in the highest spirits, and with evident satisfaction at the results he has so far attained; for much of the country through which he has had to pass is in a state of rebellion, and the local authorities have done more to hinder than to help him. Mr. Thomson sailed from Tangier to Casablanca, and thence travelled overland to Mogador. After three weeks' preparation there he made his final start, and, as he states, soon discovered that the greatest danger to his success would not be the mountaineers nor even the opposition of the Government officials, but the half-dozen men who formed the *personnel* of his small party. Mr. Thomson's past experience in Africa enabled him to deal effectively with this difficulty. By a series of surprises and cleverly-planned excursions he has been able to enter the mountain fastnesses of Morocco and do more than any previous traveller has done. From Demnat he made two extremely interesting trips into the lower ranges, visiting some remarkable caves and equally remarkable ruins, and one of the most wonderful natural bridge-aqueducts in the world. Geologically and geographically these trips are alike important. They were followed by a dart across the main axis of the Atlas to the district of Tiluit, which lies in the basin of the Draa. Here he spent a very delightful ten days, though virtually a prisoner. As the tribes further west on the southern slope were in revolt, Mr. Thomson was compelled to return to the northern plains. Starting once more, he crossed the mountains by a pass a little south of Jebel Tizah, ascended by Hooker, and reached Gindafy safely. He was able to make a trip up a wonderful cañon, which he declares rivals those of America for depth and grandeur, and ascended a mountain, where he and his party were confined to their tents until it suited them to go back to their starting-point. Here, unfortunately, Mr. Thomson's young companion, Mr. Crichton Browne, was stung by a scorpion, and they were compelled to return, happily by a new route. Though laid up for a period, fortunately in time Mr. Crichton Browne recovered. From his previous starting-point Mr. Thomson scored another great triumph. He crossed the mountains once more, and ascended with no small danger and difficulty the highest peak of the Atlas Range north of Amsiviz, a height of 12,500 feet—the highest peak, by 1500 feet, ever attained. This he describes as the most interesting of all his trips, and he enjoyed it thoroughly, though he had to sleep on the ground and was glad to make a meal on walnuts. On his return, Mr. Thomson deemed it advisable to go into the town of Morocco to recruit and wait the arrival of further supplies from the coast. He intended to resume work in a few days after the date of his letter. He proposed first to make for the Urika River and penetrate the mountains up its course. He will then work his way round to Mogador, which he expects to reach about the end of August. There probably his work of exploration will end, though he may make one or two short trips into the interior and down to Agadir. The return route to Tangier will probably be from Mogador to the city of Morocco, thence to Mazagan on the coast, and on to Casablanca and Rabat. Then he will leave the sea again and go to Mequinez and Fez, reaching Tangier about the end of the year. The *Times* understands that his contributions to various branches of science, especially to botany, will be of the highest value.

A LETTER from Cayenne to the *Temps* states that M. Coudreau, who has recently explored Guiana, arrived there last month after having travelled for eleven months in the western range of the Tumuc-Humac Mountains, between the source of the Itany and that of the Camopy. Starting by the Maroni, M. Coudreau, after having gone up the Itany and explored the region which it waters, came down to the coast by Maronimi-Crique, which is a very large tributary of the Maroni River. M. Coudreau is the first Frenchman who has passed a consecutive winter and summer in the Tumuc-Humac Mountains, and though he did not himself suffer very much from the effects of the expedition, the same cannot be said of his companions, as the only European who accompanied him was brought near to death's door by fever, from which most of the natives also suffered. M. Coudreau escaped with nothing worse than rheumatism, and he says that the climate of the Western Tumuc-Humac is not bad. The result of 1200 observations taken by him puts the mean temperature at 70°, and the country is a magnificent one; but the difficulty of reaching it is very great owing to the uncertainty of communication with the coast. M. Coudreau and his companions, when they had exhausted their provisions, had to go and live out in the open with the Indians, leading the same kind of existence, and depending for food upon the game, fish, and fruit that they could shoot, fish, and gather. For eight months M. Coudreau lived the regular native life, and he had become so accustomed to it that he was very popular with the Rucuyennes, whose language he had learned to speak, and he induced the *pamenchi* (captain) of the tribe and four of his lieutenants to accompany him to Cayenne, where their arrival created a great sensation, as the people of the town did not believe in their existence. M. Gerville-Réache, the Governor of the colony, received them with great hospitality, and made them several presents. The most important fact brought out by M. Coudreau is the existence in Upper Guiana, which is acknowledged French territory, of sixteen new Indian tribes, forming a group of at least 20,000 persons; and these Indians are not, as was supposed, mere nomads, living upon the produce of their guns and fishing-nets, but are sedentary in their habits, and have attained a certain degree of civilization. M. Coudreau is about to start on a fresh expedition to the Appruague and the Oyapack, and does not expect to get back before next spring.

THE GASES OF THE BLOOD.[1]

II.

THE next step was the discovery of the important part performed in respiration by the colouring matter of the red blood corpuscles. Chemically, these corpuscles consist of about 30 or 40 per cent. of solid matter. These solids contain only about 1 per cent. of inorganic salts, chiefly those of potash; whilst the remainder are almost entirely organic. Analysis has shown that 100 parts of dry organic matter contain of hæmoglobin, the colouring matter, no less than 90·54 per cent.: of proteid substances, 8·67; of lecithin, 0·54; and of cholesterine, 0·25. The colouring matter, hæmoglobin, was first obtained in a crystalline state by Funke in 1853, and subsequently by Lehmann. It has been analyzed by Hoppe-Seyler and Carl Schmidt, with the result of showing that it has a perfectly constant composition. Hoppe-Seyler's analysis first appeared in 1868. It is now well known to be the most complicated of organic substances, having a formula, as deduced, from the analyses I have just referred to, by Preyer (1871), of

$$C_{600}H_{960}N_{154}FeS_3O_{179}.$$

In 1862, Hoppe-Seyler noticed the remarkable spectrum produced by the absorption of light by a very dilute solution of blood. Immediately thereafter, the subject was investigated by Prof. Stokes, of Cambridge, and communicated to the Royal Society in 1864. If white light be transmitted through a thin stratum of blood, two distinct absorption bands will be seen. One of these bands next D is narrower than the other, has more sharply defined edges, and is undoubtedly blacker. "Its centre," as described by Dr. Gamgee ("Physiological Chemistry," p. 97), "corresponds with wave-length 579,[2] and it may conveniently be distinguished as the absorption band, α, in the spectrum of oxyhæmoglobin. The second of the absorption bands—that is, the one next to E—which we shall designate β, is broader, has less sharply defined edges, and is not so dark as α. Its centre corresponds approximately to wave-length 553·8. On diluting very largely with water, nearly the whole of the spectrum appears beautifully clear, except where the two absorption bands are situated. If dilution be pursued far enough, even these disappear; before they disappear they look like faint shadows obscuring the limited part of the spectrum which they occupy. The last to disappear is the band α. The two absorption bands are seen most distinctly when a stratum of 1 cm. thick of a solution containing 1 part of hæmoglobin in 1000 is examined; they are still perceptible when the solution contains only 1 part of hæmoglobin in 10,000 of water."

Suppose, on the other hand, we begin with a solution of blood in ten times its volume of water; we then find that such a solution cuts off the more refrangible part of the spectrum, leaving nothing except the red, "or, rather, those rays having a wave-length greater than about 600 millionths of a millimetre." On diluting further, the effects, as well described by Prof. Gamgee, are as follows:—"If now the blood solution be rendered much more dilute, so as to contain 8 per cent. of hæmoglobin, on examining a spectrum 1 centimetre wide the spectrum becomes distinct up to Fraunhofer's line D (wave-length 589)—that is, the red, orange, and yellow are seen, and in addition also a portion of the green, between *b* and F. Immediately beyond D, and between it and *b*, however (between wave-lengths 595 and 518), the absorption is intense."

These facts were observed by Hoppe-Seyler. Prof. Stokes made the very important contribution of observing that the spectrum was altered by the action of reducing agents. Hoppe-Seyler had observed that the colouring matter, so far as the spectrum was concerned, was unaffected by alkaline carbonates, and caustic ammonia, but was almost immediately decomposed by acids, and also slowly by caustic fixed alkalies, the coloured product of decomposition being hæmatin, the spectrum of which was known. Prof. Stokes was led to investigate the subject from its physiological interest, as may be observed on quoting his own words in the classical research already referred to. "But it seemed to me to be a point of special interest to inquire whether we could imitate the change of colour of arterial into that of venous blood, on the supposition that it arises from reduction."

He found that—

"If to a solution of proto-sulphate of iron enough tartaric acid be added to prevent precipitation by alkalies, and a small quantity of the solution, previously rendered alkaline by either ammonia or carbonate of soda, be added to a solution of blood, the colour is almost instantly changed to a much more purple-red as seen in small thicknesses, and a much darker red than before as seen in greater thickness. The change of colour which recalls the difference between arterial and venous blood is striking enough, but the change in the absorption spectrum is far more decisive. The two highly characteristic dark bands seen before are now replaced by a single band, somewhat broader and less sharply defined at its edges than either of the former, and occupying nearly the position of the bright band separating the dark bands of the original solution. The fluid is more transparent for the blue and less so for the green than it was before. If the thickness be increased till the whole of the spectrum more refrangible than the red be on the point of disappearing, the last part to remain is green, a little beyond the fixed line *b*, in the case of the original solution, and blue some way beyond F, in the case of the modified fluid."

From these observations, Prof. Stokes was led to the important conclusion that—

"The colouring matter of blood, like indigo, is capable of existing in two states of oxidation, distinguishable by a difference of colour and a fundamental difference in the action on the spectrum. It may be made to pass from the more to the less oxidized by the action of suitable reducing agents, and recovers its oxygen by absorption from the air."

To the colouring matter of the blood Prof. Stokes gave the name of cruorine, and described it in its two states of oxidation as scarlet cruorine and purple cruorine. The name hæmoglobin, given to it by Hoppe-Seyler, is generally employed. When united with oxygen it is called oxyhæmoglobin, and when in the reduced state it is termed reduced hæmoglobin, or simply hæmoglobin.

The spectroscopic evidence is, therefore, complete. Hoppe-Seyler, Hüfner, and Preyer have shown also that pure crystallized hæmoglobin absorbs and retains in combination a quantity of oxygen equal to that contained in a volume of blood holding the same amount of hæmoglobin. Thus, 1 gramme of hæmoglobin absorbs 1·56 cubic centimetre of oxygen at 0° C. and 760 milli-

[1] Address to the British Medical Association at its annual meeting at Glasgow. Delivered on August 10 in the Natural Philosophy class-room, University of Glasgow, by John Gray McKendrick, M.D., LL.D., F.R.SS.L and E., F.R.C.P.E., Professor of the Institutes of Medicine in the University of Glasgow. Continued from p. 382.

[2] Dr. Gamgee gives the measurements of the wave-lengths in millionths, not in ten-millionths of a millimetre.

metres pressure; and, as the average amount of hæmoglobin in blood is about 14 per cent., it follows that $1{\cdot}56 \times 14 = 21{\cdot}8$ cubic centimetres of oxygen would be retained by 100 cubic centimetres of blood. This agrees closely with the fact that about 20 volumes of oxygen can be obtained from 100 volumes of blood. According to Pflüger, arterial blood is saturated with oxygen to the extent of nine-tenths, while Hüfner gives the figure at fourteen-fifteenths. By shaking blood with air, its oxygen contents can be increased to the extent of from 1 to 2 volumes per cent.

These important researches, the results of which have been amply corroborated, have given an explanation of the function of the red blood corpuscles as regards respiration. The hæmoglobin of the venous blood in the pulmonary artery absorbs oxygen, becoming oxyhæmoglobin. This is carried to the tissues, where the oxygen is given up, the hæmoglobin being reduced. Thus, the colouring matter of the red blood corpuscles is constantly engaged in conveying oxygen from the lungs to the tissues. Probably the union of hæmoglobin with oxygen, and its separation from it, are examples of dissociation—that is, of a chemical decomposition or synthesis, effected entirely by physical conditions; but data regarding this important question are still wanting. If the union of oxygen with the colouring matter is an example of oxidation, it must be attended with the evolution of heat, but, so far as I know, this has not been measured. In co-operation with my friend, Mr. J. T. Bottomley, I have recently been able to detect, by means of a thermo-electric arrangement, a rise of temperature on the formation of oxyhæmoglobin. We mean to prosecute our researches in this direction. If heat were produced in considerable amount, the arterial blood returned from the lungs to the left auricle would be hotter than the blood brought to the right auricle by the veins. This, however, is not the case, as the blood on the right side of the heart is decidedly warmer than the blood on the left—a fact usually accounted for by the large influx of warm blood coming from the liver. The heat-exchanges in the lungs are of a very complicated kind. Thus, heat will be set free by the formation of oxyhæmoglobin; but, on the other hand, it will be absorbed by the escape of carbonic acid, and by the formation of aqueous vapour, and a portion will be used in heating the air of respiration. The fact that the blood in the left auricle is colder than that of the right auricle is, therefore, the result of a complicated series of heat-exchanges, not easy to follow.

Our knowledge as to the state of the carbonic acid in the blood is not so reliable. In the first place, it is certain that almost the whole of the carbonic acid which may be obtained exists in the plasma. Defibrinated blood gives up only a little more carbonic acid than the same amount of serum of the same blood. Blood serum gives up to the vacuum about 30 volumes per cent. of carbonic acid; but a small part—according to Pflüger, about 6 volumes per cent.—is given up only after adding an organic or mineral acid. This smaller part is chemically bound, just as carbonic acid is united to carbonates, from which it can be expelled only by a stronger organic or mineral acid. The ash of serum yields about one-seventh of its weight of sodium; this is chiefly united to carbonic acid to form carbonates, and a part of the carbonic acid of the blood is united to those salts. It has been ascertained, however, that defibrinated blood, or even serum containing a large number of blood corpuscles, will yield a large amount of carbonic acid, even without the addition of an acid. Thus, defibrinated blood will will yield 40 volumes per cent. of carbonic acid—that is, 34 volumes which would be also given up by the serum of the same blood (without an acid), and 6 volumes which would be yielded after the addition of an acid. Something, therefore, exists in defibrinated blood which acts like an acid in the sense of setting free the 6 volumes of carbonic acid. Possibly the vacuum may cause a partial decomposition of a portion of the hæmoglobin, and, as suggested by Hoppe-Seyler, acid substances may thus be formed.

But what is the condition of the remaining 30 volumes per cent. of carbonic acid which are obtained by the vacuum alone? A portion of this is probably simply absorbed by the serum; this part escapes in proportion to the decrease of pressure, and it may be considered to be physically absorbed. A second part of this carbonic acid must exist in chemical combination, as is indicated by the fact that blood serum takes up far more carbonic acid than is absorbed by pure water. On the other hand, this chemical combination is only a loose one, because it is readily dissolved by the vacuum. There can be no doubt that a part of this carbonic acid is loosely bound to carbonate of soda, Na_2CO_3, in the serum, probably to acid carbonate of soda, $NaHCO_3$. This compound exists only at a certain pressure. On a fall of pressure, it decomposes into sodium carbonate and carbonic acid, the latter becoming free. A third part of this carbonic acid is probably loosely bound chemically to disodium phosphate, Na_2HPO_4, a salt which also occurs in the blood serum. Fernet has shown that it binds two molecules of carbonic acid to one molecule of phosphoric acid. This salt occurs in considerable quantity only in the blood of Carnivora and Omnivora, while in that of Herbivora, such as in the ox and calf, only traces exist. It cannot be supposed in the latter instances to hold much carbonic acid in chemical combination. There must exist, therefore, other chemical substances for the attachment of the carbonic acid of the blood, and it has been suggested that a part may be connected with the albumin of the plasma.

According to Zuntz, the blood corpuscles themselves retain a part of the carbonic acid, as the total blood is able to take up far more carbonic acid out of a gaseous mixture rich in carbonic acid, or consisting of pure carbonic acid, than can be absorbed by the serum of the same quantity of blood. No compound, however, of carbonic acid with the blood corpuscles is known.

The nitrogen which is contained in the blood to the amount of from 1·8 to 2 volumes per cent., is probably simply absorbed, for even water is able to absorb to 2 volumes per cent. of this gas.

If we then regard the blood as a respiratory medium having gases in solution, we have next to consider what is known of the breathing of the tissues themselves. Spallanzani was undoubtedly the first to observe that animals of a comparatively simple type used oxygen and gave up carbonic acid. But he went further, and showed that various tissues and animal fluids, such as the blood, the skin, and portions of other organs, acted in a similar way. These observations were made before the beginning of the present century, but they appear to have attracted little or no attention until the researches of Georg Liebig on the respiration of muscle, published in 1850. He showed that fresh muscular tissue consumed oxygen and gave up carbonic acid. In 1856, Matteucci made an important advance, by observing that muscular contraction was attended by an increased consumption of oxygen, and an increased elimination of carbonic acid. Since then, Claude Bernard and Paul Bert, more especially the latter, have made numerous observations regarding this matter. Paul Bert found that muscular tissue has the greatest absorptive power. Thus we arrive at the grand conclusion that the living body is an aggregate of living particles, each of which breathes in the respiratory medium passing from the blood.

As the blood, containing oxygen united with the colouring matter (hæmoglobin), passes slowly through the capillaries, fluid matter transudes through the walls of the vessels, and bathes the surrounding tissues. The pressure or tension of the oxygen in this fluid being greater than the tension of the oxygen in the tissues themselves, in consequence of the oxygen becoming at once a part of the living protoplasmic substance, oxygen is set free from the hæmoglobin, and is appropriated by the living tissues, becoming part of their protoplasm. Whilst alive, or at all events whilst actively discharging their functions, as in the contraction of a muscle, or in those changes we term secretion in a cell, the living protoplasm undergoes rapid decompositions, leading to the formation of comparatively simple substances. Amongst these is carbonic acid. As it has been ascertained that the tension of the carbonic acid in the lymph is less than its tension in venous blood, it is difficult at first sight to account for the absorption of carbonic acid by venous blood; but its tension is higher than that of carbonic acid in arterial blood, and it must be remembered that the lymph has had the opportunity, both in the connective tissue and in the lymphatic vessels, of modifying its tension by close contact with arterial blood. Strassburg fixes the tension of the carbonic acid in the tissues as equal to 45 mm. of mercury, while that of the venous blood is only 41 mm. We may assume that as the carbonic acid is set free, it is absorbed by the blood, uniting loosely with the carbonates and phosphates of that fluid, thus converting it from the arterial into the venous condition. This constitutes respiration of tissue.

In connection with the respiration of tissue, as determined by the analysis of the blood gases and of the gases of respiration, there arises the interesting question of the ratio between the amount of oxygen absorbed and the amount of carbonic acid produced, and very striking contrasts among animals have thus been determined. Thus in Herbivora the ratio of the oxygen absorbed to the carbonic acid produced, or the respiratory quotient, as it is termed by Pflüger, $\frac{CO_2}{O}$ amounts to from 0· to 1·0, while in

Carnivora it is from 0·75 to 0·8. Omnivora, of which man may be taken as the example, come between $\frac{CO_2}{O} = 0·87$. The quotient is greater in proportion to the amount of carbohydrate in the diet, whether the animals are Carnivora, Herbivora, or Omnivora. The respiratory quotient becomes the same, about 0·75, in starving animals, a proof that the oxidations are kept up at the cost of the body itself, or, in other words, the starving animal is carnivorous. The intensity of respiration in different animals is well shown in the following table, in which the amount of oxygen used is given per kilogramme of body-weight per hour (Dr. Immanuel Munk, "Physiologie des Menschen und der Sängethiere," 1888, p. 82).

Animal.	O in grammes.	Respiratory Quotient, $\frac{CO_2}{O}$.
Cat	1·007	0·77
Dog	1·183	0·75
Rabbit	0·918	0·92
Hen	1·300	0·93
Small singing birds	11·360	0·78
Frog	0·084	0·63
Cockchafer	1·019	0·81
Man	0·417	0·78
Horse	0·563	0·97
Ox	0·552	0·98
Sheep	0·490	0·98

Smaller animals therefore have, as a rule, a greater intensity of respiration than larger ones. In small singing birds the intensity is very remarkable, and it will be seen that they require ten times as much oxygen as a hen. On the other hand, the intensity is low in cold-blooded animals. Thus a frog requires 135 times less oxygen than a small singing bird. The need of oxygen is therefore very different in different animals. Thus a guinea-pig soon dies with convulsions in a space containing a small amount of oxygen, while a frog will remain alive for many hours in a space quite free of oxygen. It is well known that fishes and aquatic animals generally require only a small amount of oxygen, and this is in consonance with the fact that sea-water contains only small quantities of this gas. Thus, according to the elaborate researches of my friend, Prof. Dittmar, on the gases of the sea-water brought home by the *Challenger* Expedition, collected in many parts of the great oceans, and from varying depths:—"The ocean can contain nowhere more than 15·6 c.c. of nitrogen, or more than 8·18 c.c. oxygen per litre; and the nitrogen will never fall below 8·55 c.c. We cannot make a similar assertion in regard to the oxygen, because its theoretical minimum of 4·30 c.c per litre is liable to further diminution by processes of life and putrefaction and processes of oxidation" (Dittmar, Proceedings of Phil. Soc. of Glasgow, vol. xvi. p. 61). As a matter of fact, a sample of water from a depth of 2875 fathoms gave only 0·6 c.c. per litre of oxygen, while one from a depth of 1500 fathoms gave 2·04 c.c. per litre. Taking 15° C. as an average temperature, one litre of sea-water would contain only 5·31 c.c. of dissolved oxygen—that is, about 0·5 c.c. in 100 c.c. Contrast this with arterial blood, which contains 20 c.c. of oxygen in 100 c.c. of blood, or there are about forty times as much oxygen in arterial blood as in sea-water. At great depths the quantity of oxygen is very much less, and yet many forms of life exist at these great depths. Fishes have been dredged from a depth of 2750 fathoms, where the amount of oxygen was probably not so much as 0·06 c.c. per 100 c.c., or 300 times less than that of arterial blood. Making allowance for the smaller quantity of oxygen in the blood of a fish than that of a mammal, it will still be evident that the blood of the fish must contain much more oxygen than exists in the same volume of sea-water. No doubt we must remember that the water is constantly renewed, and that the oxygen in it is in the state of solution, or, in other words, in a liquid state. But the question remains, where do these deep-sea creatures obtain the oxygen? Probably by a method of storage. Biot has found in the swimming-bladder of such fishes 70 volumes per cent. of pure oxygen, a gas in which a glowing splinter of wood is relit. This oxygen probably oxygenates the blood of the fish when it plunges into the dark and almost airless depths of the ocean.

Aquatic breathers, however, if they live in a medium containing little oxygen, have the advantage that they are not troubled with free carbonic acid. One of the most striking facts discovered by the *Challenger* chemists is that sea-water contains no free carbonic acid, except in some situations where the gas is given off by volcanic action from the crust of the earth forming the sea-bed. In ordinary sea-water there is no free carbonic acid, because any carbonic acid formed is at once absorbed by the excess of alkaline base present. Thus the fish breathes on the principle of Fleuss's diving apparatus, in which the carbonic acid formed is absorbed by an alkaline solution. There is nothing new under the sun. The fish obtains the oxygen from the sea-water, no doubt, by the chemical affinity of its hæmoglobin, which snatches every molecule of oxygen it may meet with, while it gets rid of its carbonic acid easily, because there is not only no tension of carbonic acid in the sea-water to prevent its escape, but there is always enough of base in the sea-water to seize hold of the carbonic acid the moment it is formed. If we could get rid of the carbonic acid of the air of expiration as easily, we could live in an atmosphere containing a much smaller percentage of oxygen.

I have now placed before you the generally accepted doctrines regarding the chemical and physical problems of respiration. But one has only to examine them closely to find that there are still many difficulties in the way of a satisfactory explanation of the function. For example, is the union of hæmoglobin with oxygen a chemical or a physical process? If oxyhæmoglobin is a chemical substance, how can the oxygen be so readily removed by means of the air-pump? On the other hand, if it is a physical combination, why is the oxygen not absorbed according to the law of pressure? It is important to note that, as a matter of fact, hæmoglobin absorbs a quantity of oxygen nearly constant for ordinary temperatures, whatever may be the amount of oxygen present in the mixture of gases to which it is exposed. This is true so long as the amount of oxygen does not fall below a certain minimum, and it clearly points to the union of the hæmoglobin with the oxygen being a chemical union. Suppose we diminish the amount of oxygen in the air breathed, the partial pressure of the gas is of course also diminished, but it is evident that we might diminish the total pressure instead of diminishing the amount of oxygen. To avoid difficulties in respiration, when one is obliged to breathe an air deficient in oxygen, we ought to increase the pressure at which the air is breathed; and, on the other hand, to avoid danger in breathing air under a low pressure, we ought theoretically to increase the richness of the air in oxygen. Thus, with a pressure of 760 mm. the air should contain, as it normally does, 21 per cent. of oxygen, while with a pressure of 340 mm. it should contain 46 per cent., and with a pressure of 250 mm. it should contain as much as 63 per cent. On this basis a pressure of 5 atmospheres should be associated with an atmosphere containing about 3 per cent. of oxygen. By increasing the pressure, we increase the quantity of oxygen by weight in a given volume.

The explanation is that in all of these cases the partial pressure of the oxygen is nearly the same—that is, not far from 157 mm. of mercury, and the general law is that for all kinds of breathing the pressure of the oxygen should be nearly that of the oxygen in ordinary atmospheric air. Whilst the absorption of oxygen by the hæmoglobin has nothing directly to do with the pressure, it is striking that any atmosphere contains enough oxygen by weight for the hæmoglobin in the blood, when the partial pressure of the oxygen is near 157 mm. On each side of this median line life can be supported with considerable differences of pressure. Thus the pressure may be gradually reduced until the point of the dissociation of oxyhæmoglobin is reached—that is to say, down to about $\frac{1}{10}$ of an atmosphere. On the other hand, animals may breathe an atmosphere containing two or three times the normal amount of oxygen without appearing to be affected. This was first noticed by Regnault and Reiset, and the observation has been much extended by Paul Bert. The latter distinguished physiologist found that an increase even up to 8 or 10 atmospheres did not produce any apparent effect, but on reaching the enormous pressure of 20 atmospheres, death, with severe tetanic convulsions, was the result. He also showed that the additional increment of oxygen absorbed by the blood under the influence of each atmosphere of added pressure was very small. Thus, with a pressure of 1 atmosphere the amount of oxygen absorbed by the blood was about 20 per cent. by volume, a pressure of 2 atmospheres caused an increase of only 0·9 per cent., of 3 atmospheres 0·7 per cent., of 4 atmospheres 0·6 per cent., of 5 atmospheres 0·5 per cent., of 6 atmospheres 0·2 per cent., of 7 atmospheres 0·2 per cent., of 8 atmospheres 0·1 per cent., of 9 atmospheres 0·1 per cent., and of 10 atmospheres 0·1 per cent. Thus from 1 atmosphere to 10 atmospheres the increase was only to the extent of 3·4 per cent., so that the blood now contained 23·4 per cent. by volume instead of 20 per cent. These facts indicate that when all the hæmoglobin has been satisfied with oxygen it becomes indifferent, within limits, to any additional oxygen that may be forced into the blood under pressure, and thus the blood of animals breathing an atmosphere richer in oxygen than ordinary air is not more highly oxygenated than normal blood. The practical result also follows that it is of no use in the treatment of disease to cause patients to breathe an atmosphere richer in oxygen than ordinary air, because, at ordinary atmospheric pressure, no more oxygen can thus be caused to enter the blood, and if it be desirable to hyperoxygenate the blood, this can only be done by breathing oxygen, under a pressure of three or four atmospheres, in a chamber in which the body of the patient is subjected to the same pressure.

In this connection it is important to notice the enormous absorptive surface for oxygen presented by the red blood corpuscles of man. There are about 5,000,000 red corpuscles in each cubic millimetre. Each corpuscle has a superficial area of 0·000128 square millimetre. Taking the blood in the body of a man of average size at 4·5 litres, that is 4,500,000 cubic millimetres, the number of corpuscles is about 22,500,000,000,000, and this would give a superficial area of 2,880,000,000 square millimetres, or 2880 square metres, or about 3151 square yards—that is to say, the absorptive area of the blood corpuscles is equal to that of a square having each side about 56 yards. The hæmoglobin in a red blood corpuscle amounts to about $\frac{12}{13}$ of its weight. The blood of a man of average size may be taken at 4536 grammes, or about 10 pounds. Such blood contains about 13·083 per cent. of hæmoglobin, and 4536 grammes will contain about 593 grammes of hæmoglobin, or about $1\frac{1}{3}$ pound. As regards the iron, which is supposed to be an essential constituent of hæmoglobin, 100 grammes of blood contain 0·0546 gramme. It follows that the total amount, 4536 grammes, contain about 2·48 grammes, or nearly 39 grains. Twenty-five minims of the tinctura ferri perchloridi contain about 1 grain of pure iron, so it will be seen that not many doses are required to introduce into the body an amount of iron as large as exists in the whole of the blood.

The absorption of oxygen, therefore, probably takes place as follows: the inspired air is separated in the alveoli of the lung by delicate epithelial cells and the endothelial wall of the pulmonary capillaries from the blood which circulates in the latter. The exchange of gas takes place through these thin porous membranes, so that the velocity of the transit must be practically instantaneous. As the oxygen is bound loosely to the hæmoglobin of the corpuscles, the laws of diffusion can have only a secondary influence on its passage, and only so far as it has to pass into the plasma so as to reach the blood-corpuscles. The plasma will absorb, at 35° C., about 2 volumes per cent., if we take the coefficient absorption of the plasma as equal to that of distilled water. Many of the blood corpuscles of the pulmonary blood have just returned from the tissues with their hæmoglobin in the reduced state, and the latter at once withdraws oxygen from the plasma. In an instant more oxygen passes out of the pulmonary air into the plasma, from which the oxygen is again quickly withdrawn by the hæmoglobin of the corpuscles, and so on. It is interesting to note that, if the oxygen did not exist in loose chemical combination, it would only be absorbed, and its amount would depend on the barometrical pressure at the moment, and would follow each fluctuation of pressure through a range, say, of one-fourteenth of the total pressure. Such an arrangement could not fail in affecting health. If, on ascending a high mountain, say 15,000 to 20,000 feet above the level of the sea, the pressure sank to nearly one-half, the blood would then contain only half its normal quantity of oxygen, and disturbances in the functions of the body would be inevitable. High-flying birds, soaring in regions of the air where the pressure falls below half an atmosphere, would suffer from want of oxygen; but in deep mines and on high mountains men and animals live in a state of health, and the quick-breathing bird has a sufficient amount of oxygen for its marvellous expenditure of energy, because the amount of oxygen in the blood is independent of the factor which exercises an immediate influence on the gas contents of the fluid—namely, the partial pressure. Kempner has also proved that so soon as the amount of oxygen in the respiratory air sinks only a few per cent. below the normal, the consumption of oxygen by the tissues and the formation of carbonic acid also fall in consequence of the processes of oxidation in the body becoming less active.

It is a remarkable fact that, in certain circumstances, tissues and even organs may continue their functions with little or no oxygen. Thus, as quoted, Max Marckwald, in his work on the "Innervation of Respiration in the Rabbit" (translated by T. A. Haig, with introduction by Dr. McKendrick; Blackie and Son, 1888): "Kronecker and MacGuire found that the heart of the frog pulsates just as powerfully with blood deprived of its gases as with that containing oxygen, while the blood of asphyxia, or blood containing reduced hæmoglobin, soon stops its action."

Further, Kronecker has found that dogs bear the substitution of two-thirds to even three-fourths of their blood by 0·6 per cent. solution of common salt, and Von Ott withdrew 14/15 of the blood of a dog, and replaced the same with serum from the horse, free from corpuscles. For the first day or two after the transfusion the dog had only 1/55 part of the normal number of red blood corpuscles, so that it had only 1/55 part of its normal amount of oxygen. But this dog showed no symptoms except weakness and somnolency, nor did it suffer from distress of breathing, a remarkable fact when we consider that the blood of an asphyxiated dog still contains 3 per cent. of oxygen, and that it may show great distress of breathing when there is still one-sixth part of the normal amount of oxygen in its blood.

The conditions regulating the exchange of carbonic acid are quite different. We have seen that the carbonic acid is almost exclusively contained in the blood plasma, the smaller part being simply absorbed, and the greater part chemically bound, a portion existing in a fairly firm combination with a sodic carbonate of the plasma, and another portion in a loose, easily decomposable combination with the acid sodium carbonate, and a third portion with the sodium phosphate. Carbonic acid is contained in air only in traces, and its tension in the air is almost nothing. The air contained in the lungs is not wholly expelled by each respiration, but a part of the air of expiration, rich in carbonic acid, always remains in the lung. It is evident, then, that by the mixing of the air of inspiration with the air in the alveoli, the latter will become richer in oxygen and poorer in carbonic acid. The air in the alveoli, however, will always contain more carbonic acid than atmospheric air. Pflüger and Wolffberg have found the amount of carbonic acid in alveolar air to be about 3·5 volumes per cent., therefore its tension will be $\frac{3·5 \times 760}{100} = 27$ mm. of mercury. The tension of the carbonic acid in the blood of the right ventricle (which may be taken as representing venous pulmonary blood) amounts, according to Strassburg, to 5·4 per cent. = 41 mm. of mercury, and is 14 mm. higher than that in the alveoli. Carbonic acid will, therefore, pass by diffusion from the blood into the alveolar air until the tension of the carbonic acid has become the same in the blood and in alveolar air. Before the state of equilibrium is reached, expiration begins and removes a part of the air out of the alveoli, so that the tension of the carbonic acid again becomes less than that in the blood. During the expiration and the following pause, the elimination of carbonic acid continues. This physical arrangement has the advantage for diffusion, that by expiration the whole air is not driven out of the lungs, for, if expiration had emptied the lungs of air, the diffusion would have ceased altogether during expiration and the following pause, and diffusion have been possible only during inspiration. There would thus have been an incomplete separation of the carbonic acid from the pulmonary blood. But as air remains in the lungs, the stream of diffusion between pulmonary blood and pulmonary air goes on steadily, and fluctuations occur only in regard to its velocity (Munk).

Any account of the gaseous constituents of the blood would be incomplete without a reference to the ingenious theory recently advanced by Prof. Ernst Fleischl von Marxow, of Vienna, and explained and illustrated in his work "Die Bedeutung des Herzschlages für die Athmung; Eine Neue Theorie des Respiration," a work distinguished alike by the power of applying a profound knowledge of physics to physiological problems, and by a keen and subtle dialectic. The author starts with the antagonistic statements that of all animal substances, hæmoglobin is the one which possesses the greatest affinity for oxygen, *or* that substances exist in the animal body which, at least occasionally, have a greater chemical affinity for oxygen than hæmoglobin possesses. If the tissues have a greater affinity for oxygen than hæmoglobin has, how is it that in the blood of animals that have died of asphyxia there is still a considerable quantity, in some cases as much as 5 volumes per 100 volumes, of oxygen? It is well known that the blood of such animals invariably shows the spectrum of oxyhæmoglobin. The tissues, then, do not use up all the oxygen of the oxyhæmoglobin, and they cannot, therefore, have a stronger affinity for the oxygen than hæmoglobin has. On the other hand, as the tissues undoubtedly seize hold of the oxygen, and rob the hæmoglobin of it, it would appear as if they really had a stronger affinity for the oxygen. There is thus a contradiction according to Fleischl von Marxow, and it shows that our theories as to the ultimate chemical changes of respiration are not valid.

It might be objected at this point that the death of an animal from asphyxia, while oxygen still remains in its blood, is no proof that the tissues have lost their power of removing oxygen from oxyhæmoglobin. It only indicates that certain tissues, probably those of the nervous centres, require more oxygen than is supplied to them; and, therefore, this part of the bodily mechanism is arrested, with the result of somatic death. Other tissues still live, and use up oxygen so long as their vitality lasts. At the same time, I am willing to admit that it is a striking circumstance that the nervous tissues stop working before they have exhausted every atom of oxygen in the blood.

But if tissues have, as all admit, an affinity for oxygen, and if, at the same time we grant, for the sake of argument, that this affinity is not strong enough to dissociate the oxygen from the oxyhæmoglobin, can we perceive any physical action which would, in the first place, perform the work of dissociation, and then present the oxygen to the tissues in a form in which they would readily take it up? Ernst Fleischl von Marxow holds that he has discovered such an action or agency in the stroke of the heart. He founds his theory on some remarkable experiments, which may be readily repeated with an ordinary tight-fitting hypodermic syringe. (1) Immerse the syringe wholly in water, so as to exclude air. Place one finger over the nozzle, draw up the piston for about half the length of the syringe, and then suddenly remove the finger from the nozzle. The water will rush in, and gas will be given off in considerable amount, the water being quite frothy for a short time. This is what one would expect. (2) Then carefully empty the syringe of air and gently draw it half full of water; then place the finger on the nozzle and draw the piston up a little, so as to leave a vacuum above the water. In these circumstances a few large bubbles of gas will come off, but the water will not froth. (3) Empty the syringe thoroughly, fill it half full of water, raise it obliquely so that the knob at the end of the handle of the piston is above the water, strike the knob sharply with a piece of wood, using the latter as a mallet; then draw the piston up a little, so as to leave a vacuum above the fluid. You will now observe that so large an amount of gas is given off as to cause the fluid to froth. In this experiment, the percussion stroke has evidently altered the mode in which the gas escapes when a vacuum has been formed above it. These experiments may also be done by using a long barometer tube, with a stop-cock at one end, and an india-rubber tube communicating with a movable mercury cistern (a bulb) at the other. By lowering and depressing the bulb, a Torricellian vacuum may be formed, and water may be admitted, as with the syringe. Of the effects of percussion, in these circumstances, there can be no doubt, and the experiments are extremely interesting from the physical point of view. Fleischl von Marxow holds that when gases are dissolved in fluids the condition is analogous to the solution of crystalloids. If a fluid containing gas is shaken, more especially by a sudden sharp stroke, the close connection between the molecules of the fluid and of the gas is rent asunder, and the gas molecules lie outside and between the molecules of fluid. A shock, therefore, converts a real solution into a solution in which the fluid and gaseous molecules are in juxtaposition; and, if a vacuum is formed soon after the stroke, small bubbles of gas make their appearance more readily than if a stroke had not been given.

He then applies this theory to the phenomena of the circulation and of respiration. Starting with the query why the stroke of the heart should be so sudden and violent, when a much slower and more prolonged rhythmic movement would have been sufficient to keep up the tension in the arterial system on which the movement of the fluid depends, he boldly advances the opinion that it serves for the separation of the gases. The blood is kept in motion by a series of quick, sudden strokes, because, for the taking up of the oxygen by the tissues, and the elimination of carbonic acid by the lungs, it is not sufficient that the blood runs steadily through the systemic and pulmonary circulations; and, therefore, a short, hard stroke is given to it immediately before it enters the lungs and immediately after it has left the lungs. These strokes liberate the gases from a state of solution, and they become mixed with the fluid in a state of fine dispersion. This condition of fine dispersion is favourable for the elimination of the carbonic acid by the lungs, and for the using up of oxygen by the tissues.

Fleischl von Marxow then proceeds to state that loose chemical combinations may also be dissolved by shocks, the gas passing into a condition of fine molecular dispersion, and that a quick repetition of the shocks prevents a recombination. As examples of such loose combinations, he cites oxyhæmoglobin and the compounds of carbonic acid with the salts of the plasma. It is here, in my opinion, that the theory fails, from want of experimental evidence. There is no proof that shocks, such as those of the contraction of the right and left ventricles, can liberate gases from loose chemical combinations such as those with which we have to deal, and it is somewhat strained to point to the explosion of certain compounds excited by strong mechanical shocks or by vibratory impulses.

Some of the applications of the theory are very striking. For example, Fleischl von Marxow suggests that asphyxia occurs before the oxygen has disappeared from the blood, because it is held by the hæmoglobin so firmly that the tissues cannot obtain it. Thus suppose no oxygen is admitted by respiration. It is well known that all the blood in the body passes through the heart and lungs in the time of one complete circulation—that is, in about twenty seconds; and we have it on the authority of Pflüger that in this time one-third of the oxygen is used up by the tissues. According to the percussion theory, the stroke of the left ventricle arterializes the blood—that is, liberates the oxygen from the hæmoglobin—and this arterialized blood is carried to the tissues. The hæmoglobin does not get sufficient time to recombine with the oxygen, because of the successive strokes of the heart and the vibrating thrill kept up in the arterial ramifications. The free oxygen is used up by the tissues in the capillary circulation, to the extent of one-third. After leaving the capillaries, the two-thirds of oxygen again recombine with the hæmoglobin, and in this condition return to the heart, along with one-third of hæmoglobin that has lost its oxygen. In ordinary circumstances this one-third would again obtain oxygen from the alveoli of the lungs; but if all the oxygen there has been used up, of course it cannot obtain any oxygen. The blood flows from the lungs to the left ventricle, when it is again arterialized, and again sent out through the arteries; but as there is now a large amount of free hæmoglobin present in the capillary circulation, it will seize hold of a part of the oxygen, and the tissues will obtain less than the usual supply. With each successive circulation, the amount of oxygen available for the tissues will become less and less, until the tissues receive none, because all the oxygen set free by each beat of the left ventricle is seized hold of in the capillary circulation by the reduced hæmoglobin. The tissues die from want of oxygen, because there is too much reduced hæmoglobin present, a substance having a greater affinity for oxygen than the tissues possess, a result that would probably occur, as in drowning, in the time of six or eight complete circulations—that is, in three or four minutes.

Time will not allow me to refer further to this ingenious theory, which still requires the proof that such shocks as those of the heart can liberate gases from the compounds that exist in the blood. In my opinion, Fleischel von Marxow exaggerates the importance of the shock, while he under-estimates the evidence of the spectroscope, which always shows the spectrum of oxyhæmoglobin even in arterial blood drawn from the neighbourhood of the heart, and kept from contact with the air. Nor can I accept his statement that the force of the stroke of the heart is practically the same in all classes of warm-blooded animals, and one can hardly imagine the feeble stroke of the left ventricle of a mouse would be sufficient to liberate the oxygen from the oxyhæmoglobin of its blood. Further, it may be urged that the conditions of the experiments with the syringe are very unlike those of the circulation, more especially in the fact that the walls of the syringe are rigid, while those of the heart and vessels are yielding and elastic. Again, when an organ is supplied with a solution of oxyhæmoglobin from a pressure bottle, by a process of transfusion, the tissues will reduce the oxyhæmoglobin, and take up the oxygen without any kind of percussion action being brought into play.

Physiologists, however, cannot but treat with the greatest respect the experiments and reasoning of a physicist so able as Fleischel von Marxow is known to be, and the theory will be thoroughly tested in every detail. I may be allowed to contribute an expression of deep interest in this brilliant speculation, and to say that I entirely agree with its author in accepting the suggestions of teleology in the investigations of such problems.

While the rigid investigation of facts is no doubt one of the great methods of science, we must not forget that by asking questions as to the use or value of a particular physiological arrangement, we may obtain light as to the road along which investigations are to be pursued. This is the guiding star of Fleischl von Marxow's speculation, and it has led him and other physiologists to scrutinize anew the theories of respiration now in vogue.

In this address we have had abundant evidence of the fact that physiology, in the solution of some of her problems, depends entirely upon the methods of chemistry and physics. The air-pump, the special advantages of the mercurial air-pump, the methods devised for collecting and analyzing the gases of the blood, the spectroscope, have all contributed important facts to our knowledge of respiration. The narrative placed before you also illustrates in a striking manner the relation of modern physiology to the physiology of our forefathers. The latter were engaged in observing and explaining the more obvious phenomena, whilst the modern physiologists are pushing their researches further, and are endeavouring to study the hidden phenomena, which, like a second order, lie behind these. I need scarcely add that even the results of modern research are not to be regarded as final. Although we see a little further and more clearly than those who went before, there is still uncertainty as to fact and obscurity as to explanation in most departments of physiological science, and not least as regards the function of respiration. Enough has been said to show that in the study of respiratory mechanisms we meet with numerous examples of the same wonderful adaptation of organic structure to physical conditions as may be traced in the mechanism of the eye and of the ear. The structure of a lung or of a gill is just as much adapted for the play of the physical laws regulating gases as the retina is tuned to the vibrations of the ether, or as the organ of Corti responds sympathetically to the waves of musical tone.

List of Experiments in illustration of the Lecture.

1. Appearance of blood after having been shaken with carbonic acid.
2. Appearance of blood after having been shaken with hydrogen.
3. Appearance of blood after having been shaken with nitrogen.
4. Appearance of blood after having been shaken with oxygen.
5. *Fac-simile* model of Leeuwenhoek's syringe, by which gases were first demonstrated in the blood.
6. Absorption of ammonia by water.
7. Gases escaping from water in Torricellian vacuum.
8. Gases escaping from blood in Torricellian vacuum.
9. Spectrum of oxyhæmoglobin shown by electric light.
10. Spectrum of reduced hæmoglobin; the reduction effected by ammonium sulphide.
11. Spectrum of oxyhæmoglobin changing into that of reduced hæmoglobin by heating blood *in vacuo*.
12. Demonstration of a new gas-pump for the physiological lecture table (Figs. 1, 2, and 3).
13. Demonstration of the use of Pflüger's gas-pump.
14. Collection of blood-gases and demonstration of the existence of carbonic acid and of oxygen.
15. Carbonic acid collected from a solution of carbonate of soda *in vacuo*.
16. Method, by use of thermo-electric piles with galvanometer, of observing thermal changes attending formation of oxyhæmoglobin.
17. Demonstration of Fleischl von Marxow's experiments, not with a syringe, but with the fluid in a Torricellian vacuum so arranged as to receive a shock.

Dr. McKendrick asks us to direct the attention of our readers to a statement in his address which he wishes to correct. He stated: "If the union of oxygen with the colouring matter is an example of oxidation, it must be attended with the evolution of heat, but, so far as I know, this has not been measured." He then referred to a method by which Mr. J. T. Bottomley and he had been able to observe the heat produced. Dr. McKendrick was not then aware of an important research on this subject conducted in 1871 by his friend Dr. Arthur Gamgee, and contained in a Report to the British Association for the Advancement of Science in 1871. Dr. Gamgee, both by the use of thermometers and by thermo-electric arrangements, demonstrated the important fact that an evolution of heat accompanies the union of oxygen with hæmoglobin, and in the Report referred to there is ample evidence that the research was conducted with great skill and with an appreciation of the difficulties to be surmounted. He arrived at the conclusion "that the mean rise of temperature during the absorption of oxygen amounted to 0°·0976 C. The maximum heating found was 0°·111 C., and the minimum 0°·083 C."

MOLECULAR PHYSICS: AN ATTEMPT AT A COMPREHENSIVE DYNAMICAL TREATMENT OF PHYSICAL AND CHEMICAL FORCES.[1]

I.

THE author states that his attention was drawn to the subject in the first place by personal intercourse with Sir William Thomson, and by his opening address to the Mathematical and Physical Section of the British Association at the Montreal meeting in 1884, followed by the study of the lithographed report of his lectures on "Molecular Dynamics" at the Johns Hopkins University.

The opening paragraph of the paper contains a restatement of the portions of Thomson's theory applicable to the explanation of optical phenomena. Thomson did not succeed in arriving at a satisfactory explanation of the fact that metallic reflection and double refraction are accompanied by little or no dispersion. The author believes that he has overcome this difficulty by a more complete discussion of the formulæ by expansion in series. He then proceeds to apply the theory to the explanation of chemical phenomena on a purely dynamical basis, and arrives at a method of determining the spectrum of a compound from the spectra of its constituents.

The second portion of the paper is quite independent of the first, and also of Thomson's theories, except that it gives a complete explanation of the manner in which the ether vibrations can be taken up by the molecules of a body.

The author endeavours to explain electrical phenomena by transverse vibrations of the ether, which are very small compared to the diameter of a molecule or of an atom, and one of the most remarkable and interesting results of his investigation is that the theory leads to Weber's law expressing the mutual action of two electric currents, subject to a restriction which excludes exactly those cases the consideration of which led Helmholtz to the conclusion that the law was untenable. A further confirmation of the theory is given by its explanation of a number of other phenomena, such as fluorescence, magnetism, and diamagnetism, and the electro-magnetic rotation of the plane of polarization.

PART I.—LIGHT, HEAT, AND CHEMICAL AFFINITY.

§ 1.—*The Internal Structure of Molecules.*[2]

The ether is assumed to fill the whole of space, and to be everywhere of equal elasticity and density. It is further assumed that, with respect to vibrations of periods comparable with those of light-waves, the ether behaves like a perfectly elastic solid; while with respect to slower vibrations, such as those due to the motion of gaseous molecules, it behaves like a perfect fluid, so that the molecules can traverse it freely.

A molecule is supposed, on Thomson's[3] theory, to consist of a solid core inclosed within a series of spherical shells. Between the core and the innermost shell there is supposed to be an elastic action of a nature which might be represented by a series of symmetrically disposed elastic springs.

A similar elastic action is supposed to take place between every pair of adjacent shells, and also between the outermost shell and the external ether.

Let j be the number of shells in a molecule, and let their masses, beginning with the outermost one, be

$$\frac{M_1}{4\pi^2}, \frac{M_2}{4\pi^2}, \ldots \frac{M_j}{4\pi^2}.$$

The centres of the core and shells may be supposed to lie in a straight line and to be capable of oscillations along this line. The elastic force between each pair of shells is assumed to be proportional to the relative displacement of their centres; and that between the outermost shell and the external ether, proportional

[1] A Paper read before the Physico-Economic Society of Königsberg, by Prof. F. Lindemann, on April 5, 1888.

[2] The author generally uses the term molecule to denote either an atom or a molecule except when he is considering chemical compounds.—G. W. T.

[3] "Lectures on Molecular Dynamics and the Wave Theory of Light," by Sir William Thomson. (Baltimore, 1884.)

to the displacement of the centre relatively to the external ether. Let $x_1, x_2, \ldots x_j$, be the absolute displacement of the j shells, and ξ the displacement of the ether; and let $c_1, c_2, \ldots c_j$, be the magnitudes of the elastic forces. We then have the following equations:—

$$\frac{M_1}{4\pi^2}\frac{d^2x_1}{dt^2} = c_1(\xi - x_1) - c_2(x_1 - x_2), \quad \ldots (1)$$
$$\frac{M_2}{4\pi^2}\frac{d^2x_2}{dt^2} = c_2(x_1 - x_2) - c_3(x_2 - x_3),$$
$$\ldots\ldots\ldots\ldots\ldots$$

Let the point ξ have a periodic motion given by

$$\xi = a\cos\frac{2\pi t}{T} \quad \ldots\ldots\ldots (2)$$

Then this motion will gradually be communicated to the centres of the shells in a manner which has been fully worked out by Thomson. The value of T will vary, and after a certain interval a steady condition will be arrived at in which all the points will have periodic motions, so that

$$x_i = a_i\cos\frac{2\pi t}{T} \quad \ldots\ldots\ldots (3)$$

where T is now arbitrary.

Writing $a_1 = M_i/T^2 - c_i - c_{i+1}$, equations (1) give

$$-\frac{c_1\xi}{x_1} = a_1 - \cfrac{c_2^2}{a_2 - \cfrac{c_3^2}{a_3 - \cfrac{\ddots}{\quad - \cfrac{c_{j-1}^2}{a_{j-1} - \cfrac{c_j^2}{a_j}}}}},$$

which may be written in the form—

$$-\frac{x_1}{c_1\xi} = \frac{T^2}{m_1}\left\{\frac{K_1^2R_1}{K_1^2 - T^2} + \frac{K_2^2R_2}{K_2^2 - T^2} + \ldots + \frac{K_j^2R_j}{K_j^2 - T^2}\right\} \quad . (4)$$

The constant R_1 represents the ratio of the energy of the shell m_i to the total energy of the system. The quantity K_i is determined by the condition that when $T = K_i$ the ether remains at rest, or $\xi = 0$; and it may be called a critical period of the molecule, which will accordingly have j critical periods, and the molecule may undergo vibrations corresponding to any or all of them simultaneously without affecting the external ether.

Instead of this somewhat artificial structure, the molecule may be regarded as consisting of a sphere filled with continuous matter of density varying with the radius, the density having different values for each of j assigned values of the radius, but though this would be a simpler physical representation, it would lead to great difficulties in the mathematical treatment, though the results would necessarily be of a similar nature to those obtained for the discrete molecule, and it is therefore preferable to retain this representation.

To apply the theory to transparent media let $M_i/4\pi^2$ represent the thickness instead of the mass of a shell, and let $\rho/4\pi^2$ and $l/4\pi^2$ be the density and elasticity respectively of the ether.

The vibrations of the ether will then be given by the equation

$$\rho\frac{d^2\xi}{dt^2} = l\frac{d^2\xi}{dx^2} \quad \ldots\ldots\ldots (5)$$

And the vibrations of the outermost shell will, in virtue of the assumptions which have been made, be connected with those of the neighbouring ether particle ξ by an equation of the form

$$\rho\frac{d^2\xi}{dt^2} = l\frac{d^2\xi}{dx^2} + c_1(x_1 - \xi)4\pi^2 \quad \ldots\ldots (6)$$

in which c_1 only differs from its former value by an unimportant factor. The axis of x is here supposed to be perpendicular to the line of centres, or diameter, of the molecule.

Suppose a light-wave in a direction perpendicular to this axis, and given by the equation

$$\xi = a\sin 2\pi\left(\frac{x}{\lambda} - \frac{t}{T}\right) \quad \ldots\ldots (7)$$

to strike the molecule; then on the assumption that within a definite interval only one wave strikes the molecule, or that the diameter of the molecule is small in comparison with the wave-length, where μ is the index of refraction of the medium, and v the velocity of the wave in it, equation (6) gives the equation

$$\mu^2 = \frac{1}{v^2} = \frac{T^2}{\lambda^2} = \frac{1}{l}\left\{\rho - c_1T^2\left(l - \frac{x_1}{\xi}\right)\right\}$$
$$= \frac{1}{l}\left\{\rho - c_1T^2\left[1 + c_1\frac{T^2}{m_1}\sum\frac{K_i^2R_i}{K_i^2 - T^2}\right]\right\}. \quad . (8)$$

expressing the index of refraction as a function of the period of vibration of the ray. For waves of period equal to one of the critical periods of the molecule, μ becomes infinite, so that the medium is opaque for such waves, which are entirely absorbed in increasing the energy of the internal vibrations of the molecules. The critical periods of the molecule are therefore the vibration-periods of the dark lines of its absorption spectrum.

§ 2.—*The Index of Refraction as a Function of the Wave-Length.*

As a preliminary to the more general investigation, it will be advisable to trace the dependence of the index of refraction upon the period of vibration in the simple cases $j = 1$ and $j = 2$.

For $j = 1$ the molecule will consist of a core and a single shell, and equation (8) will reduce to

$$\mu^2 = \frac{\rho}{l} - \frac{c_1T^2}{l} - \frac{c_1^2T^4}{lm_1}\frac{K^2R}{K^2 - T^2} \quad \ldots (9)$$

Writing

$$\frac{\rho}{l} = \alpha, \ -\frac{c_1}{l} = \beta, \ -\frac{c_1^2K^2R}{lm_1} = \gamma, \ T^2 = x, \ \mu^2 = y,$$

this may be written in the form

$$y(K^2 - x) = (\alpha + \beta x)(K^2 - x) + \gamma x^2 \quad . . (10)$$

the equation of a hyperbola having the asymptotes

$$x = K_1^2, \ y = (\beta - \gamma)x + \alpha - \gamma K^2 \quad . . . (11)$$

The former represents the single critical period, and the latter practically determines by its direction whether the index of refraction increases or diminishes as T, the period of vibration, increases, and this the more exactly the more nearly the curve coincides with its asymptotes—that is, the more nearly the value of its determinant, which reduces to $-\gamma K^2/4$ approaches the value zero.

There will therefore be three cases to consider—

(*a*) $\beta - \gamma > 0$, μ increases as T increases.

(*b*) $\beta - \gamma = 0$, μ approximately constant.

(*c*) $\beta - \gamma < 0$, μ diminishes as T increases.

There will be two expansions for μ^2 in powers of T, viz.:

For $T < K$,

$$\mu^2 = \alpha + \beta x + \frac{\gamma x^2}{K^2}\left\{1 + \frac{x}{K^2} + \frac{x^2}{K^4} + \&c.\right\}$$
$$= \frac{\rho}{l} - \frac{c_1}{l}T^2 - \frac{c_1^2R}{lm_1}T^2\left\{1 + \frac{T^2}{K^2} + \frac{T^4}{K^4} + \&c.\right\} \quad . (12)$$

For $T > K$,

$$\mu^2 = \alpha + \beta x - \gamma x\left\{1 + \frac{K^2}{x} + \frac{K^4}{x^2} + \&c.\right\}$$
$$= \frac{\rho}{l} + \frac{c_1^2K^4R}{lm_1} - \frac{c_1}{l}\left(1 - \frac{c_1K^2R}{m_1}\right)T^2$$
$$+ \frac{c_1^2K^6R}{lm_1T}\left\{1 + \frac{K^2}{T^2} + \&c.\right\} \quad . (12a)$$

The coefficient of T^2 must be very small in order that the formulæ may be in accordance with experimental results.

Both the equations (12) and (12*a*) give, as a first approximation to the relation between wave-length and period of vibration in the medium considered—

$$T = \lambda\sqrt{\frac{\rho}{l} + \frac{c_1^2K^4R}{lm_1}} \quad \ldots . (13)$$

But λ is approximately proportional to T, so that

$$\mu^2 = A + B\lambda^2 + \frac{C\lambda^4}{\lambda^2 - \lambda_0^2},$$

where λ_0 is the wave-length corresponding to the period $T = K$. This agrees with the results of Helmholtz's theory, and with experiment.[1]

For values of T not in the neighbourhood of K, the hyperbola

[1] Wüllner's "Experimental-Physik," vol. ii. p. 161, fourth edition.

may be replaced by its non-vertical asymptote, and then it follows from (11) that

$$\mu^2 = \frac{\rho}{l} + \frac{c_1{}^2 K^4 R}{l m_1} - \frac{c_1}{l}\left(1 - \frac{c_1{}^2 K^2 R}{m_1}\right) T^2 \quad . \quad . (13a)$$

the right-hand expression consisting of the first two terms of (12a). When $j = 2$, or the molecule consists of a core and two shells, equation (8) becomes

$$\mu^2 = \frac{\rho}{l} - \frac{c_1}{l} T^2 - \frac{c_1{}^2 T^4}{l m_1}\left(\frac{K_1{}^2 R_1}{K_1{}^2 - T^2} + \frac{K_2{}^2 R_2}{K_2{}^2 - T^2}\right) \quad . (14)$$

or

$$y = \alpha + \beta x + \frac{\gamma x^2}{K_1{}^2 - x} + \frac{\delta x^2}{K_2{}^2 - x},$$

where x, y, α, β, γ, have the same meanings as before, and $\delta = - c_1{}^2 R_2 / l m_1$. The curve is therefore of the third order with two vertical asymptotes, $x = K_1$, and $x = K_2$, and a third given by the equation

$$y = \alpha - \gamma K_1{}^2 - \delta K_2{}^2 + (\beta - \gamma - \delta) x \quad . \quad . \quad . (15)$$

If the curve nearly coincides with its asymptotes, μ^2 will be given approximately in terms of T^2 by (15), except near the critical periods, and as before there will be three cases, viz. :—

(a) $\beta - \gamma - \delta > 0$, μ increases as T increases.
(b) $\beta - \gamma - \delta = 0$, μ approximately constant.
(c) $\beta - \gamma - \delta < 0$, μ diminishes as T increases.

Near the critical periods μ^2 will always diminish as T increases.

When the condition (a) is fulfilled, and the curve does not approximately coincide with its asymptotes, μ may continue to decrease as T increases throughout the whole branch of the curve between the two vertical asymptotes, the curve running from the upper left-hand to the lower right-hand side.

The expansions in powers of T will be different for the three branches, viz. :—

For $T < K_1$,

$$\mu^2 = \alpha + \beta x + x^2\left(\frac{\gamma}{K_1{}^2} + \frac{\delta}{K_2{}^2}\right) + x^3\left(\frac{\gamma}{K_1{}^4} + \frac{\delta}{K_2{}^4}\right) + \&c. \quad . \quad . (16)$$

For $T > K_2$,

$$\mu^2 = \alpha - \gamma K_1{}^2 - \delta K_2{}^2 + (\beta - \gamma - \delta) x - \frac{1}{x}(\gamma K_1{}^4 + \delta K_2{}^4) - \frac{1}{x^2}(\gamma K_1{}^6 + \delta K_2{}^6) + \&c. \quad . \quad . \quad . \quad . \quad . (16a)$$

For $K_1 < T < K_2$,

$$\mu^2 = \alpha - \gamma K_1{}^2 + (\beta - \gamma) x - \frac{\gamma K_2{}^4}{x} + \frac{\delta x^2}{K_1{}^2} - \frac{\gamma K_1{}^6}{x^2} + \frac{\delta x^3}{K_1{}^4} - \frac{\gamma K_1{}^8}{x^3} + \&c. \quad . \quad . \quad . \quad . \quad . (16b)$$

The first terms of (16a) are identical with the right-hand side of (15), and therefore if the curve nearly coincides with its asymptotes, it will closely approximate to the curve (14), except near the critical periods. This explains why Cauchy's expansion of μ^2 in descending powers of T, or of λ, gives approximately correct results. In this expansion the coefficient of T^2 vanishes if the asymptote is parallel to the axis of x, viz. if $\beta = \gamma + \delta$, or if

$$m_1 = c_1(K_1{}^2 R_1 + K_2{}^2 R_2) \quad . \quad . \quad . \quad . \quad . \quad . (17)$$

If $\delta = 0$ it reduces to the preceding case; the curve breaking up into the asymptote $x = K_2{}^2$, and a hyperbola. If $\gamma = 0$ it breaks up into the asymptote $x = K_1{}^2$ and a hyperbola.

In general, with a greater number of critical periods, if the curve is of the order n, it will have $n - 1$ vertical, and one other asymptote. To the left of the first vertical asymptote and to the right of the last there will be a hyperbolic branch, and between every two of them will be a branch of the curve proceeding from the upper left-hand to the lower right-hand side, either falling continuously or reaching a minimum, then rising to a maximum, and again falling and approaching the next asymptote. There will be n distinct expansions for μ^2 in powers of T^2, one for each branch of the curve. In many cases the curve, except near the critical periods, will approximately coincide with its non-vertical asymptote, and there will then be the three cases, (a), (b), (c), to consider, as in the previous examples.

§ 3.—*Dispersion and Reflection.*

It is well known that the spectrum of light of a given kind depends on the function of T^2 serving to express μ^2. The dispersion in a refracting medium will be designated as normal when, except near the critical periods, μ^2 diminishes without limit as T^2 increases, and anomalous when μ^2 increases without limit, or passes through a series of maxima and minima. In the first case the colours of the spectrum will appear in their "natural" order, the smaller values of T^2 corresponding to the blue, and the larger values to the red end of the spectrum. In the examples considered in § 2 the dispersion will accordingly be normal in case (c), and anomalous in case (d), while in case (b) the spectrum will be compressed into a line.

When the dispersion is anomalous throughout, the colours will appear in the inverse of the natural order, but it will be otherwise when it is alternately normal and anomalous.

Consider, for example, the non-vertical asymptote in case (c). Then if there are only two critical periods there will be to the left of the asymptote $x = K_1{}^2$, a hyperbolic branch, along which μ^2 will decrease continuously, giving normal dispersion at the blue end of the spectrum above the axis of x. Below this axis μ^2 will be negative, and therefore μ will be imaginary, so that light of the corresponding period will be entirely reflected by the medium. From the point of intersection of the branch of the curve with the axis of x to the point $x = K_1{}^2$ there will therefore be a dark space or absorption band. To the right of this point μ^2 will again decrease from positive infinity to a minimum.

Suppose this to be at a position for which $x = p$ above the axis of x, the curve will then rise to a maximum, say for $x = q$. For $p < T^2 < q$ the light will then be more strongly refracted than for $T^2 < p$, and therefore the corresponding colours will be displaced, and may overlap the colours for which $T^2 < p$. There will therefore be a dark band at the part of the spectrum which should be occupied by them, but this is not now an absorption band, and may be made to disappear by further dispersion. For $T^2 < q$ the dispersion will be normal up to the intersection of the branch with the axis of x, from which a dark band will extend to the point $x = K_2{}^2$, after which the dispersion will again become normal.

Phenomena of this kind have been observed by Kundt and others, and the fact that they follow from the formulæ was considered by Thomson to afford important confirmation of the theory. In fact, taking T proportional to λ, the preceding equations do not differ essentially from those obtained from quite different phenomena by Sellmayer, von Helmholtz, Lommel, and Ketteler, and which have been shown to be in complete accordance with experiment.[1]

Sir William Thomson, in his Baltimore lectures, came to the conclusion that according to his theory metallic reflection would necessarily cause dispersion. This would be the case if there were only a single expansion for μ^2, but in the case of most of the metals there are so many lines, distributed over the whole spectrum, that there is no reason for selecting any one in preference to the others. The fact that all the colours are reflected to practically the same extent, which means that μ^2 must be a negative constant, may be completely explained by the assumptions that the corresponding curve of the nth order approximates very closely to its n asymptotes, and that the single non-vertical asymptote is very nearly parallel to the axis $\mu = 0$. The essential portion of the curve may then be replaced by its horizontal asymptote, as in the cases previously considered, in which $\beta - \gamma$ and $\beta - \gamma - \delta$ respectively were assumed to be nearly zero. The non-existence of dispersion does not therefore afford an objection to the theory.

It is easy to see that by a suitable choice of the disposable constants, the curve may be made to practically coincide with its asymptotes, for consider the curve of the third order given by (14). This may be written in the form

$$(K_1{}^2 - x)(K_2{}^2 - x)(y - \alpha - \beta x) = \gamma x^2 (K_2{}^2 - x) + \delta x^2 (K_1{}^2 - x);$$

or

$$(K_1{}^2 - x)(K_2{}^2 - x)(y - \alpha - \beta x + \gamma K_1{}^2 + \delta K_2{}^2 + \gamma x + \delta x) = x^3(\gamma + \delta - \gamma K_1{}^2 - \delta K_2{}^2) - x(\gamma K_1{}^4 + \delta K_2{}^4) + K_1{}^2 K_2{}^2 (\gamma K_1{}^2 + \delta K_2{}^2),$$

and it is evident that when $K_1{}^2$ and $K_2{}^2$ are given, the right-hand member may be made to vanish by taking γ and δ small enough, and the required condition will then be fulfilled, since the left-hand member equated to zero represents the three asymptotes.

[1] See Wüllner, "Experimental-Physik," vol. ii. pp. 106 and 169, fourth edition. An outline of the various theories of reflection and refraction will be found in the British Association Reports for 1885 and 1887.

§ 4.—*Spectra of Luminous Gases.*

It was first shown by Kirchhoff that glowing gases emit light of the same wave-length, and therefore also of the same period, as that which they absorb.

In the modern theory of gases it is assumed that the molecules of a luminous gas move over a certain distance, the length of the "free path," in straight lines, until they collide with other molecules, or with the sides of the containing vessel, when they move off rectilinearly in another direction.

At every collision the molecule is subjected to an elastic impulse in a direction passing through its centre, causing internal elastic vibrations. The periods of these vibrations could, on the analogy of a corresponding problem in the theory of elasticity, be calculated from a transcendental equation, if the interior of the molecule were uniformly filled with matter; according to Thomson's theory of molecular structure they are determined *a priori*, being the critical periods of the molecule. In fact, during the collisions the external shells only are in contact, but the surrounding ether remains unaffected, and therefore the external vibrations must be of such a nature that $\xi = 0$ (§ 1), which is the condition determining the critical periods. But according to § 1 these periods determine the wave-length of the light absorbed. Thus Kirchhoff's law is a consequence of the theory.

It has hitherto been assumed that the vibrations in a molecule, arising from the collisions, take place along a fixed diameter, and therefore that the vibrations due to one encounter are not disturbed by a later one in another direction. If the temperature or the density of the gas is so great that the encounters follow one another very rapidly, the investigation of § 1 is no longer applicable, and light-waves of other than the critical periods will be emitted. If a second encounter takes place only after the vibration due to the first has nearly subsided, the period of the emitted light will only differ slightly from a critical period. As the density and temperature increase, the bright lines will therefore gradually increase in width.[1] If a molecule receives impulses in different directions in rapid succession, very few of the vibrations will have the critical periods, and therefore the dark spaces between the bright lines will ultimately disappear, and the spectrum become continuous, as is well known to be experimentally true.

§ 5.—*Applications to the Theory of Heat.*

It will be of interest to see what explanation Thomson's molecular hypothesis can give of the manner in which the velocity of gaseous molecules can be increased by the action of heat, as has been assumed in what precedes.

The energy due to the internal molecular vibrations cannot possibly exceed a definite maximum value, for the amplitudes and therefore the velocities of the centres of the shells must have fixed upper limits, since the shells must remain one within the other. This maximum may be attained either for vibrations of a single critical period, or of all the critical periods. Suppose this maximum value to have been nearly reached, then any further disturbance of the internal equilibrium, tending to increase the amplitude of motion of one of the centres beyond the maximum value possible while the centre of gravity remains fixed, will necessarily displace the centre of gravity, whether the disturbance be due to a wave of light or to a mechanical impulse.

This leads to the general and fundamental proposition that "A molecule will begin to move as soon as the energy of its internal vibrations has attained its maximum value, supposing the external influences to which the attainment of the maximum is due continue to act.[2]

The internal equilibrium of a molecule may be disturbed either by light or heat, the disturbance in the case of light being due to its action on the critical periods of the molecule. A medium will therefore be heated when traversed by light-rays; the rays of the critical periods set the molecular shells in vibration, and when the internal energy has reached its maximum value, the centres of gravity of the molecules will begin to move, and this motion will be perceived as heat.

[1] This result may be expressed by saying that the characteristic constant c_i of the molecule is a function of the temperature. It is preferable to regard the ideal spectrum, whether due to emission or absorption, as something definitely fixed; external circumstances merely assisting or hindering its formation.

[2] Sir W. Thomson also points out ("Lectures," p. 280) that a considerable ncrease in the internal vibrations of a molecule must set it in motion, and therefore cause a production of heat.

The energy of internal motions therefore accounts for a portion of the internal work of the mechanical theory of heat.[1]

The external work is effected by the motion of the centres of gravity of the atoms, and this takes place in different and known ways in solid, liquid, and gaseous bodies. Heat may act on a medium either by radiation or conduction. Radiant heat differs from light only in its action on our senses, so that what has been said about light will apply also to radiant heat. In the case of conduction of heat the process is exactly the reverse. The external work of the medium emitting the heat will be transmitted directly to the medium receiving it by contact—that is, by collisions of molecules.[2]

The disturbance of the internal equilibrium of the molecules is here merely a secondary effect, but in this case also the internal energy will gradually increase to the maximum value.[3]

The emission of light by a sufficiently heated solid is explained as in the case of gases, but the spectrum in the case of the solid is continuous.

Just as the action of heat may produce such violent molecular motion as to cause the emission of all possible kinds of light, so the action of light may produce a molecular motion giving rise to a special kind of light. This will only happen, however, when the molecule (owing to specially favourable values of the constants c_i and m_i) is specially susceptible to some among its critical periods. In this way the phenomenon of fluorescence may be explained. G. W. DE TUNZELMANN.

(*To be continued.*)

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 21.—"On the Determination of the Photometric Intensity of the Coronal Light during the Solar Eclipse of August 28-29, 1886. Preliminary Notice." By Captain W. de W. Abney, C.B., R.E., F.R.S., and T. E. Thorpe, Ph.D., F.R.S.

Attempts to measure the brightness of the corona were made by Pickering in 1870, and by Langley and Smith, independently, in 1878, with the result of showing that the amount of emitted light as observed at various eclipses, may vary within comparatively wide limits. These observations have been discussed by Harkness ("Washington Observations for 1876," Appendix III.) and they are again discussed in the present paper. Combining the observations, it appears that the total light of the corona in 1878 was 0·072 of that of a standard candle at 1 foot distance, or 3·8 times that of the full moon, or 0·0000069 of that of the sun. It further appears from the photographs that the coronal light varied inversely as the square of the distance from the sun's limb. Probably the brightest part of the corona was about 15 times brighter than the surface of the full moon, or 37,000 times fainter than the surface of the sun.

The instruments employed by the authors in the measurement of the coronal light on the occasion of the solar eclipse of August 28-29, 1886, were three in number. The first was constructed to measure the comparative brightness of the corona at different distances from the moon's limb. The second was designed to measure the total brightness of the corona, excluding as far as possible the sky effect. The third was intended to measure the brightness of the sky in the direction of the eclipsed sun. In all three methods the principle of the Bunsen photometric method was adopted, and in each the comparison-light was a small glow-

[1] The discrepancies occurring in the determination of the atomic weights of gases may therefore be explained by assuming that internal work is done by the motions of the atoms, instead of assuming, as would otherwise be necessary, that the internal work is only done by the motions of the molecules and a decrease in the attractive force between them. For "motion of the atoms" we should have to substitute "motion of the inner spherical shells."

[2] For the method of deducing the differential equation of heat-conduction from these considerations, see F. Neumann, "Vorlesungen über die Theorie der Elasticität," § 59.

[3] Dulong's law of atomic heat gives some information respecting the relative value of this maximum. This law states that the quantity of internal work due to heating is approximately the same, at any rate when in the gaseous state, for elementary bodies which are ordinarily solid or liquid, a given number of atoms always requiring the same quantity of heat to produce a given rise of temperature. It follows, then, that for these elements the maximum internal energy is very nearly the same. Carbon, silicon, sulphur, and phosphorus behave exceptionally in this, as in many other respects, and the law is not generally true for the elements which are ordinarily gaseous. Since the maximum value of the internal energy depends on the diameter of the molecule, as well as on the constants c_i and m_i, it may perhaps be concluded that the diameter of the molecules of these elements are approximately equal.

lamp previously standardized by a method already described by one of the authors in conjunction with General Festing. In the first two methods the photometer-screen was fixed, the intensity of the comparison-light being adjusted by one of Varley's carbon resistances; in the third the glow-lamp was maintained at a constant brightness, the position of the screen being adjusted along a graduated photometer bar, as in the ordinary Bunsen method. Full details of the construction of the several pieces of apparatus are given in the original paper.

The observations during the eclipse were made at Hog Island, a small islet at the south end of Grenada, in lat. 12° 0′ N. and long. 61° 43′ 45″ W., with the assistance of Captain Archer and Lieutenants Douglas and Bairnsfather of H.M.S. *Fantôme*. The duration of totality at the place of observation was about 230 seconds, but measurements were possible only during 160 seconds, at the expiration of which time the corona was clouded over. A careful discussion of the three sets of measurements renders it almost certain that the corona was partially obscured by haze during the last 100 seconds that it was actually visible. Selecting the observations made during the first minute, which are perfectly concordant, the authors obtain six measurements of the photometric intensity of the coronal light at varying distances from the sun's limb, from which they are able to deduce a first approximation to the law which connects the intensity of the light with the distance from the limb.

The observations with the integrating apparatus made independently by Lieutenants Douglas and Bairnsfather, agree very closely. It appears from their measurements that the total light of the corona in the 1886 eclipse was—

Douglas	0·0123	standard candle
Bairnsfather	0·0125	,,
Mean . . .	0·0124	,,

at a distance of 1 foot.

In comparing these observations with those made during the 1878 eclipse, it must be remembered that the conditions of observation on the two occasions were widely different. The observations in the West Indies were made at the sea's level, in a perfectly humid atmosphere and with the sun at no greater altitude than 19°. Prof. Langley, in 1878, observed from the summit of Pike's Peak in the Rocky Mountains at an altitude of 14,000 feet, in a relatively dry atmosphere and with the sun at an altitude of 39°.

From observations on the transmission of sunlight through the earth's atmosphere (Abney, Phil. Trans., A, clxxviii (1887), 251) one of the authors has developed the law of the extinction of light, and, by applying the necessary factors, it is found that the intensity of the light during the 1886 eclipse, as observed at Grenada, is almost exactly half of that of which would have been transmitted from a corona of the same intrinsic brightness when observed at Pike's Peak. Hence to make the observations of Prof. Langley comparable with those of the authors, the numbers denoting the photometric intensity of the corona in 1878 must be halved. The result appears, therefore, that whereas in 1878 the brightness of the corona was 0·0305 of a standard candle at a distance of 1 foot, in 1886 it was only 0·0124 of a candle at the same distance. Several of the observers of the West Indian eclipse (including one of the authors) were also present at the eclipse of 1878, and they concur in the opinion that the darkness during the 1886 eclipse was very much greater than in that of 1878. The graduations on instruments, chronometer faces, &c., which were easily read in 1878, were barely visible in 1886. In explanation of this difference in luminous intensity it must not be forgotten that the 1878 eclipse was not very far removed from a period of maximum disturbance, whereas in 1886 we were approaching a period of minimum disturbance.

Paris.

Academy of Sciences, August 6.—M. Janssen, President, in the chair.—Fresh experiments on the fixation of nitrogen by certain vegetable soils and plants, by M. Berthelot. These researches, made with three different kinds of argillaceous soil and with plants of the leguminous family, fully confirm the results of previous studies. The fundamental fact that both plants and soil absorb nitrogen under the most diverse conditions is now placed beyond all reasonable doubt. So certain does the author consider this conclusion, that he declines all further discussion on the subject of certain recent negative experiments carried out under defective conditions.—On a recent change in the views of meteorologists regarding gyratory movements, by M. H. Faye. The author claims that the new school of meteorologists, represented by Messrs. Loomis, Meldrum, and Douglas Archibald (see NATURE, June 14, p. 149), shows a tendency to accept his conclusions on certain points at issue. These authorities already admit that the cyclonic movements originate, not on the surface of the earth as had long been contended, but in the higher atmospheric regions, a position irreconcilable with their hypothesis of an ascending, but in full accordance with M. Faye's view of a descending motion.—Summary of the solar observations made at the Royal Observatory of the Collegio Romano during the second quarter of 1888, by M. P. Tacchini. These observations show an increase of the solar spots in May and June, and of the protuberances in April. The general inference is that the relation between these two orders of phenomena is less intimate than might be supposed from previous observations.—On a new apparatus for studying the friction of fluids, by M. M. Couette. This method, differing from those of Coulomb and Poiseuille hitherto employed, is based on the principle indicated by Dr. Margules in 1881 (*Wiener Berichte*, 2nd series, vol. lxxxiii. p. 588). It has the advantage of controlling Navier's theory for very thin tubes and slow discharge, and of operating on gases at constant pressure.—On levulose, by MM. E. Jungfleisch and L. Grimbert.—On the malonates of potassa and soda, by M. G. Massol.—On the hydrates of methane and ethylene, by M. Villard.—On experimental tetanus, by M. Rietsch.—M. A. de Schulten describes a process by which he has succeeded in producing the crystallized anhydrous sulphates of cadmium and zinc (artificial zincosite); and M. A. Poincaré shows how are produced the barometric movements corresponding to the displacement of the moon in declination.—The present number contains the text of the address delivered by the President, M. Janssen, at the unveiling of the monument raised by the city of Tours to the memory of General Meusnier on July 29, 1888.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Nature and the Bible: J. Davis, 2nd edition (Houlston).—Earth Knowledge, Part 2: W. J. Harrison and H. R. Wakefield (Blackie).—The Elementary Geometry of Conics: C. Taylor, 5th edition (Bell).—The Bacon-Shakspere Question: C. Stopes (Johnson).—Curve Pictures of London: A. B. MacDowall (Low).—Great Circle Sailing: R. A. Proctor (Longmans).—Fifty Years of Economic Botany: J. W. Ellis.—Journal of the Royal Microscopical Society, August (Williams and Norgate).—Proceedings of the Liverpool Geological Society, vol. v. Part 4 (Liverpool).—Brain, Part 42 (Macmillan).—Bulletin de l'Académie Royale des Sciences de Belgique, 1888, No. 7 (Brussels).—Quarterly Journal of the Geological Society, August (Longmans).—Meteorologische Beobachtungen in Deutschland, 1886 (Hamburg).

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THURSDAY, AUGUST 30, 1888.

THEORETICAL GEOLOGY.

Theoretische Geologie. Von Dr. E. Reyer, A. O. Prof. der Geologie an der Universität Wien. (Stuttgart, E. Schweitzerbart'sche Verlagshandlung, 1888.)

IT would be most unfair to compare the work before us with any of the numerous treatises on geological science which have during recent years made their appearance in England, Germany, and France. The author's aim, as defined in his preface, has been not so much to give a well-proportioned summary of the ascertained facts of the science, as to prepare an historical and critical review of the ideas that have been put forward concerning the fundamental principles of geology. To find a parallel to the present essay, indeed, we should have to go back to the "Philosophie der Geologie" of Vogelsang, or even to the works of Lyell and Von Hoff.

Those who are familiar with Dr. Reyer's earlier works —"Die Euganeen: Bau und Geschichte eines Vulcanes," and "Beitrag zur Fysik der Eruptionen und der Eruptiv-Gesteine"—will be prepared to find the problems of geology treated by the author, not only with great fullness of knowledge, but with a remarkable freedom from the influence of traditional modes of thought; and they will not be disappointed by the perusal of the present volume. Since the period when his earlier works appeared, Dr. Reyer has travelled very extensively, and has had the fortunate opportunity of studying those splendid manifestations of terrestrial forces which are found in the Western Territories of the United States. Everywhere the reader of this volume is enabled to profit by these widened experiences of its author.

In his preface, Dr. Reyer expresses a regret that there does not exist in Germany the same class of private students of science as is found in this country; for to the labours of men who have been alike free from the conservative pedantry of the professor and from the shallow pretensions of the mere *dilettante*, he justly ascribes a very great part of the credit of advancing geological science in England. The author instances the names of Hopkins and Herschel, but no one acquainted with the history of geology will fail to add those of Hutton, Sir James Hall, William Smith, Scrope, De la Beche, Conybeare, Lyell, Darwin, Godwin-Austen, Sorby, and a host of others. Regret has sometimes, and not unjustifiably, been expressed that the moulding of geological thought has, during recent years, fallen more completely into the hands of those who may be called professional geologists—a result which is perhaps a necessary consequence of the more specialized nature of the study at the present day; but we trust that the day is very far distant when the advance of geological knowledge in England will be wholly, or indeed mainly, dependent on the labours of those engaged in teaching or in making geological maps.

Dr. Reyer seems to hold that it is almost impossible that physical geology and palæontology should be cultivated and taught by the same individual, and he advocates the practical divorce of these two branches of science. It would not be difficult to point out objections to this course and serious difficulties in the way of its adoption; such difficulties must arise in the case of rocks which are wholly or in part made up of the remains of organisms, and in connection with questions concerning the physical conditions under which certain rock-masses have been accumulated, when these can only be adequately discussed after the nature of the organic remains inclosed in them has been taken into account. Nevertheless, no one will contest the author's right to limit the scope of his own discussions to purely physical problems; and, indeed, Dr. Reyer has found himself compelled to confine the present volume to the questions more or less directly connected with igneous activity upon the globe, leaving the problems more especially connected with the waters of the globe and those of cosmical geology for future sections of the work.

Commencing with an account of the explosive action of volcanoes and of the circumstances connected with the outflow of lava from them, the author, enlarging the scope of the inquiry pursued in his former works, proceeds to discuss the physical problems involved in these remarkable phenomena. Observations made in recent years upon the absorption of gases by molten metals and other substances, and the phenomena attending the escape of the gases from such magmas, are fully described; and the bearing of these facts upon the problems of vulcanology are clearly pointed out. English readers will be pleased to find a German treatise in which "Elevation-craters" have finally disappeared, and scarcely less gratified to read our author's conviction, very clearly expressed, that the modified characters of the older lavas, as well as the apparent deficiency of volcanic products among the older geological formations, are due to secondary changes, and that there is no real ground for the supposed absence of granitic rocks among the igneous products of the younger geological periods. We are glad, too, to notice that Dr. Reyer recognizes the value and importance of the observations of Scrope and Darwin upon the banded structure produced in viscid lavas; though we think he fails to appreciate the full bearing of these facts when he afterwards proceeds to discuss the important question of the origin of foliation.

In the discussion of the problems connected with the folding and faulting of rock-masses, during mountain-making, Dr. Reyer exhibits the fullest knowledge and impartiality. To the labours of Henry Rogers and other American geologists, who nearly fifty years ago worked out the structure of the Appalachians with such remarkable skill and geological insight, he renders full justice, and not less to the observations of their able successors who have in recent years shown what singular variations from the normal structure of mountain masses exist in the Western Territories of their country. It is a fortunate circumstance that the eastern and western portions of the United States should present such perfect examples of the diverse structures found in mountain ranges, and that the geologists of that country have proved themselves so capable of dealing with the grand but difficult problems presented for their study. But at the same time our author has fully set forth the value of the researches of Lory, Baltzer, Heim, and others, who have shown that the structures found in the Appalachians are equally characteristic of the Alps, and the more denuded mountain chains of Central and Northern Europe. In explaining the causes of regional or mechanical metamorphism, Dr. Reyer fully appreciates the importance of the experimental researches of Tresca, Daubrée, and Spring; while he fails not to point out the important additions and confirmation of the theory of "mechanical metamorphism," which are furnished by the microscopical investigations of Lossen, Lehmann, and other recent authors on the subject.

Seismology, the study of earthquake phenomena, is usually treated by the writers of text-books as a branch of vulcanological science; but we agree with the author in regarding it rather as connected with the great movements of earth-masses. It finds an appropriate place in this work between the chapters dealing with dislocations of the earth's crust, and those devoted to the great secular movements of the earth's surface.

In a work like the present, devoted to a discussion of problems of the greatest difficulty, many of which are far from ripe for solution, some of the views of the author will be sure to challenge criticism and others to provoke dissent. Every unprejudiced reader will admit, however, that Dr. Reyer's presentation of his views upon these problems is characterized not only by much originality of thought, but by a studious fairness of manner. The citation of original authorities in every case is a most praiseworthy feature of the work, and those writers from whom the author differs have no cause to complain, as is too often the case, that he has not even tried to understand their arguments. Nowhere does there exist such a rich storehouse of facts and observations bearing upon the great questions of geology as in the volume before us, and we cannot doubt that the completion of Dr. Reyer's important work will mark an epoch in the history of the science, and at the same time constitute an important starting-point for further advances. J. W. J.

A GUIDE TO THE LICK OBSERVATORY.

Hand-book of the Lick Observatory of the University of California. By Edward S. Holden, LL.D. (San Francisco: The Bancroft Company, 1888.)

THERE are two classes of readers to whom this little book ought to be especially welcome—namely, those who propose to visit California, and those who do not so propose. Travellers will miss from it no useful item of information. They are told where to lodge, what to wear, how to get themselves conveyed to their destination, what to look at and admire. They are put, moreover, in the proper frame of mind for approaching an astronomical sanctuary. The coldest and dullest can hardly under such guidance remain utterly apathetic and unintelligent. The general interest of the work, on the other hand, is sufficiently attested by a glance at the table of contents. It includes a "Sketch of the Life of James Lick," the founder of the Observatory, a history of the institution, descriptions of the buildings and instruments, with sections on "The Work of an Observatory," "Telescopes," "Astronomical Photography," "Clocks and Time-keeping," and "The Principal Observatories of the World." On none of these subjects are there many, on some there is no one entitled to speak with greater authority than Prof. Holden. Nor is there a second astronomer in the world whose utterances—so far as they are an index to his intentions—are at present of higher moment to science. The future course of observation largely depends upon his use of the vast opportunities placed in his hands. A colossal experiment is being tried at Mount Hamilton; its upshot will lay down the lines of astronomical effort for many a decade to come. For results govern the star-gazing, no less than every other section of mankind.

Prof. Holden vainly, we fear, seeks to disabuse the public of its fixed idea that "an astronomer's business is to watch the heavens go by and to 'make discoveries.' Exactly what these discoveries are," he goes on to say, "is usually not stated, but unless a sufficient number are forthcoming the astronomer is held to be blameworthy." The Lick Observers, however, possess a unique advantage in the value of their negative results. "What we cannot see with our telescope, the most powerful of all, in our elevated situation, the best in the world, need not be looked for with inferior telescopes in less favoured situations."

Celestial photography is evidently designed to be vigorously prosecuted on Mount Hamilton. "One of the principal objects of the Observatory," we are told, "will be to make a photographic map of the heavens, by means of the large telescope and its photographic objective." If carried out on the scale which appears to be indicated, this will indeed be a gigantic undertaking. Its plan is doubtless not yet definitely laid down, but exposures of three hours are spoken of. On Mount Hamilton, two hundred nights in the year—just double the low-level allowance—can be counted on as fit for such work; yet even so, twenty-five years should elapse before the whole sky could be *once* covered by plates each embracing four square degrees, and exposed during three hours. And the resulting priceless record would lose, unless obtained in duplicate, great part of the value properly belonging to it.

The time-service of the Lick Observatory has been for some time completely organized. Every railway-clock in the Southern Pacific States is now regulated from Mount Hamilton. Any watch in San Francisco can be set by the beats of the Lick standard clock, rendered audible by telephone at a distance of sixty miles. The time distributed is the "Pacific standard," which is 6m. 34·3s. faster than the Mount Hamilton local time. Numerous plans and illustrations enhance the usefulness of the "Guide to the Lick Observatory." A. M. C.

OUR BOOK SHELF.

Curve Pictures of London for the Social Reformer. By Alex. B. Macdowall, M.A. (London: Sampson Low, 1888.)

THIS little volume ought to be of great service to all who interest themselves practically in questions relating to social reform in London. It presents by means of diagrams a large amount of trustworthy information about population; density of population; birth, marriage, and death rates; early marriages; death by disease; suicides; drunkenness; licensed houses; apprehensions; felonies;

pauperism; education; illiteracy; prices of commodities; and prices of wheat. Students who may wish to know the recent history of London with regard to any one of these subjects will at once find what they want by turning to the diagram or diagrams referring to the matter. Opposite each diagram are short notes indicating clearly and concisely what the curves appear to teach, and directing the reader to the original sources from which the facts are taken. It is impossible to turn over these pages without feeling, as the author does, that if some improvement of the social condition of London is discernible it is, after all, but meagre. Probably, too, most people who make themselves familiar with the results he has so carefully classified, and rendered so easily intelligible, will agree with him that in dealing with the social problem we as a people are apt to think too much about cure, and too little about prevention. "Year by year," says Mr. Macdowall in his interesting preface, "we reap, somewhat sadly, our weedy crop; but we leave the weed-roots in the ground. To use another figure, we contend in a vigorous way with the waters of a domestic deluge, but omit to turn off the tap from which they come."

A System for the Construction of Crystal Models. By John Gorham, M.R.C.S.Eng., &c. (London and New York: E. and F. N. Spon, 1888.)

THE author of this book expounds an ingenious method of making models in paper by plaiting together three or four strips cut into the form of a succession of the crystal faces. The book consists mainly of figures, which show how these plaits are to be drawn, and the order in which they are to be interwoven for some of the primitive forms in the different systems.

It does not appear that the models are more easily or neatly made by this than by the more familiar methods, but they have one real advantage in their portability, since they may at any time be unfolded into a flat sheet. The method would, however, be somewhat awkward when applied to complicated combinations.

Some of the simple forms are omitted in the descriptions, *e.g.* the icositetrahedron, pentagonal dodecahedron, &c., and it is hardly necessary to remark that the four-faced cube is not a form assumed by some varieties of quartz (p. 8). We hesitate to believe the author serious in his suggestion that a natural cube may actually grow by plaiting itself from three zones of molecular laminæ, "each endowed with a force compelling it to bend at a right angle at given intervals."

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Functionless Organs.

I HAVE only just seen NATURE for August 16 and 23 (pp. 364 and 387). In both these there are letters which attribute to me personally the assertion that the electric organs in "the skate" are functionless, and are "on the way to use"—not aborted or degenerated from former use. I made no such assertion. My letter on the subject referred to a verdict given on this question in respect to one particular species (*Raia radiata*) by Prof. Ewart in NATURE of July 26 (p. 310). I refer Prof. Ray Lankester to the paper of Prof. Ewart, communicated to the Royal Society through Prof. J. Burdon-Sanderson. As the result of an elaborate argument, founded on anatomical details, the author concludes that the "cups of *Raia radiata* are in process of being elaborated into more complex structures"; and again, "that the electric organ of *Raia radiata*, notwithstanding its apparent uselessness and its extremely small size, is in a state of progressive development."

This is not my conclusion, but the conclusion of an expert, who gives his reasons, and differs from Prof. Ray Lankester in having, apparently, no preconceived theory to support.

If the doctrine of evolution be true—that is to say, if all organic creatures have been developed by ordinary generation from parents—then it follows of necessity that the primæval germs must have contained potentially the whole succeeding series. Moreover, if that series has been developed gradually and very slowly, it follows also, as a matter of necessity, that every modification of structure must have been functionless at first, when it began to appear. On this theory it seems to me to be not a matter of argument, but a matter of certainty, that all organic nature must have been full of structures "on the rise," as well as of others on the decline.

Why is this not recognized? Because organs "on the rise" cannot be due to utility as a physical cause, but must be due to utility as an end yet to be attained. This is what I mean by a "prophetic germ." We now know that Darwin resisted and rejected this idea, at least at one time of his life, as fatal to his own theory of natural selection. And so it is, if natural selection is made to account for structures before they are presented for selection to act upon. But this is obviously nonsense. Things cannot be selected until they have been first produced. Nor can any structure be "selected by utility in the struggle for existence" until it has not only been produced, but has been so far perfected as to be actually used.

If Prof. Ray Lankester will explain how "natural selection" can act upon "congenital variations" which he calls "non-significant"—*i.e.* which are not yet of any actual use—and if he will explain how this action can afford "the single and sufficient theory of the origin" of (as yet) useless variations, he will have accomplished a great triumph in logic and philosophy.

Meantime, I adhere to that view of all organs which is indelibly impressed on our very forms of speech, and is notably expressed in Prof. Burdon-Sanderson's letter in NATURE of August 23. He speaks of electric organs as "an apparatus for producing electric discharges." This is exactly correct. They are "apparatuses" for a special purpose or function; and like all other apparatuses, they have to be prepared through embryotic stages in which they are not capable of use. I have been long looking for some actual case in which experts should recognize an organ "on the rise." Prof. Ewart's is the first I have seen. I am not responsible for his facts, or for his reasoning. But the mere fact of such a view being taken by an eminent man in a responsible position is a circumstance highly significant.

The recognition of even one case will be the recognition of a new idea—new, at least, in its application, and new in its wide significance of interpretation. It will be the counterpart in actual observation of that strategic movement in abstract reasoning which has recently led Mr. Herbert Spencer to expose the fallacies involved in the phrase "natural selection," and in his own neater and adroiter form of it, "survival of the fittest."

ARGYLL.

I HAVE read with much interest the report in NATURE of July 26 (p. 310) of Prof. Ewart's very remarkable paper on the electric organ of the skate, and the Duke of Argyll's letter on the same subject in NATURE of August 9 (p. 341). The Duke is manifestly right, that a single proved instance, such as Prof. Ewart here endeavours to make out, of an organ which has been evolved, or is in process of evolution, while not in a state of functional activity, would be sufficient to disprove Darwinism as a complete theory; for if all perfectionment is due to the two causes of exercise through habit and natural selection among variations, it is obvious that no improvement can be effected which is not immediately useful.

I believe that the animal kingdom, and in all probability the vegetable kingdom also, are full of organs which cannot have been evolved by anything like a Darwinian process, because their immature states cannot have been in functional activity. In my work on "Habit and Intelligence" (2nd edition, Macmillan, 1879), chapters xvii., xviii., and xix., I have enumerated some of these. The strongest part of the argument is, I think, that derived from the brain of man. It has been pointed out by Wallace, the naturalist who was near anticipating Darwin in the theory of natural selection as applied to the rest of the organic creation, that the brain of savage man has attained a development which is out of all proportion larger than can correspond to the mental development which is united with it—in other words, the brain of savage man is nearly equal to that of civilized man, while his mental development is very far inferior;—so that, as Wallace remarks, "the idea is suggested of a surplusage of power; of an instrument beyond the wants of its possessor." And if it is true, as I believe it is, that the brain of savage man finds its special activity in the formation and use of language, this does not solve but only transforms the difficulty; for language itself must in prehistoric times have attained a development far in advance of the intellectual wants of those who formed it, because the same languages, with comparatively few additions to the vocabulary and no further grammatical development, still suffice for the wants of their civilized descendants; whereas, on Darwinian principles, language could not be evolved beyond the intellectual needs of those speaking it.

There are also many cases in the lower creation where structure appears to have been developed, not as the result of function but in anticipation of function; just as a ship is built on the land for the purpose of afterwards floating on the water. I cannot occupy your space with details of these, but will enumerate those of which the evidence seems tolerably distinct.

All the Hydrozoa are probably descended from a form resembling the Hydra, between which and the Medusæ there is a gradation, though not quite unbroken. Once the Medusa is produced and swims away from the plant-like stem that bore it, its powers of wandering, and dispersing its eggs widely, will give its species a great advantage in the struggle for existence. But how can any natural selection effect the evolution of the Medusa while it is still imperfect, and sheds its eggs without leaving the parent stem?

Müller, in his "Facts for Darwin," says of the transition from the Zoea to the Mysis form in the metamorphoses of a species of Peneus, or prawn, that "the long abdomen, which just before was laboriously dragged along as a useless burden, now, with its powerful muscles, jerks the animal through the water in a series of lively jumps." The Nauplius, which is the form in which this Peneus leaves the egg, has no abdomen; this is acquired when the Nauplius develops into a Zoea, and consists of segments which appear between the body and the tail of the Nauplius. Müller's account seems to show that this abdomen is developed before it is useful to the animal, and for the purpose of becoming useful further on in its development. It is to be observed that in this case, as in that of the Medusa, the entire evolution goes on amid the same surroundings: unlike the case of Batrachia and most insects, there is no change of the conditions of life to accompany the transformation and to help to account for it. The same remark applies to the development of the star-fish out of the Bipinnaria, and of the sea-urchin out of the Pluteus—two of the most wonderful metamorphoses known.

The development of the lungs of the Batrachian out of the swim-bladder of the fish is an adaptive modification, and presents no special difficulty. But in the cellular and spongy texture of the swim-bladder of many Ganoid fishes, there appears to be a preparation for future transformation into lungs. This, however, is a point on which it would not be right to lay much stress. But it is different with the development of the fin of the fish into the leg of the Batrachian. The intermediate state appears to be preserved in the fin-rays—we can scarcely call them fins—of Lepidosiren. The single fin-ray of Ceratodus has in Lepidosiren lost its membrane, and consequently become inefficient as a fin, without being in any degree efficient as a leg, or acquiring any vestige of a foot; such a change cannot be beneficial to an animal which is still a fish and lives a fish's life; it can be interpreted, so far as I see, only as a preparation for the ultimate development of feet and legs. This development, however, does not appear to have been actually attained by any descendant of Lepidosiren; for its scaly covering, and the peculiarity of its nostrils, go far to forbid the supposition that the Batrachia can be its descendants.

Another instance of the same kind is that of those Ascidian larvæ which are the probable origin of the Vertebrata. Of what use can the dorsal groove and the notochord be to those minute and lowly organized animals themselves? They appear capable of interpretation only as the preparation for a vertebral column and a spinal cord to be afterwards evolved. But the strongest instance of the kind which I know of, except that of the brain of man, is the existence of pneumatic bones (that is to say, bones hollowed out for lightness, like those of flying birds) among Dinosaurians (see Prof. Cope's paper on *Megadactylus probyzelus* as reported in NATURE, vol. i. p. 347). The resemblances of the skeleton appear to prove that birds must be descended from Dinosaurians. No Dinosaurian had the power of flight, yet here is a character useful only to flying animals, and interpretable only as a preparation for a power of flight to be afterwards evolved.

Were a competent anatomist and morphologist to search for them, the entire organic world would probably be found to be full of such instances of what I call structure in anticipation of function.

JOSEPH JOHN MURPHY.

Belfast, August 22.

It is evident from the letter of the Duke of Argyll under this heading in your issue of August 9 (p. 341), that he has altogether misconstrued some of the main biological principles which Darwin promulgated; and it also appears as if the entire neglect of certain important items which received due consideration in the "Origin of Species" is either done with purpose, or else is simply an effect of obscuration. In either case, the fallacious interpretation may be due to the polemical style in which his Grace is usually wont to distinguish himself, and the strong bias imported into the treatment has rendered a true representation of the conclusions he assails altogether impossible.

Exception must be taken to the very setting forth of the premises of the Duke's argument. "Sometimes," he says, the organs "are *called* 'aborted,' sometimes 'degenerated,'" &c. This certainly is so for no less a reason than that sometimes they *are* aborted, while at other times they are "representative," or sometimes, again, they are incipient organs. So variously, indeed, are the organs affected, that Darwin found it in some cases extremely difficult to pronounce respecting them.[1] The uninitiated in the subject would naturally infer, from the letter in question, that Darwin had never devoted any of his pages to the discussion of those organs which he generally spoke of as "nascent"—a term which the Duke, for the purposes of his argument, ignores. There is, moreover, nothing in any way new suggested by him in his communication.

The special case referred to, for instance, is simply one of the difficult problems which Darwin set himself, in the "Origin of Species," to solve, and respecting which he concluded, from his knowledge of all the facts then available, that "as we know nothing about the habits and structure of the progenitors of the existing electric fishes [of the various non-related types dealt with], it would be extremely bold to maintain that no serviceable transitions are possible by which these organs might have been gradually developed" ("Origin," sixth edition, p. 150). Whilst in the "Descent" a long section is devoted to the citation of instances of homologous rudimentary structures in man, and functionless organs generally are amply treated upon elsewhere—compare the various references in "Variation," and also "Origin" (pp. 108–12). In some instances they are determined to be vestigial, though for the most part they can only be so recognized in their ancestral relation.

Although Prof. Ray Lankester, in his interesting chapter on "Degeneration" ("Nature Series"), aptly remarked, "We have as possibilities either *balance*, or *elaboration*, or *degeneration*," I am inclined to think—in agreement, probably, with the Duke—that in that paper perhaps too much weight was attached to the last-named process. Perhaps it was due to the expressed desire of calling attention to Dr. Dohrn's treatise on the subject; but, after all, it is simply a view that is taken respecting certain important facts, and whether an organ in a transitional state is progressing or retrogressing is a matter which relates chiefly to time, and does not invalidate the fact of the change that is in process of evolution. It has not, however, been satisfactorily proved, so far as I can find, that the limited digitation in *Bipes* and *Seps* is the result of atrophy, as decidedly stated by Prof. Lankester; but as I am uninformed respecting the ontogeny—upon which everything depends—of these reptilian forms, I may be wrong in questioning this point.

Instances of transitional and incipient organs, rare though they may be, *have*, therefore, been fully considered by Darwin in his volumes; but we may, speaking more generally, truly say that the entire development hypothesis is a recognition of the structural deformation of nascent organs, which are ever being reconstituted, mainly in a progressive direction. The difficulty of actually observing an organ in process of development was acknowledged by Darwin to be considerable, partly on account of the slow rate of progression, and partly because

[1] "It is often difficult to distinguish between rudimentary and nascent organs; for we can judge only by analogy whether a part is capable of further development, in which case alone it deserves to be called nascent" "Origin," sixth edition, p. 398).

"nascent organs will rarely have been handed down, . . . for beings with any important organ but little developed will generally have been supplanted by their descendants with the organ well developed" ("Life and Letters," vol. ii. p. 214). If, however, the Duke of Argyll is prepared to furnish a chapter upon the evidences of "prophetic germs," with which he appears to be extensively acquainted, he will confer a favour upon the waiting scientific world by publishing it.

With regard to the utility of rudimentary organs, the Duke has perverted entirely the doctrine given by Darwin, who certainly did *not* ascribe "*all* organic structures to utility as a physical cause," while even if that were the case, the doctrine of prophetic germs is in no way opposed to it. In his lengthy letter to Lyell, which probably the Duke has in his mind, Darwin remarks:—"A nascent organ, though little developed, as it has to be developed, must be useful in every stage of development. As we cannot prophesy, we cannot tell what organs are now nascent" ("Life," ii. p. 213). This observation, before it can be properly understood, however, requires to be amplified by reference to the full text from which it was abstracted, in which the following remarks also occur:—"In many cases we are far too ignorant to be enabled to assert that a part or organ is so unimportant for the welfare of a species that modifications in its structure could not have been slowly accumulated by means of natural selection. In many other cases modifications are probably the direct result of the laws of variation, or of growth, *independent of any good* having thus been gained" ("Origin," pp. 165-66). Yet natural selection "will never produce in a being any structure more injurious than beneficial to that being" (p. 162), and Darwin fully held that it does not necessarily lead to "absolute perfection," which, indeed, would almost amount to a denial of the necessity of variation. Furthermore, "useful organs, however little they may be developed, unless we have reason to suppose that they were formerly more highly developed, ought not to be considered as rudimentary. They may be in a nascent condition, and in a progress towards further development. Rudimentary organs, on the other hand, are either quite useless . . . or almost useless; . . . they cannot have been produced through variation and natural selection, which act solely by the preservation of useful modifications. They have been partially retained by the power of inheritance, and relate to a former state of things" (*et seq*, 'Origin," p. 398).

The above references I have quoted somewhat fully, because they appear to me to present a complete disavowal of the two assertions made by the Duke of Argyll, namely (1) that the physical cause of all organic structures is ascribed to utility; and (2) that functionless organs "are never interpreted as utilities which are yet to be." WILLIAM WHITE.

August 18.

Lamarckism *versus* Darwinism.

MR. POULTON says it is to be regretted that I have not written anything which can be considered as a reply to his previous letter. But this is exactly what I did. In that letter he merely made the bald statement that I had not acquainted myself with the views of Prof. Weismann, which I had "professed to express." This statement I denied, and what further "reply" it admits of I must leave Mr. Poulton to explain.

Regarding his present suggestion, that it would be well for me to justify a remark in the *Contemporary Review*, which he challenged, I can only repeat that I have no desire to continue a correspondence which was opened in the manner alluded to. And, in repeating this plain statement, I am as far as ever from experiencing any of the "annoyance" which he says I have taken no pains to conceal. Unless by annoyance he means surprise, I am at a loss to understand why he should suppose that I entertain any feeling of the kind.

Prof. E. Ray Lankester's views "upon the interesting question of Lamarck *versus* Darwin" are, of course, well known to all biologists; but I think it is somewhat too strong a statement to say that they are "diametrically opposed" to mine. No doubt he has been more influenced by Prof. Weismann's recent theories; but I feel sure he would agree with me that the time has not yet come for the formation of any matured "opinion" upon this subject.

Prof. Meldola is kind enough to express disappointment that I have not given a more explicit statement of my views on the theoretical bearings of Mr. Poulton's experiments. The tone of this invitation induces me to comply with his request. But in order to do so it will be necessary to go at considerable length into the whole question of "Lamarckism *versus* Darwinism." As this has now become a very extensive and somewhat involved question, I cannot feel that the correspondence columns of NATURE afford a suitable place for its discussion. But I will bear the matter in mind, and, as soon as other work shall have been cleared off, will publish an essay upon the whole subject. Mr. Poulton, too, will then find that it is easy enough to "notice the criticism," without requiring "to show that his [my] remark about Prof. Weismann is intended to bear some other than its obvious meaning."

Meanwhile, I should like to represent how undesirable it is to employ phraseology which associates the name of Darwin with the post-Darwinian theories which are in question. Here, for instance, we have a correspondence headed "Lamarckism *versus* Darwinism" where the title ought to be "Darwinism *versus* Weismannism"; and while on the one hand Prof. Meldola speaks of "the recent revival of 'pure' Lamarckism," on the other he alludes to Prof. Weismann's interpretations as those which belong to "the purely Darwinian stand-point." The consequence of this kind of writing is that anyone who, like myself, still retains unmodified the Darwinism of Darwin himself, is ticketed as a follower of Lamarck. Therefore, if only as a matter of historical accuracy, and in order to avoid confusion in non-biological circles, it seems to me that such terms ought to be avoided. Most people will always understand that "Darwinism" is intended to mean the theory of evolution as held by Darwin, yet this is just the very thing that it is not intended to mean by your other correspondents: the more "pure" their "Darwinism," the further does it depart from the doctrine of evolution as presented in the "Origin of Species." As a matter of fact, there has been no "recent revival of 'pure' Lamarckism": there has merely been a question raised as to whether the amount of Lamarckism which was sanctioned by Darwin may not be dispensed with. Now, not only is this question, as already remarked, post-Darwinian in its origin, but the speculations which have given rise to it are ultra-Darwinian in their object: they aim at establishing for natural selection a sole and universal sovereignty which was never claimed for it by Darwin himself. Far be it from me to pre-judge a question which must assuredly involve a large amount of future research; but however this question may eventually be decided, there is no need to confuse the issues by the use of historically inaccurate terms. The school of Weismann may properly be called Neo-Darwinian: pure Darwinian it certainly is not.

GEORGE J. ROMANES.

Geanies, Ross-shire, N.B., August 26.

A Substitute for Carbon Disulphide in Prisms, &c.

IT may be worth mentioning that the highly-refractive liquid, phenyl-thiocarbimide (molecular formula, C_6H_5NCS), to which I drew attention some little time ago at a meeting of the Physical Society (see NATURE, vol. xxxvii. p. 165), can now be obtained as an ordinary article of commerce from Schuchardt, of Görlitz, and Kahlbaum, of Berlin.

I have recently again determined its refractive indices for different rays, but the values (when allowance is made for temperature) do not appreciably differ from my former results, or from those given by Nasini (*Atti R. A. dei Lyncei*, June 20, 1886) for the substance which he calls "iso-solfocianato-fenilico," as Dr. Gladstone has kindly mentioned to me since the meeting. Thus, taking rays near the ends of the visible spectrum, I find (at a temperature of 10° C.)—

Fraunhofer line.	Index of refraction.
B*	1·639
G	1·707

The coefficient of dispersion, calculated from the above values, is 0·068, rather higher than even that of carbon disulphide for the same rays, which is 0·062.

Phenyl-thiocarbimide seems, in fact, to have about the highest refractive and dispersive power of any fairly permanent *colourless* liquid known at present. Carbon disulphide, though colourless when pure, is apt to turn yellow with age and exposure to light, and monobromonaphthalene has an incurably yellow colour (as also has mercury barium iodide), which of course implies an absorptive action on the more refrangible part of the spectrum.

* Not A, as printed in NATURE (*loc. cit.*). The index for A is 1·6312.

Dense glass shows the same defect, besides its great liability to become tarnished.

Another advantage possessed by phenyl-thiocarbimide is its very high boiling-point, viz. 222° C. It is practically non-volatile at ordinary temperatures, and a lighted match may be put into it without setting it on fire. It has, of course, the pungent smell characteristic of all the mustard-oils (to which class it belongs); but this, from the slight volatility of the substance, gives no practical inconvenience.

It dissolves iodine freely, but, from the complexity of its molecule, we cannot expect it to be so diathermic as carbon disulphide. It also readily dissolves metacinnamene (one of the most highly-refractive resinous substances that I have been able to meet with; $\mu_D = 1{\cdot}6$, nearly), and the viscous solution is useful in determining refractive indices by Wollaston's total reflexion method, or Bertrand's modification of it.

It can be safely used in the ordinary hollow prisms, as it has no action on the mixture of isinglass and sugar with which these prisms are cemented. H. G. MADAN.

Eton College, August 28.

Michell's Problem.

I HAVE read with considerable interest the short letter in NATURE of August 9 (p. 342), in which Mr. Joseph Kleiber refers to a paper of his own on the controversy between Michell and Forbes, and notices what he believes to be a mistake in my paper of July 19 (p. 272). Mr Kleiber shows that the experiments of Forbes on random distribution by scattering rice over a chess-board, and also some additional experiments of his own on numbers taken successively from a table of logarithms, are in accordance with the ordinary formula for finding the probable number of squares containing r grains, where m is the number of squares and n the total number of grains—

$$mp_r = m\,\underline{|n}\,(1/m)^r(1-1/m)^{n-r}/\underline{|r}\;\underline{|n-r}.$$

He concludes that "the theory of probabilities does not affirm that 'a perfectly uniform and symmetrical disposition of stars over the sky would (if possible) be that which could alone afford no evidence of causation or any interference with the laws of random.'"

Forbes, throughout his paper, is not attempting to controvert either the theory of probabilities, of which he himself makes frequent use, or the result arrived at by Michell and Struve, but only Michell's method of applying the theory of probabilities to prove his point. Hence it seems to me that Mr. Kleiber's paper is not so convincing as he takes it to be; one may agree with his experiments, mathematics, and conclusion, without admitting the truth of Michell's argument.

Mr. Kleiber's second objection is, I think, founded on a misconception due possibly to too great regard on my part for the exigencies of your space. As an example of distribution, I suggest a number of stars shot at random from the centre upon the interior surface of a sphere. The idea may be roughly represented by the explosion of a small uniform shell of shot in the centre of a globe lined with clay. I then attempt to prove that the chance of exactly uniform distribution is *nil*, and proceed: "Michell, however, seems to assume this probability to be 1, or certainty." Mr. Kleiber strongly repudiates any such assumption on the part of Michell. It is always, I admit, a little doubtful to attribute to anyone an opinion which is not distinctly stated, but to Forbes, as well as to myself, the assumption seems to be clear. For if the distribution is not uniform, and any groups of stars are formed, Michell's argument applies in its entirety, and he would prove, of course with greater or less probability, *a posteriori* that the arrangement is due to a cause, while *a priori* from the datum of shooting out at random, the distribution is due to chance. SYDNEY LUPTON.

Remarkable Rainbows.

ON Saturday, at 3.15 p.m., there was a very brilliant primary rainbow, and a faint secondary bow above. Inside the primary, at first to the right of the centre, afterwards over the entire centre, were two other very faint bows, their colours in the same order as those of the primary, but with no distinguishable red, the violet of the upper bow seeming to touch the orange of the bow below in each case. Green was the most striking colour in the two inner bows, whose breadth appeared equal to each other, but considerably less than that of the primary; part of this effect being due to the loss of the red, probably all the remainder to irradiation. The perfect primary arch lasted fifteen minutes; an arc of the eastern side half an hour. The sun being comparatively high, the centre of the arch was low, and the bow looked flat. There was no wind, and many of the rain-drops were large, others mere dots.

At 5.5, during a thunder-shower, there was a fairly bright primary with perfect arch, and two faint arcs of a secondary bow, but I saw no trace of inner bows. This primary differed in height and brightness from the other, and the rain was a downpour of heavy drops.

Judging by relative brightness, the inner bows should seldom be seen. L. J. H.

Rock Ferry, August 27.

I HAVE been unable to send sooner the following, which you may perhaps think worth inserting in NATURE.

On the 18th of July, at 7.30 p.m., I saw a most remarkable rainbow. A sudden light fell on the book I was reading, so powerful that I thought it must be some neighbouring house on fire. It was a rainbow coming across the mountains opposite (Savoie), and ending in the lake just at the "Bec de Peilz," which some of your readers will know. It was only a section of the rainbow, and was not continued in any other part of the sky, and it was so small a section that it scarcely appeared bent, but looked like a fiery column coloured as a rainbow, but having the peculiarity of *not showing the mountain through it*: it cut it sheer off, and yet the mountain was looking unusually dark, and indeed the brilliancy of the sunset was such that all came out in strong relief. The sky was covered with stormy clouds with breaks of brightness, and above this column they hung in golden radiance such as only painting could faintly convey an idea of. Certainly I never saw so beautiful or curious a sight. It lasted about six minutes. M. C. C.

La Tour de Peilz, August 17.

Sun Columns.

I HAVE never before seen the phenomenon of sun columns in such splendour as on the 11th inst. The day was very hot, the wind a pretty stiff westerly one, and the sky perfectly cloudless. After sunset, which (according to the calendar) took place at 7h. 27m. p.m., several sun columns became visible. They were seen to grow in length, and at 7h. 40m. they extended over the whole sky. The columns were five in number, and pretty regularly distributed, so that one passed through the zenith, two on the north, and two on the south of it at equal distances. A very small cloud was visible at that time in the west-north-west. The colour of the shades was dark blue, and their width in the zenith from 2° to 4°. The lighted parts of the sky had a pale violet colour. The rays extended over the whole sky like meridians on a globe, and all five columns were seen to meet in one point in the east-south-east, about 5° above the horizon. The phenomenon could be seen well in all its extension, as I watched it from a hill 688 metres above the sea-level. The intensity of the colour of the columns was at its highest at 7h. 45m. (Prague local time), and it disappeared at 7h. 50m.

I hope, Sir, that you will be able to mention this not very common phenomenon in the columns of NATURE.

St. Benigna, Bohemia, August 14. B. BRAUNER.

Meteor.

THE most brilliant meteor I have ever seen flashed across the sky here from east to west, about 7.10 p.m. on the 30th ult. I was riding along a dark road, looking downwards, when suddenly the road was so brightly lit up that I thought the lamp-lighter had lit another lamp. Seeing neither lamp nor lamp-lighter, I looked up, just in time to catch a glimpse of the meteor. It was of an intense white colour, with a train or track of white behind it. When about 45° above the horizon, it appeared to burst like a sky-rocket, but not so violently. It lasted about two seconds. H. W. L. HIME.

Coonoor, Madras, August 1.

P.S.—August 2. Since writing my letter of yesterday I am informed by Lieut. M. de Montmorency, Hampshire Regiment, that the meteor I mentioned burst *with a loud noise*. I can only suppose that the noise of my horse's hoofs prevented me from hearing it.—H. W. L. H.

Fire-ball of August 13—August Meteors.

A COMPARISON of several good observations of this brilliant visitor shows that the point of its first appearance was over a place near Masham, in Yorkshire, at a height of 79 miles. The disappearance occurred over Gisburn, in the same county, after the meteor had traversed a course of about 48 miles, and when it had descended to within 47 miles of the earth's surface. It was directed to a point covered by the River Mersey a few miles west of Liverpool.

By a clerical error the figures representing the real paths of three of the seven doubly-observed meteors seen on August 5 and 8 last (NATURE, August 23, p. 395) were incorrectly stated. The lengths of Nos. 1, 3, and 7 in the list should be 41, 41, and 36 miles respectively.

Including the fire-ball mentioned above, the mean length of path of eight meteors seen during the present month was 37 miles. Seven of these bodies were Perseids, with an average radiant at 46° + 57°, which nearly corresponds with the best determinations for the emanating centre of this shower.

W. F. DENNING.

Bristol, August 25.

Sonorous Sand in Dorsetshire.

IT may be interesting to know that I have discovered the existence of "musical" sand on the sea-beach at a spot between Studland Bay and Poole Harbour.

This sand, though not emitting sounds quite so loud as those produced in the Eigg sand, answers all the usual tests, and gives out a distinct note when walked upon or when agitated by the hand or a stick.

Briefly, I may state that I have been investigating the phenomenon for the last two years, and that an examination of this Dorsetshire sand gives fresh evidence in support of my theory (shortly to be published) as to the cause of the sounds. I may add that I had reasons for thinking that the sand on this particular beach *ought* to be sonorous under certain favourable conditions, but that I had visited it before without success.

It is now over thirty years since Hugh Miller discovered this sand at Eigg, and up to the present instance I am not aware that it has again been found in any other part of Europe.

CECIL CARUS-WILSON.

Bournemouth, August 18.

A Column of Dust.

THE following account of a somewhat unusual phenomenon may not be uninteresting to some of your readers. As Mr. Emil Trechmann, lecturer at Bangor University, and myself were walking in the vicinity of Stockton-on-Tees on Sunday last, about half-past one o'clock, we observed a small column of dust to rise suddenly on the road about 40 or 50 yards in front of us. There was not a breath of wind stirring at the time, yet it was evidently raised by the action of what would popularly be called a small whirlwind. This column of dust moved quickly across the road, ceasing when it reached the other side; and had the incident terminated there, we should doubtless have exhibited a passing surprise and have forgotten about it. Fortunately, however, there was a hay-field on the other side of the road, and we presently saw several large wisps of hay lifted off the tops of some haycocks, to the amount of perhaps a small-sized armful, and carried across the fields for a distance of a quarter of a mile or more, at the height of 40 or 50 feet.

Trivial as the incident may seem, it was to us singularly startling and impressive, and it was easy to imagine how, in a superstitious age, such phenomena would be attributed to supernatural agency. The mind instantly recurred to stories of witches transporting haystacks through the air, and it was difficult not to believe that, with increased force of current almost anything might have been carried aloft in a similar way.

The atmosphere remained perfectly undisturbed for at least five minutes after the occurrence, when a single "sough" of wind passed by, and it then resumed its former stillness. The general aspect of the weather was somewhat thunderous, though it remained fine until night.

HUGH TAYLOR.

20 Fraser Terrace, Gateshead-on-Tyne, August 22.

THE INTERNATIONAL GEOLOGICAL CONGRESS.

EXACTLY ten years have passed since the International Geological Congress held its first meeting It was on the 29th of August, 1878, that the Congress was inaugurated at the Palace of the Trocadéro in Paris; this meeting having been the direct result of a suggestion made by the American Association for the Advancement of Science at Buffalo, on the close of the Philadelphia Exhibition of 1876. A Committee was then formed, with Prof. James Hall, of Albany, as President, and Dr. Sterry Hunt as Secretary, for the purpose of organizing an International Congress of Geologists to be held in Paris during the Universal Exhibition in 1878. The prime object of the Congress was to discuss, and if possible settle, questions of geological classification and nomenclature, and to formulate rules for securing uniformity in geological cartography. The original American Committee—*Comité fondateur*—applied in due course to the Geological Society of France for assistance in carrying their suggestions into effect, and an influential organizing Committee was formed in Paris, under the presidency of Prof. Hébert. By the action of this Committee the arrangements were carried to a successful issue. The Paris Congress numbered 304 members; it appointed Committees for the unification of stratigraphical and palæontological nomenclature, and for systematizing the colours and signs on geological maps. Ultimately its proceedings were published in a *Compte rendu* of 313 pages.

After an interval of three years, the Congress held its second session. This was in Bologna, under Prof. Capellini as President. One of the chief results of this meeting was the nomination of a Committee for the purpose of preparing an International Geological Map of Europe, on a scale of 1 to 1,500,000. On this Committee, as at present constituted, Germany is represented by Prof. Beyrich and M. Hauchecorne, France by M. Daubrée, Great Britain by Mr. Topley, Austria-Hungary by M. Mojsisovics, Italy by M. Giordano, Russia by M. Karpinsky, and Switzerland by Prof. Renevier. The Report of the Bologna meeting was issued as a handsome volume of 660 pages.[1]

As the meetings of the Congress are triennial, the next gathering was due in 1884, but an outbreak of cholera on the Continent rendered it advisable to postpone the session for another year. It was therefore in 1885 that the Congress assembled for the third time—Berlin being the place of meeting, and Prof. E. Beyrich the President. The meeting was eminently successful, but it is to be regretted that no official volume, containing a full report of the proceedings, has yet been published.

Three years have again passed, and the Congress is about to hold its fourth session. London has been selected as the meeting-place, and by permission of the Senate of the University of London the sittings will be held in the University buildings in Burlington Gardens. The first general assembly of the Congress will take place in the theatre of the University at 8 o'clock on Monday evening, September 17, when the inaugural address will be delivered in French by Prof. Prestwich, as President. French is the official language of the Congress, but considerable latitude is allowed in the discussions, and much English and German will probably be spoken at the forthcoming meetings.

On Tuesday morning the Congress will meet at 10 o'clock, for the purpose of discussing questions bearing upon geological nomenclature and classification. A full and valuable Report on these subjects will be presented by the American Committee. This Report, which has been printed in advance, forms a volume of 220 pages, edited by Prof. Persifor Frazer. Although written

[1] For report of the Bologna Congress see NATURE, vol. xxv. p. 34.

in English, a French abstract has been prepared by Prof. Dewalque, the Secretary of the General Committee on Unification of Nomenclature; and copies of this abstract will be distributed at the meeting. The English Committee, under the presidency of Prof. T. McK. Hughes, will also present its revised Report, which is now being printed, and forms a substantial work.

Opportunity will be given on Tuesday afternoon for visiting the British Museum, where the fine collections illustrative of prehistoric archæology will be examined under the guidance of Mr. A. W. Franks.

On Wednesday morning the sitting will be occupied with the discussion of a subject which has of late years been warmly debated in geological circles—the nature and origin of the crystalline schists. Special authorities on this subject have been invited to contribute short memoirs which have been printed in advance. As copies of these papers will be distributed to the members, the communications may be taken as read and the time of the meeting occupied only in their discussion. The volumes of papers entitled "Études sur les Schistes Crystallins," contains the following communications:—"Les Schistes Cristallins," by Dr. Sterry Hunt; "Zur Klassification der krystallinischen Schiefer," by Prof. A. Heim, of Zürich; "Sur la Constitution et la Structure des Massifs de Schistes Cristallins des Alpes Occidentales," by Prof. C. Lory, of Grenoble; "Bemerkungen zu einigen neueren Arbeiten über krystallinisch-schiefrige Gesteine," by Prof. J. Lehmann, of Kiel; "Sur l'Origine des Terrains Cristallins Primitifs," by M. Michel-Lévy, of Paris; "The Archæan Geology of the Region North-West of Lake Superior," by A. C. Lawson, of the Geological Survey of Canada; "On the Crystalline Schists of the United States and their Relations," by various members of the United States Geological Survey; and a paper by M. K. A. Lossen, of the Geological Survey of Prussia. The group of papers contributed by the United States Survey contains first an "Introduction," by Major J. W. Powell, the Director, followed by a paper on "The Crystalline Schists of the Lake Superior District," by the late R. D. Irving, and T. Chamberlin and C. R. Van Hise; this is succeeded by a sketch of "The Crystalline Schists of the Coast Ranges of California," by G. F. Becker, and a brief description of "The Crystalline Rocks of Northern California and Southern Oregon," by Captain C. E. Dutton.

Wednesday afternoon will be devoted to a visit to the Natural History Department of the British Museum where the visitors will be received by Prof. Flower, as Director of the establishment.

On Wednesday evening the Congress will be received by Dr. A. Geikie, as Director-General of the Geological Survey, at the Museum of Practical Geology in Jermyn Street. With the view of illustrating the subjects that will have been discussed at the morning sitting, it is proposed that during the evening a series of microscopic sections showing the structure of the crystalline schists shall be exhibited on the screen, by means of the lime-light, in the theatre of the Museum.

At 10 o'clock on Thursday morning the Congress will re-assemble in the University theatre, and proceed to the discussion of questions bearing upon the International Map of Europe. The Map Committee will present its Report, and exhibit specimen sheets illustrating the character of the work. In the afternoon the members will make excursions in various directions. One party will visit Windsor and Eton, where they will be entertained by the masters of Eton College; another party will visit Kew, and be received by Mr. Thiselton Dyer, as Director of the Royal Gardens; other members will go down the river to Erith and Crayford for the purpose of examining the brick-earths and gravels of the Thames valley; while others will probably visit Watford and St. Albans.

At the meeting on Friday morning the discussion on nomenclature and classification, and on the coloration of maps, will be resumed. In the evening there will be a reception at the rooms of the Geological Society at Burlington House, by Dr. W. T. Blanford, as President of the Society. An evening reception, the date of which is not yet fixed, will also be held by Prof. Prestwich, the President of the Congress. The concluding business of the Congress, mostly of a formal character, will be taken at Saturday morning's sitting.

By permission of the Council of the Zoological Society, the Society's Gardens in Regent's Park will be open free to members of the Congress, not only during the week of the meetings but (after 1 o'clock) on Sundays, September 16 and 23.

Several geological excursions have been organized for the week following the London session. One of these, which promises to be extremely popular, is to the Isle of Wight, under the direction of Messrs. W. Whitaker, J. Starkie Gardner, A. Strahan, and H. Keeping. By invitation of Sir Charles Wilson, this party will also visit the offices of the Ordnance Survey at Southampton. Another interesting excursion is to North Wales under Dr. H. Hicks, assisted by Prof. J. H. Blake for Anglesey, and by Mr. G. H. Morton for the Carboniferous Limestone of Llangollen. A third excursion is planned to East Yorkshire, under the direction of Mr. J. W. Woodall and Mr. C. Fox-Strangways, assisted by Mr. W. H. Hudleston, for some of the Oolitic series. Mr. G. H. Lamplugh for the Flamborough Chalk, and Mr. Hugh Bell, for the mines and iron-works of Middlesborough. West Yorkshire will also be visited by a party under the guidance of Mr. J. E. Marr and Mr. R. H. Tiddeman. Finally, an excursion to East Anglia has been organized under Mr. F. W. Harmer (Mayor of Norwich) and Mr. Clement Reid, assisted for the older Pliocene beds of Suffolk by Dr. J. E. Taylor, of the Ipswich Museum. A guide-book containing geological descriptions of the localities about to be visited, written in French and illustrated by coloured geological maps, is in course of preparation, and will be presented to the members of the Congress. To this guide-book Mr. Topley has contributed a sketch of the geology of the various railway routes by which foreigners will reach London.

The great interest taken in the forthcoming meeting is attested by the fact that already between 500 and 600 members have been registered. The list includes nearly all the most distinguished geologists on the Continent and in America, many of whom will arrive in time to be present at the Bath meeting of the British Association during the week preceding the opening of the Congress. It is known that many of these geologists will bring with them collections of minerals, rocks, and fossils, for exhibition in the temporary Museum which will be formed in the library of the University of London, and which promises to be one of the most interesting features of the meeting. On the whole, there can be no question that the success of the forthcoming session of the Congress is abundantly assured.

MODERN VIEWS OF ELECTRICITY.[1]

PART IV.—RADIATION.

X.

HAVING now described a possible method of measuring the velocity of electric wave propagation, and therefore of at least the ratio of the two ethereal constants k and μ, by an experiment on the different parts of one enormously large and properly chosen circuit: return to the consideration of the ordinary small discharging Leyden jar or other alternating current circuit of a moderate size, it may be a few yards or a foot or an inch in diameter.

If the alternating currents are produced artificially by

[1] Continued from p. 393.

some form of alternating machine, their frequency is, of course, arbitrary; but if they be automatically caused by the recoil of a given Leyden jar in a given circuit, their frequency is, as we have already said,

$$\frac{1}{2\pi\sqrt{(LS)}} \text{ per second};$$

where L is the electrical inertia or self-induction of the circuit, and where S is the capacity or reciprocal of the elasticity constant of the jar.

It is not convenient here to go into the determination of the quantity L, but roughly one may say that for an ordinary open single-loop circuit it is a quantity somewhat comparable with twelve or fifteen times its circumference multiplied by the constant μ.

The value of S has to do with the area and thickness of the condenser, being, as is well known, $\frac{A}{4\pi z}$ multiplied by the constant K.

The product LS contains therefore two factors, each of linear dimensions, expressing the sizes of circuit and jar, and likewise contains a factor μK expressing the properties of the surrounding medium. Hence, so far as the ether is concerned, the above expression for frequency of vibration demands only a knowledge of the *product* of its two constants K and μ, and since this is known by the previous velocity experiment, it is easy to calculate the rate of oscillation of any given condenser discharge. It is also easy to calculate the wave-length; for if there are n waves produced per second, and each travels with the velocity v, the length of each wave is $\frac{v}{n}$.

Hence the wave-length is $2\pi\sqrt{\left(\frac{L}{\mu}\cdot\frac{S}{K}\right)}$.

Now, if we go through these numerical calculations for an ordinary Leyden jar and discharger, we shall find waves something like, say, 50 or 100 yards long. They may plainly be of any length, according to the size of the jar and the size of the circuit. The bigger both these are the longer will be the waves.

A condenser of 1 microfarad capacity, discharging through a coil of self-induction 1 secohm, will give rise to ether waves 1900 kilometres or 1200 miles long.

A common pint Leyden jar discharging through a pair of tongs may start a system of ether waves each not longer than about 15 or 20 metres.

A tiny thimble-sized jar overflowing its edge may propagate waves only about 2 or 3 feet long.

The oscillations of current thus recognized as setting up waves have only a small duration, unless there is some means of maintaining them. How long they will last depends upon the conductivity of the circuit; but even in a circuit of infinite conductivity they must die out if left to themselves, from the mere fact that they dissipate their energy by radiation. One may get 100 or 1000, or perhaps even 100,000, perceptible oscillations of gradually decreasing amplitude, but the rate of oscillation is so great that their whole duration may still be an extremely small fraction of a second. For instance, to produce ether waves a metre in length requires 300,000,000 oscillations per second.

To keep up continuous radiation naturally requires a supply of energy, and unless it is so supplied the radiation rapidly ceases. Commercial alternating machines are artificial and cumbrous contrivances for maintaining electrical vibrations in circuits of finite resistance, and in despite of loss by radiation.

In most commercial circuits the loss by radiation is probably so small a fraction of the whole dissipation of energy as to be practically negligible; but one is, of course, not limited to the consideration of commercial circuits or to alternating machines as at present invented and used. It may be possible to devise some less direct method—some chemical method, perhaps—for supplying energy to an oscillating circuit, and so converting what would be a mere discharge or flash into a continuous source of radiation.

So far we have only considered ordinary practicable electrical circuits, and have found their waves in all cases pretty long, but getting distinctly shorter the smaller we take the circuit. Continue the process of reduction in size further, and ask what sized circuit will give waves 6000 tenth-metres (three-fifths of a *micron*, or 25 millionths of an inch) long. We have only to put $2\pi\sqrt{\left(\frac{L}{\mu}\cdot\frac{S}{K}\right)} =$ 0·00006, and we find that the necessary circuit must have a self-induction in electro-magnetic units, and a capacity in electrostatic units, such that their geometric mean is 10^{-5} centimetre (one-tenth of a micron). This gives us at once something of atomic dimensions for the circuit, and suggests immediately that those short ethereal waves which are able to affect the retina, and which we are accustomed to call "light," may be really excited by electrical oscillations or surgings in circuits of atomic dimensions.

If after the vibrations are once excited there is no source of energy competent to maintain them, the light production will soon cease, and we shall have merely the temporary phenomenon of phosphorescence; but if there is an available supply of suitable energy, the electrical vibrations may continue, and the radiation may become no longer an evanescent brightness, but a steady and permanent glow.

Velocity of Electrical Radiation compared with Velocity of Light.

We have thus imagined the now well-known Maxwellian theory of light, viz. that it is produced by electrical vibrations, and that its waves are electrical waves.

But what justification is there for such an hypothesis beyond the mere fact which we have here insisted on, viz. that waves in all respects like light-waves except size, *i.e.* transverse vibrations travelling at a certain pace through ether, can certainly be produced temporarily in practicable circuits by familiar and very simple means, and *could* be produced of exactly the length proper to any given kind of light if only it were feasible to deal with circuits ultra-microscopic in size? The simplest point to consider is: Does light travel at the same speed as the electrical disturbances we have been considering? We described one method of measuring how fast electrical radiation travels in free space, and there are many other methods: the result was 300,000 kilometres per second.

Methods of measuring the velocity of light have long been known, and the result of those measurements in free space or air is likewise 300,000 kilometres a second. The two velocities agree in free space. Hence surely light and electrical radiation are identical.

But there is a further test. The speed of electrical radiation was not the same in all media: it depended on the electrical elasticity and the ethereal density of the transparent substance; in other words, it was equal to the reciprocal of the geometric mean of its specific inductive capacity and its magnetic permeability—

$$v = \frac{1}{\sqrt{(K\mu)}}.$$

Now, although the absolute value of neither K nor μ is known, yet their values relatively to air are often measured and are known for most substances.

Also, it is easy to compare the pace at which light goes through any substance with its velocity in free space: the operation is called finding the refractive index of a substance. The refractive index means, in fact, simply the ratio of the velocity of light in space to its velocity in the given substance. The reciprocal of the index of refraction is therefore the relative velocity of light. Calling the index of refraction n, therefore, we ought, if the electrical theory of light be true, to find that $n^2 = K\mu$; or that the index of refraction of any substance is the geometric mean of its electrostatic and magnetic specific capacities.

That this is precisely true for all substances cannot at present be asserted. There are some substances for which it is very satisfactorily true: there are others which are apparent exceptions. It remains to examine whether they are not only apparent but real exceptions, and, if so, to what their exceptional behaviour is due.

It must be understood what the essential point is. It has been proved by various methods, and with greater approach to exactness as the accuracy of the methods is improved, that electrical disturbances—such as the long waves emitted by any alternating machine—travel through air or free space with exactly the same velocity as light; in other words, that there is no recognizable difference in speed between waves several hundred miles long and waves so small that a hundred thousand of them can lie in an inch. This is true in free ether, and it is a remarkable fact. If it proves anything concerning the structure of the ether, it proves that it is continuous, homogeneous, and simple beyond any other substance; or at least that if it does possess any structural heterogeneity, the parts of which it is composed are so nearly infinitesimal that a hundred miles and the hundred-thousandth of an inch are quantities of practically the same order of magnitude so far as they are concerned: its parts are able to treat all this variety of wave-length in the same manner.

But directly one gets to deal with ordinary gross matter we know that this is certainly not the case. Ordinary matter is composed of molecules which, though small, are far from being infinitesimal. Atoms are much smaller than light-waves, indeed, but not incomparably smaller. Hence it is natural to suppose that the ether as modifed by matter will be modified in a similarly heterogeneous manner; and will accordingly not be able to treat waves of all sizes in the same way.

The speed of all waves is retarded by entering gross matter, but we should expect the smallest waves to be retarded most. The phenomenon is well marked even within the range of such light-waves as can affect the retina: the smaller waves—those which produce the sensation of blue—are more retarded, and travel a little slower, through, say, glass or water, than the somewhat larger ones which produce the sensation of red. This phenomenon has long been known, and is called dispersion. Hence it is not easy to say at what rate waves a few inches or a few yards or miles long ought to travel, by merely knowing at what rate the ultra-microscopic light-waves travel.

But there is even more to be said than this. There is not only dispersion, there is selective absorption possessed by matter: not only does it transmit different-sized waves at different rates, but it absorbs and quenches some much faster than others. Few substances, perhaps none, are equally transparent to all sizes of waves. Glass, for instance, which transmits readily the assortment of waves able to affect the retina, is practically quite opaque to waves a few hundred times longer or shorter. And whenever this selective absorption occurs, the laws of dispersion are extraordinary—so extraordinary that the dispersion is often spoken of as "anomalous"; which of course means, not that it is lawless, but that its laws are unknown. Dispersion in any case is an obscure and little understood subject, but dispersion modified by selective absorption is still worse. Until the theory of dispersion is better understood, no one is able to say at what speed waves of any given length ought to travel. One can only examine experimentally at what rate they *do* travel. This has been done for long electrical waves, and it has been done for short light-waves: in the case of some substances the speed is the same, in the case of others it is different. But that the speed should be different is, as I have now explained, very natural, and can by no means be twisted into an admission that light-waves and electrical waves are not essentially identical. That the speed of both should agree at all is noteworthy; the agreement appears to be exact in air, and practically exact in such simple substances as sulphur, and in the class of hydrocarbons known as paraffins; whereas in artificial substances like glass, and in organic substances like fats and oils, the agreement is less perfect.

So much for the vital question of the speed at which electrical and optical disturbances travel. In some cases the speeds are accurately the same, in no case are they entirely different; and in those cases where the agreement is only rough, an efficient and satisfactory explanation of the difference is to hand in the very different lengths of wave which have at present been submitted to experiment. To compare the speeds properly, we must either learn to shorten electrical waves, or to lengthen light-waves, or both, and then compare the two things together when of the same size.

It cannot be seriously doubted that they will turn out identical.

Manufacture of Light.

The conclusions at which we have arrived, that light is an electrical disturbance, and that light-waves are excited by electric oscillations, must ultimately, and may shortly, have a practical import.

Our present systems of making light artificially are wasteful and ineffective. We want a certain range of oscillation, between 7000 and 4000 billion vibrations per second: no other is useful to us, because no other has any effect on our retina; but we do not know how to produce vibrations of this rate. We can produce a definite vibration of one or two hundred or thousand per second, in other words, we can excite a pure tone of definite pitch; and we can command any desired range of such tones continuously by means of bellows and a keyboard. We can also (though the fact is less well known) excite momentarily definite ethereal vibrations of some million per second, as I have at length explained; but we do not at present seem to know how to maintain this rate quite continuously. To get much faster rates of vibration than this we have to fall back upon atoms. We know how to make atoms vibrate: it is done by what we call "heating" the substance, and if we could deal with individual atoms unhampered by others, it is possible that we might get a pure and simple mode of vibration from them. It is possible, but unlikely; for atoms, even when isolated, have a multitude of modes of vibration special to themselves, of which only a few are of practical use to us, and we do not know how to excite some without also the others. However, we do not at present even deal with individual atoms; we treat them crowded together in a compact mass, so that their modes of vibration are really infinite.

We take a lump of matter, say a carbon filament or a piece of quick-lime, and by raising its temperature we impress upon its atoms higher and higher modes of vibration, not transmuting the lower into the higher but superposing the higher upon the lower, until at length we get such rates of vibration as our retina is constructed for, and we are satisfied. But how wasteful and indirect and empirical is the process. We want a small range of rapid vibrations, and we know no better than to make the whole series leading up to them. It is as though, in order to sound some little shrill octave of pipes in an organ, we were obliged to depress every key and every pedal, and to blow a young hurricane.

I have purposely selected as examples the more perfect methods of obtaining artificial light, wherein the waste radiation is only useless, and not noxious. But the old-fashioned plan was cruder even than this, it consisted simply in setting something burning: whereby not only the fuel but the air was consumed, whereby also a most powerful radiation was produced, in the waste waves of which we were content to sit stewing, for the sake of the minute, almost infinitesimal, fraction of it which enabled us to see.

Everyone knows now, however, that combustion is not a pleasant or healthy mode of obtaining light; but everybody does not realize that neither is incandescence a satisfactory and unwasteful method which is likely to be practised for more than a few decades, or perhaps a century.

Look at the furnaces and boilers of a great steam-engine driving a group of dynamos, and estimate the energy expended; and then look at the incandescent filaments of the lamps excited by them, and estimate how much of their radiated energy is of real service to the eye. It will be as the energy of a pitch-pipe to an entire orchestra.

It is not too much to say that a boy turning a handle could, if his energy were properly directed, produce quite as much real light as is produced by all this mass of mechanism and consumption of material.

There might, perhaps, be something contrary to the laws of Nature in thus hoping to get and utilize some specific kind of radiation without the rest, but Lord Rayleigh has shown in a short communication to the British Association at York[1] that it is not so, and that therefore we have a right to try to do it.

We do not yet know how, it is true, but it is one of the things we have got to learn.

Anyone looking at a common glow-worm must be struck with the fact that not by ordinary combustion, nor yet on the steam-engine and dynamo principle, is that easy light produced. Very little waste radiation is there from phosphorescent things in general. Light of the kind able to affect the retina is directly emitted, and for this, for even a large supply of this, a modicum of energy suffices.

Solar radiation consists of waves of all sizes, it is true; but then solar radiation has innumerable things to do besides making things visible. The whole of its energy is useful. In artificial lighting nothing but light is desired; when heat is wanted it is best obtained separately, by combustion. And so soon as we clearly recognize that light is an electrical vibration, so soon shall we begin to beat about for some mode of exciting and maintaining an electrical vibration of any required degree of rapidity. When this has been accomplished, the problem of artificial lighting will have been solved. OLIVER J. LODGE.

(To be continued.)

[1] B.A. Report, 1881, p. 526.

STORM WARNINGS.

TWENTY-EIGHT years ago, M. Le Verrier wrote to the Astronomer-Royal at Greenwich inviting the co-operation of this country in his scheme for giving warning of storms by announcing them and following their course by telegraph as soon as they appear at any point of Europe, and in the following year (1861) Admiral FitzRoy established his system, giving notice of storms before they actually strike our coast. Notwithstanding the success which has attended these efforts, storms sometimes overtake us before warning of their approach can be given, and every endeavour to increase our foreknowledge of their movements should be gladly welcomed. Since the year 1860 much additional light has been thrown upon the subject by the systematic publication of synchronous charts, such as those commenced by the late Captain Hoffmeyer, Director of the Danish Meteorological Institute. Several attempts have also been made to utilize the Atlantic cables with the object of giving warning of storms leaving the American coast or met with by the fast steamers bound to the United States; but these efforts have hitherto met with little success from want of sufficient knowledge of the conditions existing over the Atlantic, many storms passing wide of the British Isles, others originating in mid-ocean or dying out there. Of the endeavours to connect our knowledge of the weather over the Atlantic with the reports received from the two shores, the labours of Captain Hoffmeyer as explained in "Études sur les Tempêtes de l'Atlantique septentrional" (Copenhagen, 1880), and recent publications of M. Léon Teisserenc de Bort in the *Annales* of the Central Meteorological Office of France, deserve especial attention. With the view of utilizing the American weather reports for the purpose of improving European weather predictions, M. de Bort has made an investigation of the mean positions of high and low pressures in the northern hemisphere for all winters since 1838, and he shows how these great centres of atmospheric action correspond to different types of weather, that during each season these centres are limited in number, and that each of them when displaced still lies within a definite area.

During the winter season, for instance, the maxima are arranged as follows:—(1) The maximum of Asia, which generally includes two parts, one being near Irkutsk, the other either in Siberia or Russia, one of the positions being usually to the south-west of Tobolsk. (2) The maximum of Madeira, which is also sometimes split up into two parts, one being over the ocean and the other over Switzerland and Central Europe, or joining with a part of the high pressures of Asia. (3) The Bermuda maximum, which is often found over the east of the United States or even in the neighbourhood of Nova Scotia. (4) The continental maximum of America, which usually lies over the mountainous parts. (5) The Polar maximum, which appears over Greenland, Iceland, or Norway. With respect to the minima, there are (1) the low pressure situated over the north of the Atlantic, which may be called the Iceland minimum; (2) a minimum which is mostly to be found in America, generally over the region of the Great Lakes; and (3) a minimum which appears to belong to the Arctic Ocean, and whose centre generally lies near Nova Zembla. These mean positions are laid down in recent barometrical charts, such as those of the Meteorological Office and other institutions. The maxima and minima may combine respectively, but there are scarcely any conditions where at least three centres of high pressure and two centres of low pressure are not to be found between China and California, and between the equator and 80° N. latitude. When the positions of the high and low pressures are well known, we may proceed like the naturalist, and discover, by the examination of some portions, those that are wanting to the whole. The introduction of this method into meteorology has a direct application in the prediction of weather. The telegraphic reports now received allow of the construction on one hand of the isobaric chart over Europe (which ought to be extended as far as Asia), and the isobars in their general features over the United States; between the two there remains the great unknown of the Atlantic. Now by a methodical discussion of the isobars of the two shores of the ocean we ought to be able to reconstitute the conditions over the Atlantic with a great amount of probability. But how are we to know that there are not two or three centres of depression over the ocean, for the number of depressions is not limited? Evidently this is very difficult; but for the object in view—viz. to reconstruct the general features of the isobars with sufficient accuracy to make use of the data for forecasting the weather in Europe—the difficulty is not so great. In fact, either the depressions are grouped so as to be only the subsidiary disturbances of a great minimum, and in this case the position of the minimum may be indicated, which is the important point, or they are completely separated, forming distinct systems of low pressure, and then the trace of them is found in the isobars on the coasts, and often even in the arrangement or the number of the maxima situated over the Continent.

The essential condition for successfully constructing the isobars is a knowledge of the various types that present themselves, so that we may discover by a simple inspection of the charts of Europe and America what general type is in question. We will give two examples in which M. de Bost shows that the reconstruction of the general chart according to this method would have enabled him to foretell two important storms.

On December 2, 1886, the general conditions were as shown in Chart No. 1.

"By confining our attention to the indications given solely by the chart of Europe," he writes, "we might expect in France cold weather with cloudy sky and sleet showers. On the 4th, a depression which was foretold on the 3rd had spread over the British Isles, where it brought bad weather; but the barometer rose rapidly over the west of Europe. Supposing that the high pressures would extend over Central Europe, we might expect a spell of fine and cold weather. Instead of that a rapid fall of the barometer occurred over the north of Europe, and the wind shifted to south-west. On the 7th and 8th this condition was intensified, and one of the most violent storms that we have experienced for a long time struck us, the barometer at Mullaghmore falling to 27·45 inches."

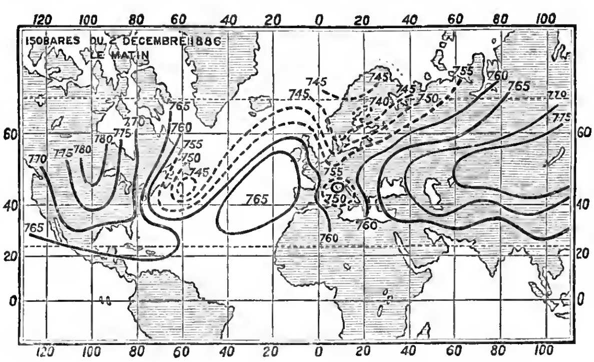

CHART No 1.—762 mm. = 30·0 inches.

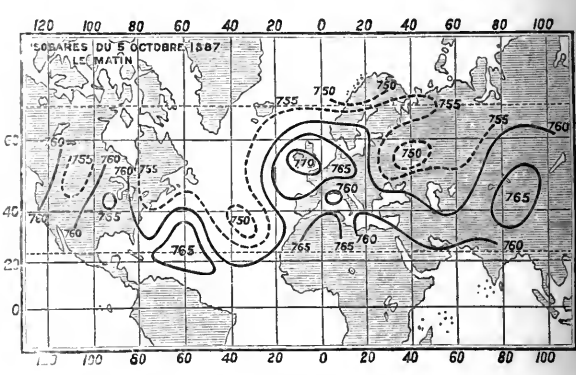

CHART No. 2.

If we refer to the general charts, and particularly to the chart of December 2, we shall see that the barometric maximum of Asia is in its place, and that over the United States there is a large area of high pressure. This is rather extended, and leaves no room for the low pressure of the region of the Lakes except in the vicinity of Newfoundland. As there is also a rather important barometrical maximum to the west of Europe, we conclude that very important low pressures must exist over the ocean off the American coast. Under these conditions, in order to have fine weather, it is necessary that the area of low pressure of Central Europe should shift towards Siberia, so as to allow the maximum to advance over our regions. On the 5th, the increase of the high pressure of Asia towards St. Petersburg and Finland clearly indicates that the area of low pressure cannot shift in its entirety towards the East. The rise of the barometer over the west of Europe must not therefore be taken as a sign of lasting fine weather, but as the result of the approach of the low pressure from the ocean. From these conditions M. de Bort shows that the forecast to be drawn for Western Europe was entirely opposite to that which resulted from the study of the conditions over Europe alone.

Another example of the utility of the construction of the charts over the ocean is afforded by the very sudden change of weather that occurred on the 8th of October, 1887 (Chart No. 2).

The European weather charts indicate at the beginning of the month the existence of an area of high pressure over the west of Europe, and it appears to extend to some distance over the ocean; low pressures are observed over the north of the Continent. This condition persists, with rather cold weather, and on the 7th, in France, a continuation of dry weather is foretold. In England, the forecast bears principally on the consequences of the movement of a small barometric minimum which exists over Scotland.

On the 8th a complete change of system occurred; an important depression reached Europe over Portugal, and the low pressure extended over the north of the British Isles. This depression did not fail to bring a storm from the north-east. While the situation in Europe was considered as fairly stable, and the low pressures of the north of Europe were considered to be chiefly in operation, an important minimum which was advancing from the central part of the ocean suddenly appeared, and produced a complete change of conditions. And yet the predictions of the European meteorologists were certainly the only ones that could have been made from the study of the various daily weather reports. But if we construct the chart of the 5th of October, we shall recognize that a centre of low pressure was very close to the south-west of Spain, and was directly threatening Europe. A mere glance at the Chart No. 2 would have been sufficient to change entirely the weather forecasts in Western Europe, and would have given ample warning of the approach of the storm of the 8th.

As to the reasons which would allow us to trace the isobars over the Atlantic in the way they are represented on the chart; the importance of the barometric maximum situated over the British Isles and the west of Europe is such that we must infer from it that the normal maximum of Madeira was displaced. This conclusion was confirmed by the observations from Madeira, where the barometer was below 29·9 inches, with a south-east wind. Secondly, there were no important low pressures in America, therefore these must exist over the ocean and near Europe, as the observations from Nova Scotia show higher pressures than those from Canada. Everything concurs, therefore, in indicating with certainty the presence of a large barometric minimum over the centre of the ocean, and the importance of this indication for the prediction of the weather in Europe cannot be contested.

From these examples M. de Bort concludes—(1) that, with the aid of telegraphic reports from America, and the knowledge of what is taking place over Europe and Siberia, we can trace the isobars over the ocean with much chance of success; (2) this trace being made, we may take useful advantage of it to reveal the true character of the general condition of the atmosphere, which our charts, limited to Europe and the British Isles alone, are powerless to indicate.

SONNET*

Commemorative of an Incident which occurred in St. Margaret's Church, Westminster, on August 9, 1888.

BEFORE the altar Man and Maid they stood,
On altar-step as Man and Wife then kneeled—
Its Heaven-lent strains the sacred Organ pealed,
Moulding thoughts, hopes, and passions as it would,
Till all hearts swam in one melodious flood.
When as rapt Fancy wandered far a-field
Lo! Eve's fresh bower stood to her sight revealed
Where hung upon the spray a pure white bud—
The bride's half-sister on her nurse's breast—
Fair-writ indenture of prevenient Mind,
An Imogene stole back from far Dream-land
(Spirit of womanhood by a child possessed!)
O'er whose soft gaze, as Ocean deep, inclined
My lips with reverence, kissed her dimpled hand.

New College, Oxford, August 26. J. J. S.

* This sonnet would furnish an unrivalled new situation and a noble subject for a young painter with lofty aims (if such there be among us) to depict. In the church invited to participate in the sacred rite were Star-gazers, Wonder-workers, and Magi (the Darwins, the Thomsons, and the Cayleys), who may be supposed in the person of their representative to be doing homage to the Spirit of Womanhood incarnated in the infant held in the arms of her proud and comely nurse, from whom I learned that the child's name was Imogene.

NOTES.

PROF. PIAZZI SMYTH has resigned the office of Astronomer-Royal for Scotland, and no one who takes the trouble to read the second appendix to a paper on "The Edinburgh Equatorial in 1887," contributed by him lately to the Proceedings of the Royal Society of Edinburgh, and now reprinted, will be surprised at his decision. Before he consented to join in the project of the Board of Visitors, about 1870, of applying to the Government for a large equatorial, Prof. Piazzi Smyth pointed out that such an instrument, even if once set up complete, would require further expenditure year after year to keep it fully efficient, and that the working with it would be so peculiarly onerous and responsible that the salaries of the officers of the Royal Observatory, Edinburgh, already acknowledged to be at, or below, starvation point, should be raised more nearly to the level of those of other Observatories or of any ordinary Government offices. He was told that all that was most certainly right, and would be brought about; and the Board of Visitors did most honourably proceed to frame a scheme providing for a modest addition not only to the observers' salaries, but to the available income of the Observatory, to be expended by the Astronomer in instrumental repairs, experiments, and improvements at his discretion. Under these promising circumstances he acted with the Board in their application for a large equatorial. The instrument was in part set up, under the authority of the Office of Works, in 1872; but in the following year, when the election was found very incomplete, the scheme of the Board of Visitors for increasing the salaries and available income of the Observatory to a point sufficient to finish, maintain, and work the instrument, was suddenly and finally disallowed. A Committee, appointed in 1876 by Mr. (now Lord) Cross to investigate the matter, reported for a series of financial improvements similar to those suggested by the Board of Visitors; but the Home Secretary declined to listen to his own Committee. Another Committee was appointed in 1879. This Committee did not admit the Astronomer to its council, and limited its inquiries to the equatorial. It advised certain improvements, and obtained a grant for executing them; but the grant either still remains in the possession of the Office of Works or has lapsed to the Treasury—a fact which is the less to be regretted as the sum was absurdly inadequate. It would be hard to match this wretched tale in any other civilized country; and we can scarcely expect that science will continue to flourish in Great Britain if its claims are to be treated with so much contempt. Prof. Piazzi Smyth, having withdrawn from his position at Edinburgh, retires to Clova, Ripon, where he will continue his astronomical studies. Warm appreciation of his services during the forty-three years in which he has held office has been expressed on behalf of the Secretary for Scotland, and by the Senatus Academicus of the University of Edinburgh.

THE British Archæological Association, of which the Marquess of Bute is this year the President, began its sittings in Glasgow on Monday. Although this is the forty-fifth annual Congress, it is the first occasion on which the Association has crossed the border.

THE Association of Public Sanitary Inspectors of Great Britain held on Saturday, by invitation of the Mayor (Alderman Martin), a public conference on sanitation in the Royal Pavilion, Brighton. An address by Mr. Edwin Chadwick, the President, who could not be present on account of his advanced age, was read by Dr. B. W. Richardson. The address presented an interesting general view of the recent progress of sanitation in this country.

MR. WILLIAM CHAPPELL, F.S.A., who died on the 20th inst. at his residence in Upper Brook Street, was known chiefly for his efforts to popularize old English music; but he deserves to be remembered also as an ardent student of music in its scientific aspects. He had a wide and accurate knowledge of the natural laws on which the principles of musical composition are based, and his book on the History of Music, both as an Art and as a Science, is of great value. Mr. Chappell was seventy-eight years of age.

MR. P. H. GOSSE, F.R.S., the well-known zoologist, died at his residence, St. Marychurch, Torquay, on the 23rd inst., at the age of seventy-eight. Mr. Gosse was elected a Fellow of the Royal Society in 1856.

MR. WILLIAM A. CROFFUT has been appointed executive officer of the United States Geological Survey, in the place of the late Mr. James A. Stevenson. Mr. Croffut is a well-known journalist, and *Science* anticipates that he will fill with success the difficult position in which he is placed.

CAPTAIN H. FABRITIUS, of the Norwegian Hydrographical Office, is engaged during the present summer in the steamer *Professor Hansteen* in making hydrographical researches on the west coast of Norway, similar to those of last year. The course followed is from Malangen southwards, soundings being taken at about every mile to a distance of some sixty miles from the coast.

SEVERAL recent shocks of earthquake are reported from Norway and Sweden. In the former country an earthquake shock of great severity was felt in various parts of Hardanger, on the west coast, shortly after midnight on July 17. Houses were shaken, and furniture was thrown down. In one place three shocks were felt. The earthquake was accompanied by loud subterranean rumblings. Its area seems to have been very limited: in places only a few miles distant no trace of disturbance was perceived. On July 28, about 3 a.m., a very severe shock of earthquake was felt along a great portion of the northern Baltic shore of Sweden. At Hernösand, Örnsköldsvik, and Lungön, the shocks are reported as particularly severe, houses shaking, &c. In some places two or three shocks were felt, lasting, so correspondents maintain, several minutes. In every place loud subterranean detonations were heard. Again, on the evening of August 17, during a hurricane, a severe earthquake shock was felt in the neighbourhood of Ystad in Scania, in the extreme south of Sweden.

On the morning of August 17, about 3 a.m., a remarkable phenomenon attracted attention at the Island of Rügen, in the Baltic. A deep rumbling out at sea was heard, and soon afterwards two enormous waves approached from the north-west, breaking over the shore and doing considerable damage to small craft. At the time the sea was calm, and there was no wind.

On the night of July 31 a brilliant meteor was seen at Linköping, in Sweden, going in a north-westerly direction. It finally burst, the fragments appearing to fall near the railway park.

Symons's Monthly Meteorological Magazine for August contains an interesting summary of the climate of the British Empire during 1887. Comparing with the summary for 1886, Stanley, Falkland Isles, takes the place of London, as the dampest station. Adelaide has the highest shade temperature, 111°·2; the highest temperature in the sun, 164°; and is the driest station. Winnipeg has the lowest shade temperature, −42°·7, and the greatest yearly range, 135°·9. Bombay has the greatest rainfall, and Malta the least, and also the least cloud. Although the maximum shade temperatures in Australia exceed those in India, the *average* maxima of the latter far exceed those of Australia.

The Pilot Chart of the North Atlantic Ocean for August shows that although the weather over that ocean was generally fine and very mild during July, a number of depressions were generated, and produced gales over the trans-Atlantic routes. The most violent was one which developed on June 27, in about latitude 42° and longitude 52°, reaching our coasts on July 4. A wind-force as high as 11 of the Beaufort scale was recorded during its course. Dense continuous fog was encountered over and to the westward of the Grand Banks. Large quantities of ice have been reported as far west as the 60th meridian. The tracks of all the most notable August hurricanes on record are plotted on the chart, and show where these dangerous cyclones are likely to be encountered. A supplementary chart showing the derelicts in the North Atlantic, gives also the complete history up to date of the great log raft which was abandoned last December. This most dangerous obstruction to navigation consisted of about 27,000 trunks of trees bound together, and measured 560 feet long. Thousands of the great logs of which it was composed are still drifting over the commercial routes.

In the *American Meteorological Journal* for July, Lieut. Glassford describes a new wind vane in use at the California State University. The design is said to possess novel advantages, such as supporting all the weight upon a point, like a compass-card, an oil vessel into which paddles dip to lessen the suddenness of vibration, &c. It may here be mentioned that anemometers with liquid brakes have also been made in this country. Mr. Rotch contributes an article on the Observatory on the Säntis, in Switzerland. The observations of this mountain station are regularly published in the *Annalen* of the Swiss Central Meteorological Office. Mr. F. Waldo gives an abstract of the results of comparisons of several of the combined cistern-syphon barometers, known as the Wild-Fuess check barometers. These portable instruments have been for some time in use in Russia, and some of them are now introduced into the United States Signal Service. The full account of the comparisons was prepared for the Chief Signal Officer's Report, but is not yet printed.

Dr. G. N. Stewart, Owens College, sent recently to the Royal Society of Edinburgh a preliminary communication on the electrolytic decomposition of proteids. He pointed out that it is an important question whether the conduction of electricity by animal tissues is mainly or entirely electrolytic. If it is mainly electrolytic, the further question becomes interesting, What are the electrolytes? The inquiry is thus brought into relation with the whole of electro-physiology on the one hand, and the whole of electro-therapeutics on the other, and, at the present moment, it gains special interest from the practical point of view, in connection with the recent introduction of strong currents into gynæcological treatment. The investigation is as yet far from being complete, and Dr. Stewart is at present carrying on the experiments. In the case of egg-albumen it has been found that the resistance at any given temperature is not changed by coagulation, but that it is enormously increased by dialysis. The conclusion is that it is, mainly at any rate, by the electrolysis of the simple inorganic constituents that the current passes.

A third edition of Prof. Silvanus P. Thompson's "Dynamo-Electric Machinery" (E. and F. N. Spon) has just been issued. Most of the treatise has been rewritten for this edition, and much new matter has been added.

The University College of North Wales has issued its Calendar for the year 1888–89.

In the Annual Report, for the year 1887, of the Trustees of the American Museum of Natural History, Central Park, New York City, it is stated that the collections of this Museum are now valued at the sum of about 600,000 dollars. "It is but right to say," add the Trustees, "that of this large amount your Trustees have been the main contributors. The necessity of adding to these collections increases as time goes on, and it is hoped that more of our citizens will take an earnest and increased interest in our Museum, and so aid the Trustees in making this institution what it should be and what our city has a right to expect—the great museum of the country."

In a letter written on board the seal-ship *Jason* in the Denmark Sound, Dr. Nansen draws attention to the scarcity of seals on the coast of Greenland in recent years. Only ten years ago the animals were so plentiful and tame that thousands could be "clubbed" with the greatest ease, whereas now they have become scarce and shy. Dr. Nansen is of opinion that the ruthless persecution of these animals since 1876, when the first sealer appeared in the Denmark Sound, has caused them to alter their habits. Formerly they were found on the edge of the drift-ice, where they were safe from their only enemy, the Polar bear, though falling an easy prey to the sealer. Now they gather on the ice close to the shore, whither vessels cannot penetrate, and where they are, at all events, safe from one enemy. This, says Dr. Nansen, was fully demonstrated on several occasions, particularly on July 2, when seals were seen lying in thousands close under the shore to the north and north-east as far as the eye could reach from the mast head. To the north especially the ice was for miles one mass of dark animals. Dr. Nansen advocates a closer preservation of the seal. The seal fishery was a failure this year, and sealers report that the ice-masses were enormous.

The additions to the Zoological Society's Gardens during the past week include three Black-headed Lemurs (*Lemur brunneus*) from Madagascar, presented by Captain J. Bonneville; a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Captain James Smith; a Razorbill (*Alca torda*), British, presented by Mr. T. H. Nelson; a Nightingale (*Daulias luscinia*), British, presented by Mr. J. Young; an American Wild Turkey (*Meleagris gallo-pavo* ♂) from North America, presented by Mr. F. J. Coleridge Boles; a Raven (*Corvus corax*), British, presented by Mr. F. Steinhoff; two Pallas's Sand Grouse (*Syrrhaptes paradoxus*), bred in Fifeshire, N.B., presented by Mr. Alexander Speedie; a Macaque Monkey (*Macacus cynomolgus* ♂) from India, a Lesser White-nosed Monkey (*Cercopithecus petaurista* ♀) from West Africa, a Vulpine Squirrel (*Sciurus *vulpinus*) from North America, deposited; two Great White Herons (*Ardea alba*), European, purchased; a Moor Monkey (*Semnopithecus maurus*) from Java, a Malabar Squirrel (*Sciurus maximus*) from India, a Red-bellied Squirrel (*Sciurus variegatus*) from Vera Cruz, a Sclater's Curassow (*Crax sclateri* ♀) from South America, a River Jack Viper (*Vipera rhinoceros*) from West Africa, received in exchange; a Wapiti Deer (*Cervus canadensis* ♂), five Brazilian Teal (*Querquedula brasiliensis*), two Chilian Pintails (*Dafila spinicauda*), two Triangular Spotted Pigeons (*Columba guinea*), three Chinese Blue Magpies (*Cyanopolius cyanus*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE SPECTRUM OF R CYGNI.—The Rev. T. E. Espin, in Circular No. 21 of the Wolsingham Observatory, reports that he observed a remarkably bright line (apparently F) in the spectrum of this star on August 13. The observation was confirmed on August 22, on which night Dr. Copeland also observed the bright line and determined its position. Dunér's observations of this star in 1879, 1880, and 1882 showed it as possessing a feebly-marked spectrum of the third (Secchi's) type. A change would therefore seem to have taken place in this star. Place for 1887·0, R.A. 19h. 33m. 49s., Decl. 49° 57′·0 N.

MILAN DOUBLE-STAR OBSERVATIONS.—Prof. Schiaparelli has recently published, in No. xxxiii. of the Publications of the Royal Observatory of Brera, the results of his measures of 465 systems of double stars made with the fine 8-inch Merz refractor of that Observatory in the eleven years 1875–85. The observations are nearly 4000 in number, and are for the most part of stars of small distances, *i.e.* less than 5″, apart, the binaries in rapid motion receiving especial attention. The measures are grouped together into four parts, the stars of the Dorpat and Pulkowa catalogues forming the first two, then follow stars discovered by Burnham, and those of other discoverers are grouped together in the last. Besides these detailed results of the measures made with the old 8-inch, with which Prof. Schiaparelli has done so much excellent work in the past, there are given in an appendix mean results for a number of the closest pairs as measured with the new 18-inch refractor. Prof. Schiaparelli seems well satisfied with the performance of this new instrument, and records the discovery that the principal star of Σ 1273, ε Hydræ, is itself a very close double, a fact that had hitherto escaped notice, notwithstanding the number of observations which have been made with various telescopes upon the star. The magnitudes of the two components of the new double are 4 and 5·5, and the distance is 0″·2 or 0″·25. The earlier part of the volume contains a detailed description of the optical performance of the 8-inch refractor, a discussion of the errors of the micrometer and of the accidental errors of observation, a determination of the systematic errors of observation, and a very full comparison with Dembowski's measures. The differences in the determination of position-angle due to the varying inclination of the line joining the two stars to the line of the observer's eyes are also investigated, but the reversion prism was not used. Prof. Schiaparelli finds that on the whole his measures of distance are free from systematic errors due to personality, but his position-angles have a tendency to be small as compared with those of other observers.

Amongst the notes to some of the more interesting stars is one on OΣ 285 in which a correction of 180° is suggested to the angles of Englemann and Perrotin in 1883 and 1885, the star being supposed to have passed rapidly through periastron in the long period from 1865 to 1883, in which it was unobserved. All the observations would then be satisfied by an ellipse of 100 years of revolution. Σ 2367 and Σ 2525 are noted as appearing as single stars with the 18-inch refractor in 1887.

ENCKE'S COMET.—Mr. John Tebbutt, Windsor, New South Wales, informs us that he picked up this object on the evening of July 8. Its place as observed closely accorded with that given in Dr. Backlund's ephemeris.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *SEPTEMBER* 2–8.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on September 2

Sun rises, 5h. 17m.; souths, 11h. 59m. 21·6s.; sets, 18h. 42m.: right asc. on meridian, 10h. 47·6m.; decl. 7° 40′ N. Sidereal Time at Sunset, 17h. 31m.

Moon (New on September 6, 5h.) rises, 0h. 39m.; souths, 8h. 49m.; sets, 16h. 55m.: right asc. on meridian, 7h. 37·2m.; decl. 20° 54′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	6 2 ...	12 33 ...	19 4 ...	11 21·6 ...	5 28 N.
Venus.....	6 35 ...	12 55 ...	19 15 ...	11 43·3 ...	3 12 N.
Mars......	12 26 ...	16 39 ...	20 52 ...	15 27·8 ...	20 24 S.
Jupiter....	12 38 ...	16 58 ...	21 18 ...	15 47·5 ...	19 19 S.
Saturn....	2 43 ...	10 19 ...	17 55 ...	9 6·9 ...	17 19 N.
Uranus ...	8 33 ...	14 9 ...	19 45 ...	12 57·5 ...	5 29 S.
Neptune..	21 28*...	5 15 ..	13 2 ...	4 2·4 ...	18 59 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich).

Sept.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
2 ...	61 Geminorum ..	6 ...	0 4 ..	0 50 ...	84 216
3 ...	B.A.C. 2854 ...	6 ...	4 54	near approach	135 —

Sept.	h.	
4 ...	1 ...	Saturn in conjunction with and 0° 34′ south of the Moon.
7 ...	0 ...	Mercury in conjunction with and 3° 46′ south of the Moon.
7 ...	7 ...	Venus in conjunction with and 3° 32′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′			h. m.	
λ Tauri...	3 54·5 ...	12 10 N. ...	Sept.	4,	20 26	*m*
ζ Geminorum ...	6 57·5 ...	20 44 N. ...	,,	8,	21 0	*m*
δ Libræ	14 55·0 ...	8 4 S. ...	,,	6,	21 42	*m*
V Ophiuchi... ...	16 20·5 ...	12 10 S. ...	,,	6,		*M*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,,	4,	1 15	*m*
			,,		21 22	*m*
W Sagittarii ...	17 57·9 ...	29 35 N. ...	,,	8,	1 0	*m*
β Lyræ...	18 46·0 ...	33 14 N. ...	,,	7,	23 0	m_2
R Lyræ	18 51·9 ...	43 48 N. ...	,,	2,		*M*
S Vulpeculæ ...	19 43·8 ...	27 1 N. ...	,,	2,		*M*
η Aquilæ	19 46·8 ...	0 43 N. ...	,,	7,	0 0	*M*
R Sagittæ	20 9·0 ...	16 23 N. ...	,,	6,		*m*
X Cygni	20 39·0 ...	35 11 N. ...	,,	5,	3 0	*M*
T Vulpeculæ ...	20 46·7 ...	27 50 N. ...	,,	2,	3 0	*M*
δ Cephei	22 25·0 ...	57 51 N. ...	,,	4,	1 0	*m*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near Algol	43 ...	39 N. ...	Swift; streaks.
,, ε Lyræ	282 ...	42 N. ...	Swift; bright.
,, β Piscium	345 ...	1 N. ...	Slow; bright.

GEOGRAPHICAL NOTES.

THE current number of the Proceedings of the Royal Geographical Society contains the report of his first year's work by Mr. Mackinder, the Reader in Geography to the University of Oxford, whose appointment is due to the Society. He describes the year as one of reconnoitre and preparation; nevertheless he delivered forty-two ordinary lectures in the University, and one public lecture; in each of the three terms he lectured for seven weeks twice a week, having two courses going on side by side on different days, to one of which he imparted a scientific, to the other an historical bias. The notices were, by permission of the Board of Faculties, published in the lists of two separate Faculties—Natural Science and Modern History. On the scientific side the lectures have been on the principles of geography—"a review of the subject not merely physical, yet taking the feature, and not the region, as the basis of classification." This course has not been so well attended as the other, but Mr. Mackinder congratulates himself that he has never been wholly without an audience, "a fate not altogether unknown just now to Oxford Professors and Readers." On the historical side the lectures were on the geography of Central Europe, and the influence of physical features on man's movements and settlements. "My aim is to furnish general instruction to as large a number as will favour me with their attention; and also to have always round me two or three whom we may style specialists. I can only say that I now see a very fair prospect of obtaining the latter. It may be well to place on record my humble opinion, that the best preliminary training for a geographical *specialist* is sound grounding in general science, and superadded to this an elementary knowledge of history. I have found by experience that it is exceedingly hard to give the necessary scientific knowledge to an historian"—a somewhat hard saying for the historians. In the coming academical year the lectures will be on the physical geography of continents, the geography of the British Isles, and the historical geography of North America. As Extension Lecturer, Mr. Mackinder has delivered 102 lectures on geography and physiography at various towns throughout the country.

In the August number of the *Scottish Geographical Magazine*, Mr. Forbes reports on his attempts to reach the Owen Stanley Peak, and incidentally describes the moving adventures by flood and field of his last expedition. Although not successful, owing to more than one unexpected mishap, in reaching his goal, he claims that the results accomplished so far. have not been few or inconsiderable. Large additions have been made to botanical and some to zoological science; an extensive series of meteorological observations has been tabulated, and a tract of country has been mapped for the first time. Mr. Ravenstein briefly describes the recent explorations in the territories of the African Lakes Company between Nyassa and Tanganyika. Both these papers are accompanied by excellent maps. Archdeacon Maples, of the Universities Mission to Central Africa, gives a detailed account of Lukoma, the principal island in Lake Nyassa, which, although only 4½ miles long by 2½ broad, has a population of 2500, or about 220 to the square mile, in consequence of its comparative freedom from war. "Ula," or witchcraft, of the kind described by Mr. Rider Haggard with such graphic force in one of his earlier works, prevails, and is a curse to the island. Herr Metzger contributes a most interesting paper on the scientific work lately done in the Dutch East Indies, based mainly on recent Government publications and those of various learned Societies in Holland and Java.

The current number of the *Deutsche Geographische Blätter* contains two papers of considerable geographical and ethnological interest. The first, by Herr August Fitzau, is devoted to the little-known region of the north-west African seaboard between Morocco and the Senegal River. After an historical survey of the various attempts made to found European settlements in this region, the writer describes in detail the sections of the coast between Agadir and Cape Juby, and thence to Saint Louis. He then deals with the Western Sahara in general, and especially with the ethnological relations of the regions south of the Atlas and north of the Senegal River, arriving at the general conclusion that, although Arabic has become the dominant language, the old Berber or Hamitic is still the prevailing ethnical element, variously modified by Semitic and Negro influences. In the second paper the distinguished traveller and ethnologist, Dr. O. Finsch, gives a sympathetic and permanently valuable account of the life and work of the late Mikluho-Maclay, to whom anthropological science is so much indebted for his profound studies of the Malayan, Papuan, Negrito, Melanesian, and Australian races. The memoir is very complete, including a detailed account of the naturalist's travels with their scientific results, his vast ethnological collections and the zoological stations founded by him, and concluding with a full descriptive catalogue of his numerous geographical, anthropological, and zoological writings.

The July number of the *Bollettino* of the Italian Geographical Society is mainly occupied with Leonardo Fea's recent explorations in Tenasserim. The chief points visited were the curious "Farm Caves" in the Moulmein district, and Mount Mulai (Moolaee) in the Dona Range. This peak, culminating point of Tenasserim (6300 feet), was reached and ascended to its summit after a journey full of difficulties and hardships, which followed the course of the Jeeayng-Myit and its great southern tributary, the Undurò, as far as Meetan in 46° N., 98° 30′ E. From Meetan the route struck north to Tagata and Mulai through the hilly territory of the little-known Ayaeen Karens. Of this tribe Signor Fea gives an interesting account, and he was also successful in securing a large zoological collection, including 450 skins of birds, over 400 mammals, many hundred reptiles, batrachians, and fishes, besides numerous insects, spiders, mollusks, and other small animals. These treasures go to enrich the valuable zoological materials already brought together in the Natural History Museum founded at Genoa by the Marquis Giacomo Doria. The paper is accompanied by a map of the region explored, as well as by several original sketches by the naturalist himself. The Marquis Doria has added a useful list of the various memoirs that have appeared in connection with Signor Fea's geographical and biological researches in Burmah during the last four years.

The most important amongst recent explorations in Indo-China are those undertaken by the Vice-Consul for France at Luang Prabang, the capital of an outlying region of Siam of the same name, and itself situated on the Mekong. M. Pavie, the official in question, has since succeeded in reaching Tonquin from this place by two different routes, the most practicable apparently being that to the north-east along the valley of the Namseng, a tributary of the Mekong, and then across the mountains forming the watershed of the Mekong and Songkoi, or Red River of Tonquin, to the valley of Nam Tay or Black River, down which M. Pavie proceeded to Sontay and Hanoi.

At a recent meeting of the Swedish Geographical and Anthropological Society, Baron H. von Schwerin gave an account of his late expedition to the Congo and West Africa, extending over a period of nearly two years, and undertaken at the instance of the Swedish Government. He had proceeded in a steamer as far up the Congo as Stanley Falls, and then up the Kassai, the principal tributary of the former. Next he had, in the company of his countryman, Lieut. C. Håkansson, explored the basin of the Inkissi, another tributary of the Congo, and from Banana made an excursion into the land of the Mushirongi, south of the mouth of the river, a country never hitherto visited by any European. After a journey to Angola and Mossamedes, on the west coast, a journey performed in a sailing-vessel, and extending as far north as Cape Negro, he made an excursion into the lands of Kakongo and Kabinda, situated to the north of the mouth of the Congo, which had also hitherto been considered closed to Europeans. The heat on the Congo was not so excessive as was generally imagined. A temperature above 35° C. was rare, but what were particularly enervating and exhausting were the steadiness of the high temperature and the total absence of cooling breezes, whether in the shade or at night, and, more than either, the exce sive humidity of the air. He considered the climate of the Congo one of the healthiest in Africa. Finally, Dr. Schwerin gave an account of his discovery on the promontory south of the Congo River of the remains of the marble pillar raised there in 1484 by Diego Cam in commemoration of the discovery of this mighty river, and destroyed by the Dutch in the sixteenth century. The speaker also exhibited a large and valuable collection of scientific objects gathered in Africa.

NOTES ON METEORITES.

I.

Their Fall and Physical Characteristics.

A perusal of the Chinese annals—which reach back to the year 644 before our era, and are still models of patient record—or of the much more irregular and less complete ones of the Western world, shows in the most definite manner that since the very commencement of human history, from time to time falls of bodies on to the earth from external space have been noticed. Biot has traced in Ma-tuan-lin the record of sixteen falls from the date before mentioned to A.D. 333.

The earliest fall recorded in Europe, however, transcends in antiquity anything the Chinese can claim, dating as it does from 1478 B.C. It happened in Crete, but the record is much more doubtful than that of the falls in 705 and 654 B.C., noted, the first by Plutarch, and the second by Livy.

But in 466 B.C. occurred a fall at Aegos Potamos, in Thrace, concerning which the Chronicles of the Parian marbles, Plutarch, and Pliny all give us information. It was of the size of two mill-stones, and equal in weight to a full waggon-load.[1] Later, there fell in Phrygia, in about the year 2-4 B.C., a stone famous through long ages, which was preserved there for many generations. It was described as "a black stone, in the figure of a cone, circular below and ending in an apex above." It was worshipped by the ancients as Cybele, the mother of the gods,

[1] Humboldt, "Cosmos," Otté's translation, vol. i. p. 103.

and was transferred to Rome, as an oracle had announced that the possession of it would secure continual prosperity to the State.[1]

In more modern times we have records of various falls of these bodies. The following—a few out of a very great number—either possess a national interest or are the statements of eye-witnesses.

In England there fell a stone in the afternoon of December 13, 1795. A labourer happened to be working near Wold Cottage, Thwing, Yorkshire, when this stone fell within a few yards of him. On digging the stone out of the ground it was found to have penetrated a foot of soil and half a foot of chalk rock, and to weigh 56 pounds. The inhabitants of the neighbouring villages likened the explosion to the firing of guns at sea, while in two of them the sounds were so distinct of something rushing through the air towards Wold Cottage that some of the people went to see if anything extraordinary had happened.

The next account is from Ireland. It is the narrative of an eye-witness of a fall of meteorites in the county of Limerick.

"Friday morning, the 10th of September, 1813, being very calm and serene, and the sky clear, about 9 o'clock, a cloud appeared in the east, and very soon after I heard eleven distinct reports appearing to proceed thence, somewhat resembling the discharge of heavy artillery. Immediately after this followed a considerable noise not unlike the beating of a large drum, which was succeeded by an uproar resembling the continued discharge of musketry in line. The sky above the place whence this noise appeared to issue became darkened and very much disturbed, making a hissing noise, and from thence appeared to issue with great violence different masses of matter, which directed their course with great velocity in a horizontal direction towards the west. One of these was observed to descend; it fell to the earth, and sank into it more than a foot and a half, on the lands of Scagh, in the neighbourhood of Patrick's Well, in the county of Limerick. It was immediately dug up, and I have been informed by those that were present, and on whom I could rely, that it was then warm and had a sulphurous smell. It weighed about 17 pounds, and had no appearance of having been fractured in any part, for the whole of its surface was uniformly smooth and black, as if affected by sulphur or gunpowder. Six or seven more of the same kind of masses, but smaller, and fractured, as if shattered from each other or from larger ones, descended at the same time with great velocity in different places between the lands of Scagh and the village of Adare. One more very large mass passed with great rapidity and considerable noise at a small distance from me; it came to the ground on the lands of Brasky, and penetrated a very hard and dry earth about 2 feet. This was not taken up for two days; it appeared to be fractured in many places, and weighed about 65 pounds! Its shape was rather round, but irregular. It cannot be ascertained whether the small fragments which came down at the same time corresponded with the fractures of this large stone in shape or number, but the unfractured part of the surface has the same appearance as the one first mentioned. There fell also at the same time, on the lands of Faha, another stone, which does not appear to have been part of or separated from any other mass; its skin is smooth and blackish, of the same appearance with the first mentioned; it weighed about 74 pounds; its shape was very irregular, for its volume was very heavy. . . . . . It was about three miles in a direct line from the lands of Brasky, where the very large stone descended, to the place where the small ones fell in Adare, and all the others fell intermediately; but they appeared to descend horizontally, and as if discharged from a bomb and scattered in the air."[2]

The fall of the meteorite of 1885, near Mazapil, in Mexico, was thus described by an eye-witness vouched for by Prof. Bonilla:—[3]

"It was about nine in the evening when I went to the corral to feed certain horses, when suddenly I heard a loud hissing noise, exactly as though something red-hot was being plunged into cold water, and almost instantly there followed a somewhat loud thud. At once the corral was covered with a phosphorescent light and suspended in the air were small luminous sparks as though from a rocket. I had not recovered from my surprise when I saw this luminous air disappear, and there remained on the ground only such a light as is made when a match is rubbed. A number of people from the neighbouring houses came running toward me, and they assisted me to quiet the horses, which had become very much excited. We all asked each other what could be the matter, and we were afraid to walk in the corral for fear of getting burned. When, in a few moments, we had recovered from our surprise, we saw the phosphorescent light disappear, little by little, and when we had brought lights to look for the cause, we found a hole in the ground and in it a ball of fire. We retired to a distance, fearing it would explode and harm us. Looking up to the sky we saw from time to time exhalations or stars,[1] which soon went out, but without noise. We returned after a little, and found in the hole a hot stone, which we could barely handle, which on the next day we saw looked like a piece of iron; all night it rained stars, but we saw none fall to the ground, as they seemed to be extinguished while still very high up."

The next record of the phenomena attending a fall in the United States (though the observer quoted did not actually see the fall) is taken from a lecture by Prof. Newton:—[2]

"'The observers,' he says, 'who stood near to the line of the meteor's flight, were quite overcome with fear, as it seemed to come down upon them with a rapid increase of size and brilliancy, many of them wishing for a place of safety, but not having the time to seek one. In this fright the animals took a part, horses shying, rearing, and plunging to get away, and dogs retreating and barking with signs of fear. The meteor gave out several marked flashes in its course, one more noticeable than the rest. . . . Thin clouds of smoke and vapour followed in the track of the meteor. . . . From one and a half to two minutes after the dazzling, terrifying, and swiftly moving mass of light had extinguished itself in five sharp flashes, five quickly recurring reports were heard. The volume of sound was so great that the reverberations seemed to shake the earth to its foundations; buildings quaked and rattled, and the furniture that they contained jarred about as if shaken by an earthquake; in fact, many believed that an earthquake was in progress. Quickly succeeding, and blended with the explosions, came hollow bellowings and rattling sounds, mingled with clang, and clash, and roar, that rolled away southward, as if a tornado of fearful power was retreating upon the meteor's path.'

"About 800 pounds of stones, nearly 200 in number, have been picked up in a region seven miles by four, a little east of the end of the meteor's path, which without any doubt came from the meteor. Some were picked up on the surface of the frozen ground. One was found on the top of a snow-bank, and about 40 feet away were marks of a place where it had first struck the ground. Some were ploughed up in the spring. The two largest found, of 74 pounds and 48 pounds, fell by the roadside, and a lawsuit to settle whether they were the property of the finder as being wild game, or of the owner of the lands adjacent as being real estate, was decided in favour of the owner of the land."

In some cases of observed falls the rate of movement of the meteorite through the air has been determined, or concomitant circumstances have enabled it to be roughly estimated.

The velocities have been widely different. Before they are stated, some terms of comparison may be given:—

		Metres per second		Miles an hour.
Railway trains	... ...	27 nearly	...	60
Flight of swallow	... ...	30 to 40	...	67 to 92
Projectiles	.. ..	300 to 400	...	670 to 920
Sound	... ...	335⅓ nearly	...	750
Mercury	Movement in Orbit ...	48,900	...	109,358
Venus	...	36,780	...	83 162
Earth	...	30,430	...	68,052
Mars	...	24,650	...	55,135·5

The highest velocity of flight through the air has been that of the Stannern meteorites, 45 miles a second. The lower part of the flight of the Iowa meteorite was performed at 12 miles a second.

In only a few cases have the velocities been observed to be very great at the earth's surface, the retarding effect of the passage through the atmosphere being considerable. Some have buried themselves deeply in the ground, and one (New Concord) broke a railway-sleeper. Several meteorites have fallen so rapidly that the sound of the explosion *followed* them. But generally the rate is so slow that they are not broken on striking

[1] See British Museum Introduction to the Study of Meteorites, p. 17.
[2] Quoted by Maskelyne, "Lecture Notes on Meteorites," NATURE, 1875, vol xii. p. 485.
[3] NATURE, vol. xxxv. p. 572.

[1] The meteor fell during a star-shower.
[2] NATURE, vol. xix. p. 315.

the surface, and some that fell at Hessle on ice only rebounded without cracking it.

These bodies, when they fall under such conditions that they can be picked up and examined, are called meteorites. The first thing that strikes one when looking for the first time at these meteorites, is that their general form has the character of being essentially fragmentary, indicating that what we see is the result of a fracture.

The next point observed is that there is a very great difference between the interior and exterior appearances of these bodies. That this is caused by the heat and friction to which the exterior surface is exposed is proved by what was noticed in the case of a meteorite that fell at Butsura in 1861. Fragments of this stone were picked up three or four miles apart, and, with the exception of one corner, the original meteorite has been built up again by piecing the fragments together. The faces fit perfectly. Important pieces of this meteorite are in the British Museum, and these are all coated with the crust to which reference has been made. But, on the other hand, another of these fragments *not coated* fits another also not coated. Hence, to quote Prof. Maskelyne, "We can assert that this aërolite acquired, after coming into our atmosphere, a scoriated and blackened surface or incrustation. The first explosion drove the fragments first alluded to asunder, and these became at once incrusted on their broken surfaces; but others which were separated afterwards, probably on the last of the three explosions, had not sufficient velocity left [the heat being at the same time reduced] to cause their incrustation in the same manner as was the case with the fragments previously severed."[1]

The supposition is that the temperature is practically high enough to melt the meteorite, and that its surface as we see it after it has fallen does not in all cases represent the surface exposed to the air during the whole of the flight, but that it represents the last surface. The meteorite may have been twenty times bigger, but the rest may have been melted off like tallow would be, so that finally there is very little visible effect towards the interior, as the melting is more rapid than the conduction. The thinness of the so called varnish, then, is caused by the air-molecules carrying away the results of fusion as fast as the heat penetrates towards the interior, so leaving only, as a rule, a very thin film behind.

This crust is usually dull, but sometimes, as in the Stannern meteorite, bright and shining, like a coating of black varnish.

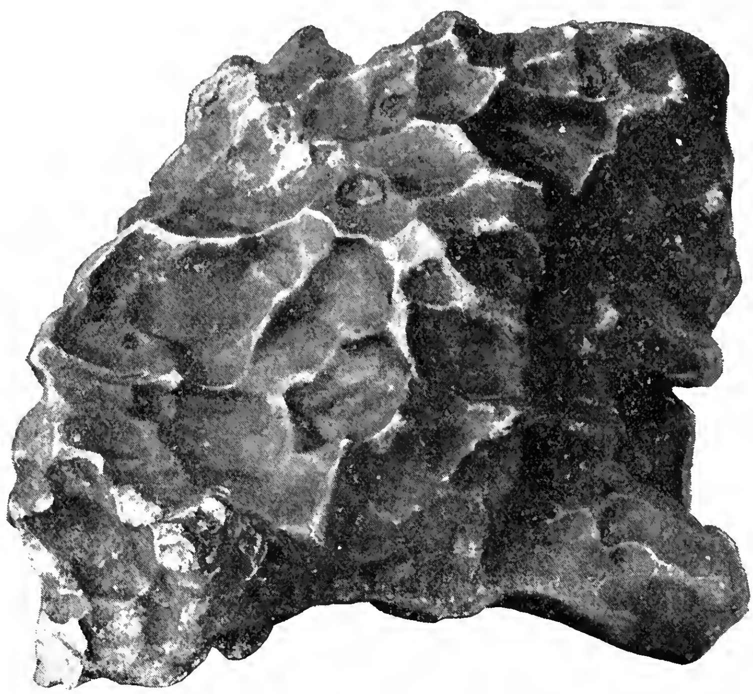

Fig. 1.—Mazapil Meteoric Iron (¾ natural size), showing thumb-marks.

Sorby,[1] on examining with a microscope a thin section of a meteorite, cut perpendicular to the crust, found that it is a true black glass filled with small bubbles, and that the contrast between it and the main mass of the meteorite is as complete as possible, the junction between them being sharply defined, except when portions have been injected a short distance between the crystals. He writes: —"We thus have a most complete proof of the conclusion that the black crust was due to the true igneous fusion of the surface under conditions which had little or no influence at a greater depth than 1/100 of an inch. In the case of meteorites of different chemical composition, the black crust has not retained a true glassy character, and is sometimes 1/50 of an inch in thickness, consisting of two very distinct layers, the internal showing particles of iron which have been neither melted nor oxidized, and the external showing that they have been oxidized and the oxide melted up with the surrounding stony matter. Taking everything into consideration, the microscopical structure of the crust agrees perfectly well with the explanation usually adopted, but rejected by some authors, that it was formed by the fusion of the external surface, and was due to the very rapid heating which takes place when a body moving with planetary velocity rushes into the earth's atmosphere—a heating so rapid that the surface is melted before the heat has time to penetrate beyond a very short distance into the interior of the mass."

In some cases close under the crust is found a mixture of the minerals troilite, asmanite, and bronzite, of an unaltered light-brown colour, although they turn deep black when raised to a temperature slightly above that at which lead melts.[2]

The crust or varnish of the meteorite in many cases contains numerous furrows and ridges, so that it is not equally thick. This effect is caused, as it is supposed, by its motion through the air in a fixed position, the forward part of the meteorite, in regard to its line of motion, being most liquefied, and the liquid flowing unequally towards the hinder part.

A very special study of the results of the passage through the air is a desideratum. Thus, in the case of the Tennessee iron, which fell from a cloudless sky (and which therefore fell with a low velocity?), the outer surface is elaborately reticulated, edges

[1] "On the Structure and Origin of Meteorites," NATURE, vol. xv. p. 495.

[1] "Lecture Notes," *loc. cit.* p. 487.
[2] Flight, "History of Meteorites," p. 169.

of thin laminæ of metal inclined at angles of 60° traversing it. Hence no fusion of the superficial layer took place.[1]

Another peculiarity of the surface is that it is generally covered with small depressions called "thumb-marks," as they have been likened to the impressions that one makes when pressing some such substance as putty with one's fingers. The cause of these thumb-marks is unknown, but they have been found to bear a close resemblance to marks which have been noticed on grains of gunpowder blown out on firing large guns.

A possible cause of these pittings is thus suggested by Prof. Maskelyne:—"The aërolite comes into our atmosphere from regions in which the temperature—'the cold of space'—may range as low as 140° C. below zero; and though the mass, from the absorption of solar heat, would possess a temperature much above this, it would nevertheless be intensely cold, and consequently more brittle than at ordinary temperatures; and hence, on its entering our atmosphere, the heat it instantaneously acquires on its outer portion expands this, and tends to tear it away, so as to dissever the exterior from the interior, which continues to be relatively contracted by the intensity of the cold which the aërolite brings with it from space. The consequence is, first, that little bits of the stone spring out all over it, leaving those curious little holes or pit-marks which are characteristic of a meteorite; and every now and then, as the heat penetrates, larger masses split away, of which interesting evidence is afforded by the meteorite, for instance, that fell at Butsura on May 12, 1861."

On this it may be remarked that the pittings are common to irons and stones, while the above explanation only applies to stones.

It is not a little worthy of notice that the pitting does not always appear on all the surfaces. In the case of a meteorite which fell in Kentucky in 1877, one portion of it is very extensively and regularly pitted, while the rest is comparatively smooth. The crust is dull black, and is as perfect as when the stone fell. There was a fresh broken spot of two or three square centimetres, which was evidently made prior to the fall, for a few small specks of the melted matter adhered to the surface.[2]

These meteorites, which we can thus examine, are in all probability, for the most part, remnants of larger bodies which had enough substance in them to stand the wear and tear of getting through our atmosphere.

The fragments picked up even from the most extensive falls have appeared to those who have witnessed or who have subsequently studied the phenomena to be out of all proportion small to the violence and magnitude of the explosive and luminous effects observed.

The origin of the concomitant phenomena so universally recorded is not far to seek.

Supposing a meteorite passing towards the earth through the atmosphere, what sort of effects are we to expect to find? It passes, as we have already seen, very rapidly into the earth's atmosphere, which consists of molecules with a certain mean free path, and the temperature and pressure of which depend upon the encounters between these molecules.

When we come to consider the general velocity of movement of these molecules, we find that the big molecule, the meteorite, is travelling towards the earth about fifty times faster. The result is that there is a tremendous crowding of air, so to speak, in front of the meteorite, a tremendous pressure and therefore a tremendous temperature brought about by its passage. There is a partial vacuum behind which subsequently has to be filled up by the transit of the molecules round the meteorite itself from the front part to the back.

We have therefore conditions for producing most violent action upon the meteorite, both by pressure and temperature; it may be crushed by the pressure to which it is subjected, it may be melted by the heat produced by the circulation of the molecules rushing past it. We may therefore have violent incandescence and explosion, and as we have the air molecules rushing violently from front to rear we shall have almost the noise of a thunderstorm added to the sudden luminosity resembling lightning.

The observers of actual falls have heard other special noises, due, not to the explosion itself, but to the rapid passage of the meteorites through the air, from the "ping" of a rifle bullet to the hum of a locomotive, sounds which have been likened to the tearing of linen, the lowing of cattle, the flapping of wings.

We can best study the differences in the structure of meteorites by preparing a polished section. In some cases this has a distinctly metallic look. We find, in fact, a metallic fragment composed almost entirely of iron, but with a certain amount of nickel.

The nickel in the iron meteorites causes them to have a whitish appearance, and it is in this way that they have been mistaken for silver when found, the nickel preventing the outer surfaces from rusting as is the case with an ordinary iron.

By taking a polished section, and exposing it to the action of an acid or bromine, we obtain what have been called the "figures of Widmanstätten." These figures are more or less complicated, and remarkable for their extreme regularity. They are due to the inequality of the action of the acid on the various constituents of the polished surface; these being various alloys of iron and nickel.

In other specimens the characteristic is that the metal, instead of being continuous as in those previously referred to, appears to have existed once as a spongy paste, and to have included fragments of stony matter, so that in the section, instead of getting the pure metallic lustre all along, we only get it here and there. We pass from metal to metal *plus* stone.

In yet other specimens we get another generic case represented in which the stone is the main point and the metal the exception, the metal appearing as excessively small granules; so that in the final term of the series we come to almost pure stone with no iron to speak of.

In the case of the stones, not only does the meteorite itself give the idea of a fragment, as in the case of the irons, but the internal structure of many of them shows that the whole meteorite is composed of fragments, giving the characteristics of a brecciated rock made up of pieces cemented together.

FIG 2.—Section of Mazapil Meteoric Iron (natural size), showing Widmanstätten figures.

Further, these constituent particles, as pointed out by Sorby, are often themselves mere fragments, although the entire body before being broken may originally have been only one fortieth or one-fiftieth of an inch in diameter.

On examining thin sections of stony meteorites by means of polarized light, they are found to be crystallized throughout, the interference tints colouring the different crystals of which the sections are composed, thus showing the crystalline character of the whole. The stony part of both siderolites and aërolites is almost entirely crystalline, and presents a peculiar "chondritic" structure, which make meteorites differ from ordinary terrestrial rocks; the loose grains in these are found to be more or less aggregated in little spherules, and of similar mineral to those which inclose them.

These spherules, or chondroi—their sizes varying very considerably, some of which can be seen only under a microscope, while others are as large as a cherry—are found embedded in a matrix, made up, as it appears, of minute splinters such as would result from the disintegration of other chondroi.

While the chondroi in terrestrial rocks such as perlite, obsidian, pitchstone, and in many diorites, are radiate-fibrous, those occurring in meteorites are but rarely so, and the arrangement of the fibres within the spherule is eccentric. While the meteoritic chondroi also consist of the same ingredients as the matrix, and often differ from it only in being more coarsely granular, the chondroi of terrestrial rocks are differently constituted from the matrix.[1]

The weight of meteorites varies very considerably, ranging from tons to very small specimens. It not only depends on their volume but on their chemical composition, as some of the stony ones have a low density while others are nearly pure metal.

The largest meteorites of which mention is made are those

[1] Flight, *op. cit.* p. 108. [2] *Ibid.* p. 200.

[1] *Ibid.* p. 141.

of Otumpa (province of Tucuman, South America), an iron weighing thirty tons; of Durango (Mexico), nineteen tons; and of Cranbourne, Australia (now in the British Museum), which weighs over three tons.

The Nejed iron, the largest which has been seen to fall, weighs nearly 130 pounds.

Considering the very considerable number of falls which have taken place, the number of irons which have been *seen to fall* is remarkably small. They are as follows:—

Agram, 1751.
Tennessee, 1835.
Braunau, 1847.
Victoria West (South Africa) 1862.
Nejed, 1863.
Nidigullam (Madras) 1870.
Rowton, Shropshire, 1876.
Mazapil, 1885.
Cabin Creek, 1886.

The following table contains a list of some of the larger meteorites, besides those mentioned above, which have been found from time to time, with the locality of their fall and their weights in grammes (1000 grammes = 2·2 pounds avoirdupois (nearly), and 1,018,181 grammes (nearly) = 1 ton):—

	Weight in grammes.
Siderites—	
Bahia, Brazil	6,350,000
Charcas, Mexico	780,000
Tucuman, Argentine Republic, South America	637,000
The Butcher Iron, Desert of Bolson de Mapimi, Mexico	253,632
Toluca Valley, Mexico	91,007
Cocke County (Cosby's Creek), Tennessee, U.S.A.	52,325
Rancho de la Pila, nine leagues east of Durango, Mexico	46,512
Obernkirchen, near Bückeburg, Germany ...	35,366
Carthage, Smith County, Tennessee, U.S.A.	24,570
Siderolites—	
Imilac, Desert of Atacama, South America ...	227,328
Estherville, Emmet County, Iowa, U.S.A. ...	116,487
Aërolites—	
Wold Cottage, Thwing, Yorkshire	20,111
Pultusk, Poland	18,007
Butsura (Qutahar Bazaar), Bengal, India ...	13,071
Knyahinya, near Nagy Berezna, Hungary ...	13,053
Durala, N.W. of Kurnal, Punjab, India ...	12,588
Dhurmsala, Kangra, Punjab, India	12,407
Nellore (Yatoor), Madras, India	11,287

Classification of Meteorites.

Meteorites have been arranged into three classes: first, masses of iron alloyed with nickel, which have been called by Maskelyne, aëro-siderites (*aer*, air, and *sideros*, iron) or briefly siderites; secondly, those which are almost wholly composed of stone, and called aërolites (*aer*, air, and *lithos*, stone); and, thirdly, those which are composed of stone and iron in more or less equal quantities, consisting of a spongy mass of iron interlaced with stony matter like that of the aërolites, and called siderolites or meso-siderites.

M. Daubrée's general classification of meteorites is as follows:—

Meteorites	Containing metallic iron	Not containing stony matter	... ...	Holosidères
		Containing iron with stony matter	The iron constituting a matrix which encases stony grains	Syssidères
			The iron existing in the form of grains among stony matter	Sporadosidères
	Not containing metallic iron	... ...		Asidères

This brings us to consider the chemistry of these messengers from the celestial spaces. J. NORMAN LOCKYER.

(*To be continued.*)

THE GLASGOW AND WEST OF SCOTLAND TECHNICAL COLLEGE.

At the present time, when so much is being said and done in connection with technical education, and so many new institutions are being founded, it may interest the readers of NATURE to learn how some old ones have been reorganized to enable them more adequately to meet the requirements of the times. The Glasgow and West of Scotland Technical College was founded by an Order of the Queen in Council, dated November 26, 1886, according to a scheme framed by the Commissioners appointed under the provisions of the Educational Endowments (Scotland) Act, 1882, whereby Anderson's College, the Young Chair of Technical Chemistry in connection with Anderson's College, the College of Science and Arts, Allan Glen's Institution, and the Atkinson Institution, were placed under the management of one governing body. A considerable amount of interest is attached to the histories of these institutions, of which a few of the chief dates may be mentioned.

Anderson's College was founded by John Anderson, M.A., F.R.S., Professor of Natural Philosophy in the University of Glasgow, who, by his will, dated May 7, 1795, bequeathed the whole of his property, with a few trifling exceptions, "to the public for the good of mankind and the improvement of science, in an institution to be denominated 'Anderson's University,' and to be managed by eighty-one trustees." The endowment included a general museum, library, and valuable philosophical apparatus; and the intention of the founder was to provide a complete circle of liberal and scientific education suitable for all classes, and adapted to the wants and circumstances of the period, but the design was never fully carried out. The Andersonian Institution or University was incorporated on June 9, 1796, and it has numbered among its Professors some distinguished men. Of these may be named Dr. Garnett, Dr. George Birkbeck, Dr. Andrew Ure, and Thomas Graham, who afterwards became Master of the Mint. The Medical School attained considerable importance, attracting students from all parts of the country, and sending forth a number of medical practitioners—many of whom have attained to eminence, and a few to great distinction, in their profession. On the foundation of the Glasgow and West of Scotland Technical College, the Medical School of Anderson's College was placed under a separate governing body, and provision is being made for its removal to other buildings.

In the year 1870, Dr. James Young, of Kelly and Durris, settled in trust the sum of £10,500 for the purpose of establishing a Chair of Technical Chemistry, to be called "The Young Chair of Technical Chemistry in connection with Anderson's University," and on the organization of the Glasgow and West of Scotland Technical College, Dr. Young's testamentary trustees conveyed to the governors of the College the Young Laboratory Buildings, situated in John Street, Glasgow. Various other endowments were given at different times to Anderson's University. In 1861, Mr. John Freeland, residing at Nice, mortified the sum of £7500 to secure the delivery, annually or periodically, of "separate courses of popular lectures on the three following subjects, or any one of them, namely (1) Chemistry; (2) Mechanical and Experimental Physics; and (3) Anatomy and Physiology," and in 1871 he made a further gift of £5000 to the University. In 1866, Mr. William Euing, insurance broker in Glasgow, settled in trust the sum of £3000 for the purpose of securing the delivery of courses of popular lectures in Anderson's University upon the history and theory of music, and upon the lives of eminent musicians; and also upon such branches of acoustics as may be connected with and illustrate the science and practice of music. By his will he bequeathed his whole musical library to the University, along with £1000 for the purpose of building a fire-proof room for its accommodation, besides the sum of £200 to print a catalogue. Mr. Euing also left the University the sum of £6000 for general purposes; and £150, the interest of which is to be applied in providing prizes in connection with the Lectureship on Music instituted by him. In 1876, through the liberality of a few friends, a Chair of Applied Mechanics, with a suitable endowment, was founded.

The College of Science and Arts was the direct successor of the Mechanics' Institution, which owed its origin to the popular lectures begun in 1800 by Dr. Birkbeck in Anderson's University, and continued by his successor. In 1823 a number of students attending these mechanics' classes resolved to sever their connection with Anderson's University, and thereafter formed the Glasgow Mechanics' Institution, of which Dr. Birkbeck became the first President. He also became President of the Mechanics' Institution in London, which was opened in November 1823, on the same plan as that of Glasgow, after which the system rapidly extended over the Kingdom. In 1879 the Institution was reorganized, and two years later the name was changed to "College of Science and Arts, Glasgow," from which time the commercial classes were discontinued, and the College classes entirely devoted to the teaching of science and its applications, more especially to engineering.

Allan Glen's Institution was founded under the will of Allan Glen, wright in Glasgow, dated 1847-48, and was intended to afford gratuitous education to about fifty boys, sons of tradesmen or persons in the industrial classes of society. In 1876 the Institution was reorganized, and it became a high-class secondary school for boys who are intended for industrial and mercantile pursuits. The trustees fitted up a laboratory, lecture-room, apparatus-room, and workshops in the school, which soon became well known for the good secondary technical instruction which it afforded.

The Atkinson Institution never really had an active existence, and the interest of the money which was left by Thomas Atkinson, bookseller and stationer in Glasgow, is now to be used in providing bursaries for the students attending the Glasgow and West of Scotland Technical College.

Provision is made under the scheme for the further endowment of the College by annual subsidies out of the funds of the Glasgow City Educational Endowments Board and the Hutcheson's Educational Trust. These subsidies are fixed in the special schemes for these Boards at not less than £800 and £1400 respectively.

By the scheme drawn out by the Educational Endowments (Scotland) Commissioners, the institutions above referred to have been amalgamated and placed under the management of one governing body, which has been selected from among the representatives of the old institutions and from various public bodies in Glasgow. The problem which the governing body had to solve was to arrange a number of hitherto competing and to a certain extent opposing institutions into something like a homogeneous unity. Of course under the circumstances it is not to be expected that a perfect scheme can at once be evolved, but on the whole it will be found that a fairly good arrangement has been made. Allan Glen's School is being extended, and is intended to be a secondary technical school for boys to sixteen years of age; while Anderson's College, the Young Chair, and the College of Science and Arts form the College proper. For entrance to this, students under sixteen years and all those who intend to go in for any of the diploma courses are required to pass an examination, but this is not so difficult as to exclude those who are likely to benefit by the work of the College classes. The diploma of the College will be awarded in the following departments of study: (1) Civil Engineering; (2) Mechanical Engineering; (3) Naval Architecture; (4) Electrical Engineering; (5) Architecture; (6) Chemical Engineering; (7) Metallurgy; (8) Mining Engineering; (9) Agriculture. Each course extends over three years, the subjects of instruction in the first year being common to all, while in the second and third years the subjects are arranged to suit the special departments selected by the students. There are three sets of examinations for the diploma: (1) at the end of the first session, in the scientific subjects of the first year's course; (2) at the end of the second session, in the modern language and the general subject selected by the student; (3) at the end of the third session, in the main subject of the department selected by the candidate. This examination will be partly by written papers and partly oral, and will be of such a nature as not only to test the candidate's knowledge of the main subject, but also of the various subsidiary subjects included in the course. When the subject admits of it, laboratory work will form an essential part of the examination. Lastly, each candidate will be required to work out a design, with specifications and estimates, from data which will be supplied. Such examinations should test a student's real knowledge of a subject, and his power of application to the solution of the problems which arise in every-day life.

The evening classes of the College are conducted chiefly according to the arrangements of the Science and Art Department, and of the City and Guilds of London Institution, and they are arranged in the following courses: (1) Mechanical Engineering; (2) Naval Architecture; (3) Electrical Engineering; (4) Architecture; (5) Building Construction; (6) Mining; (7) Metallurgy; (8) Agriculture; (9) Chemical Industries; (10) Textile Industries; (11) Art Industries; (12) Commerce. In each of these departments there are two grades of certificates, senior and junior, the latter being within the reach of all apprentices. Students who have obtained the senior certificate for the evening classes may obtain the diploma for the day curriculum by attending the third year's course in the corresponding department of the curriculum and passing the necessary examinations. In connection with both the day and evening classes of the College, there are a considerable number of scholarships and bursaries; and in addition the governors have power to remit in whole or in part the fees of artisans and others who are desirous of attending the day classes, and require aid for obtaining the education therein provided. In order to encourage systematic study this privilege will only be afforded to students who have obtained the senior certificate of the College. Arrangements are thus made which should enable all really deserving students to pass from the lowest evening class to the highest classes at the College, or the University; for the students who obtain bursaries will have the option of going to the University or of remaining at the Technical College.

Allan Glen's School is being considerably enlarged, and new class-rooms, drawing-offices, and workshops are being added, and the curriculum of the school has been re-written to suit these enlargements. The elementary department is being gradually curtailed, and will ultimately be dropped, so as to allow of the whole space being available for the secondary department. In this department there are five classes, in the first three of which are given the elements of a good general education, with the scientific side more fully developed than is the case in ordinary schools. In the fourth and fifth classes the work is of a more special nature, and in the last year the attention of the students is directed either to mechanical and electrical engineering or to chemistry. By the time they have completed the course, they ought thus to be in a position to enter on their apprenticeship in the workshops with advantage to themselves, as well as to their employers.

During the past year the number of students who attended the day classes of the College was 168, and the evening classes 1771, and the number of scholars in Allan Glen's School was 439, or a total of 2378, which shows that technical education is being taken advantage of to a considerable extent in Glasgow. One good feature in the arrangement of the College is that advantage is taken of other institutions in so far as their classes can be utilized for the different curricula. For instance, in the day classes the University, and in the evening classes the Athenæum and School of Art and Haldane Academy, make up some of the deficiencies of the Technical College. The resources of each institution are thus fully utilized, and there is no unnecessary waste of energy or money in maintaining duplicate classes.

HENRY DYER.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

DEPARTMENT OF SCIENCE AND ART.—The following is the list of candidates successful in the competition for the Whitworth Scholarships and Exhibitions, 1888:—1. Scholarships, tenable for three years (£125 a year each): Jas. Whitaker, 22, student, Nelson, Lancashire; James Mair, 22, engineer, Glasgow; C. Humphrey Gilbert, 22, engineer student, Nottingham; John Calder, 21, mechanical engineer, Glasgow. 2. Exhibitions, tenable for one year (£100 each): Harry Bamford, 22, engineering student, Oldham; John Harbottle, 21, draughtsman, Newcastle-on-Tyne; John Taylor, 21, engineer, Glasgow; John Dalglish, 24, mechanical draughtsman, Paisley; Archibald S. Younger, 23, engineer student, North Shields; Joseph Butterworth, 22, engineer, Rochdale; George A. Burls, 21, mechanical draughtsman, Greenwich; Charles H. Kilby, 20, engineer apprentice, Crewe; Charles R. Pinder, 21, engineer student, Bristol; Robert Dumas, 22, engineer, Glasgow; Charles L. E. Heath, 21, fitter apprentice, Devonport; Charles Forbes, 21, engine fitter apprentice, Glasgow; Benjamin Young, 23, electrical engineer apprentice, Belfast; Edward Y. Terry, 23, engine fitter, Devonport; William J. Collins, 23, draughtsman, Woolwich; John H. B. Jenkins, 21, assistant analytical chemist, New Swindon; John I. Fraser, 24, apprentice engineer, Glasgow; Henry E. Cheshire, 24, fitter, Crewe; Oscar Brown, 23, pattern maker, Plumstead;

Henry Elliott, 25, mechanical engineer, Glasgow; (£50 each) Jas. H. Binfield, 23, engineer student, Preston; George U. Wheeler, 20, engineer apprentice, London; William Day, 22, fitter, Wolverton; Samuel Lea, 25, turner, Crewe; Evan Parry, 22, engineer student, Bangor; Thomas O. Mein, 23, engineer, Stratford, E.; Benjamin Conner, 23, apprentice engineer, Glasgow; Thomas J. Bourne, 23, marine engineer, Tunbridge Wells; George Ravenscroft, 25, fitter, Crewe; Thomas F. Parkinson, 22, engineer student, Bury, Lancashire.

The following is the list of successful candidates for Royal Exhibitions, National Scholarships, and Free Studentships, 1888:—National Scholarships: John B. Coppock, 23, student, Nottingham; James G. Lawn, 20, mining surveyor, Barrow-in-Furness; Herbert Grime, 19, teacher, Manchester; Alfred Stansfield, 17, student, Bradford; John Eustice, 24, engine fitter, Camborne; Edwin Wilson, 19, student, Bradford; Lionel M. Jones, 18, student, Llanelly; Joseph Jefferson, 20, student, Bradford; Henry T. Bolton, 15, student, Newcastle-on-Tyne; Ben. Howe, 18, student, Manchester; John Yates, 20, draughtsman, Manchester; Harry Cavendish, 17, student, Manchester. Royal Exhibitions: Thomas S. Fraser, 17, laboratory assistant, Glasgow; Benjamin Young, 23, electrical engineer apprentice, Belfast; James Harrison, 29, shoemaker (rivetter), Northampton: John D. Crabtree, 16, student, Bradford; Joseph Burton, 19, student, Manchester; John Taylor, 21, engineer, Glasgow; Joseph Husband, 17, student, Sheffield. Free Studentships: Thomas Beatham, 16, student, Newcastle-on-Tyne; Charles H. Kilby, 20, engineer apprentice, Crewe; George H. Gough, 17, student, Bristol; Henry E. Cheshire, 24, fitter, Crewe; Ernest W. Rees, 20, engineer apprentice, Carnarvon; Stanley H. Ford, 17, student, Bristol.

University College, London.—*Gilchrist Engineering Scholarships.*—An entrance scholarship will be offered next month (September). The value is £35 per annum, tenable during two years, and the competition is limited to those who have not previously been students of the College, and who will not complete their nineteenth year before October 1. Every candidate must declare his intention of taking, at least, the two first years of one of the engineering courses, and the second payments will depend upon his success during the first year and the arrangements he makes for the second year's study. The subject of the examination will be mathematics, and any two or more of the following five subjects: mechanics, mechanical drawing, an essay on a given subject, French or German, and the use of tools. A senior scholarship of £80 will be awarded at the close of the session. Candidates must have attended College classes in the following subjects during the whole of the session: applied mathematics, physics, engineering, engineering drawing, and geology. The results of the class examinations will decide the obtainment of the scholarship, providing sufficient merit has been shown to justify the award. There are also entrance and other exhibitions and scholarships given at University College for mathematics, physics, chemistry, classics, German, French, art, Greek, Hebrew, jurisprudence and political economy, philosophy of mind and logic, English literature, medicine, surgery, pathology, and physiology.

SCIENTIFIC SERIALS.

The Quarterly Journal of Microscopical Science for July 1888 contains the following:—On *Haplodiscus piger*, a new pelagic organism from the Bahamas, by W. F. R. Weldon, M.A. (plate 1). The body is ellipsoidal in outline, the antero-posterior diameter being the shortest; in an average specimen the long diameter measured 1·3 mm., the short 1·1 mm. The dorsal surface is slightly convex, the ventral flat, but concave on muscular contraction. There is a cuticular body wall; a muscle layer on the ventral surface; the innermost body layer is a protoplasmic tunic, embedded in which are numerous mucous glands opening through the cuticle. At the anterior end of the body, embedded in the protoplasmic tunic, is the brain. The alimentary tract occupies the centre of the body. It has an oval opening: the tract itself consists largely of protoplasm, which even protrudes, pseudopodia-like, from the oval opening. A pair of ovaries and a testis are present. Yellow cells are scattered quite irregularly throughout the body. The systematic position is doubtful. The author suggests that it may be a free-living Cestode.—On the true teeth and on the horny plates of Ornithorhynchus, by E. B. Poulton, M.A. (plates 2–4). The species of Ornithorhynchus have always been described as without true teeth; but, as is well known, they possess eight horny plates—two on each side of each jaw. True teeth are, however, developed at an early stage beneath the horny plates; there are certainly three on each upper maxilla, and while two only have been proved to exist on each of the lower maxilla, it seems extremely probable that an additional pair will be found. The position and structure of these teeth are eminently mammalian, and are treated of in detail. The horny plates gradually intrude into the alveoli of the true teeth, which, ceasing to come to the surface, are absorbed, so that in the adult animal the bone and the under surface of the epithelium are in close proximity.—Note on the fate of the blastopore in *Rana temporaria*, by H. Sidebotham (plate 5). Differs from Balfour in concluding that the neural folds do not inclose the blastopore, the closure of the blastopore being effected subsequently to the meeting of the neural folds; and still more from Spencer, inasmuch as the anus is not derived from a persistent blastopore, but is formed from an independent proctodæal invagination.—Morphological studies: No. 1, the parietal eye of the Cyclostome fishes, by Dr. J. Beard (plates 6 and 7). Describes the parietal eye in the Ammocœtes of *Petromyzon planeri* in its adult form, also in Myxine.—On some Oigopsid cuttle-fish, by F. Ernest Weiss (plates 8–10). A very interesting study of some Mediterranean cuttle-fish.—On the organ of Verrill in Loligo, by M. Laurie (plate 11). An examination of the structure of this organ proved it to be glandular.

In the *Journal of Botany* for July, Mr. George Murray begins a list of the Marine Algæ of the exceedingly rich West Indian region; Mr. F. J. Hanbury describes some forms new to Britain of the very difficult genus *Hieracium*; and Mr. W. B. Grove a new genus of Fungi, *Pimina*, belonging to the Hyphomycetes, parasitic on another Fungus on the leaves of passion-flowers near Dublin.

In the *Botanical Gazette* for June, Mr. Charles Robertson begins a paper having for its object an attempt to explain the origin of the zygomorphic form in flowers, on the principle of natural selection. Herr A. F. Foerste describes a number of structures adapted to cross-fertilization in American flowers; and Mr. F. H. Knowlton a new fossil *Chara* from the Lower Tertiaries in Utah.

American Journal of Science, July.—Upon the relation which the former orbits of those meteorites that are in our collections, and that were seen to fall, had to the earth's orbit, by H. A. Newton. We printed this paper on July 12 (p. 250).—History of changes in the Mount Loa craters (continued), by James D. Dana. This paper deals mainly with Mokuaweoweo, the summit crater of Mount Loa. The history is given of its eruptions from 1832 to 1888, and the subject is illustrated with three plates, giving maps of the island of Hawaii and of Mokuaweoweo with two views of a lava fountain at the eruption of January 1887. The paper is followed by a communication from W. T. Brigham and J. M. Alexander on the summit-crater of Mount Loa in 1880 and 1885.—On an explanation of the action of a magnet on chemical action, by Henry A. Rowland and Louis Bell. These researches have reference to Prof. Remsen's discovery that magnetism has a remarkable action on the deposition of copper from one of its solutions on an iron plate, and to Prof. E. L. Nichols's inquiry into the action of acids on iron in a magnetic field. Their conclusions differ from those of Nichols, inasmuch as they give the exact mathematical theory of the action, while Nichols gives no theory, and does not notice the action of points.—Wave-like effects produced by the detonation of gun-cotton, by Charles E. Monroe. It is suggested that, in the curious phenomena here described, a means may be found for distinguishing between, and perhaps measuring the effects of, different detonating explosives.—A mode of reading mirror galvanometers, &c., by Dr. R. W. Willson. Although less accurate than that of telescope and scale, the method here proposed is stated to be often more convenient.—Bertrandite from Mount Antero, Colorado, by Samuel L. Penfield. The specimen of this rare mineral here studied was selected from some materials collected last summer at Mount Antero, in the search for specimens of phenacite. Its hardness is determined at 6–7, and specific gravity 2·598; while analysis yielded SiO_2, 51·8; BeO, 39·6; H_2O, 8·4; CaO, 1·0.—W. W. Dodge determines some localities of post-Tertiary and Tertiary fossils in Massachusetts; E. O. Hovey describes a Cordierite gneiss from Connecticut; and W. Hallock has a short note on the flow of solids.

The original articles in the *Nuovo Giornale Botanico Italiano* for July comprise a description, with plate, of a singular proliferous specimen of an *Agaricus*, by Signor U. Martelli; a summary of the characters of twenty-two of the principal varieties of the vine grown in the neighbourhood of Arezzo, by Signor L. Macchiati; and contributions to the flora of Massana, by Signor U. Martelli. In the Reports of the Proceedings of the Italian Botanical Society, is an interesting article by Signor G. Arcangeli, on Kefir, an alcoholic and effervescing drink, prepared in the Caucasus by the fermentation of cows' milk. The author confirms the statement of previous observers that in the fermented liquid there are always found a *Saccharomyces* very closely allied to *S. cerevisiæ*, and several Schizomycetes. The organism of the latter class described by previous writers as *Dispora caucasica*, and regarded as peculiar to this kind of fermentation, he identifies with *Bacillus subtilis*, which is accompanied by *B. acidi lactici*. Signors Martelli and Macchiati contribute papers on the fresh-water diatoms of the district of Modena.

Revue d'Anthropologie, troisième série, tome iii., quatrième fasc. (Paris, 1888).—Continuation of the stratigraphic palæontology of man, by M. M. Boule. In this essay the writer treats of the most recently established conclusions regarding the chronological order of the erratic deposits of the valleys of the Rhone, the Saône, and the Ain, which belong to the Quaternary and the Upper and Middle Pliocene ages. He agrees with the generally accepted opinion that the existence of interglacial deposits has been established by scientific evidence, while the identity of the animals and plants everywhere found in these beds prove that they must be nearly contemporaneous. The discovery last year by M. Tardy of a stone implement of the Saint-Acheul type, which was embedded in the alluvial banks of the Ain, and near intact moraines, would seem to connect the presence of man with one of these interglacial periods, while Dr. Penck has shown that each retrogression of a glacier corresponds to a period of alluvial deposit in valleys. Passing from the Alps to the Pyrenees, M. Boule, again following the same authority, shows that, while in the former region there is at many points evidence of repeated glaciation, in the latter the moraines rest directly on ancient rocks. Numerous other difficulties surround the question of glaciation in the Pyrenean range, and the interest of M. Boule's essay depends largely upon the care with which he has sifted the evidence derived from the numerous writers to whom he refers; and the English student will find this section of his work a useful guide to the bibliography of the subject in regard to Auvergne, as well as to the Swiss and French Alps.—The Afghans, by M. L. Rousselet. The excessive admixture of races which is to be found in the land of the Afghans is considered by the author as one of the most curious features of their ethnic history. The physical characteristics of the Afghans of Cabul and Candahar point to an Aryan origin, and would seem to ally them with the Sikhs and Rajputs of North-Western India; while the occasional appearance among the inhabitants of the larger cities of what is commonly known as the Jewish type of face is, according to M. Rousselet, sufficiently explained by the important part which from the earliest period of Islamism Arabs have taken in converting the Afghans to the faith of the Prophet. From Chinese authorities we learn, moreover, that before the middle of the sixth century invaders of a Turcoman race had entered the land of the Afghans, and subjugated some of its tribes. In the tenth century another Turcoman invasion confirmed the domination of the Mohammedans, and since then the Koran has constituted the national code; but, although of the Sunnite sect, the upper classes adhere to the tongue of their heretical neighbours, the Chiite Persians. The theory advocated by many English writers, that the Afghans are descended from the ten lost tribes of Israel, is treated by the writer as unworthy of all serious consideration. He cannot see in this people, of variously composed ethnic elements, anything that demands the establishment of a far-fetched theory to explain their history or character; but he thinks that, in spite of their want of national cohesion, they may—through their love of freedom, the independence secured to them by their geographical position, and their warlike instincts—at no very distant date be called upon to decide the fate of India.—Contributions to the history of anomalous muscles of the neck and back, by M. Ledouble. In this paper the examples cited of such anomalies have been principally taken from the printed reports of Mr. John Wood, Profs. Macalister, Flower, Huxley, &c.—Notes on the Département de l'Ain, by Dr. Aubert. These notes supply an interesting account of the mode of formation and nature of the innumerable ponds and marshes which long gave so peculiar a character to the districts of Dombes, Bresse, and Bugey, in which the great preponderance of standing waters has been for centuries a source of poverty and disease to the unfortunate inhabitants. The existence of such vast areas of more or less deep still-waters is dependent upon a geological cause which must always have been in force, since they owe their origin to the impermeability of the soil beneath them; but it would appear that prior to the fourteenth and fifteenth centuries, when the process of so-called *evolage* and *assec* was first established in these districts, the country was healthier and more populous than it has been in more recent times. This system—which consists in drawing off the waters of certain ponds every third year, and sowing the wet ground with barley and oats after the vast accumulations of fish have been cleared off—naturally gives rise to mephitic effluvia, inducing malarian diseases. These and other evils due to the system of *evolage* had the effect of gradually reducing the population to twenty-four inhabitants to the square kilometre, and giving an average longevity of less than twenty-one years. This state of things, which reached its maximum about the middle of this century, has been steadily improving since the draining of the ponds has been systematically taken in hand. At the present time 6000 hectares of land have already been recovered, and, while fevers have diminished, the tables of conscription show that, whereas in some cantons the numbers of rejections among the recruits were from 80 to 90 per cent. between 1837 and 1847, they had fallen between 1872 and 1886 to below 10 per cent. Dr. Aubert's notes supply an interesting commentary on the practical importance of applying scientific knowledge to the elucidation and modification of the physical condition of the soil, even where this seems to be dependent on apparently unalterable geological causes.—The formula for reconstructing the human figure in accordance with dimensions of the long bones, by M. Topinard. This is little more than a critique of Dr. Beddoe's paper on the stature of the ancient races of England.

Rivista Scientifico-Industriale, June 30.—Note on microscopy (continued), by Prof. Aser Poli. After a rapid survey of the various improvements or modifications introduced by Huyghens, Campani, Ramsden, and other oculists, the author proceeds to examine critically the suggestions recently made by Mr. E. M. Nelson in connection with Campani's eye-piece (Journal of the Royal Microscopical Society, 1887, p. 928). By a simple calculation, in which numerals are substituted for letters in the well-known formula, he shows that the theory is directly opposed to Mr. Nelson's statement. The assertion is also questioned that his theoretical conclusions have been confirmed by practical experiment.

SOCIETIES AND ACADEMIES.

Paris.

Academy of Sciences, August 13.—M. Janssen, President, in the chair.—Remarks in connection with the "Connaissance des Temps pour 1890" (212th year of publication), presented to the Academy by M. Bouquet de la Grye. Amongst the improvements and additions made to this volume are: the semi-diameter of the sun, the duration of its transit, the parallax and aberration for every day in the year, the conditions of visibility of Saturn's ring, and tables for calculating the phases of the solar eclipses for every point on the surface of the globe. By means of certain typographical expedients, all these additions have been made without increasing the size of the volume.—On a general property of elastic solid bodies, by M. Maurice Lévy. A demonstration is offered of the following theorem: If two systems of forces in equilibrium be successively applied to an elastic solid body, whether isotropous or crystallized, free or not (and consequently to a system of similar bodies connected together in any way), then the sum of the work produced by the forces of one of these systems, for the elastic displacements due to the other, is equal to the sum of the work produced by the forces of the latter for the elastic displacements due to the first.—On the influence exercised by antipyretic substances on the quantity of glycogen contained in the muscles, by MM. R. Lépine and Porteret. In a previous note (*Comptes rendus* for April 3, 1888), the authors showed that antipyretic substances act as an impediment to the transformation of the hepatic glycogen into sugar. They now give the results of their further researches on the influence exercised by the antipyrine and acetanilide in determining the proportion of glycogen contained in the muscles. Compared with healthy animals, those intoxicated with these substances have an excess of muscular glycogen varying from 28 to 30 per cent.—On the precautions required to be taken in order to secure good photographs of lightning, by M. Ch. Moussette. An experiment is described, which is intended to show that the defective photographs of electric discharges are mainly due to the vibrations communicated to the apparatus by the trembling of the ground, the force of the wind, or the crash of the thunder. Hence, in order to obtain good impressions, these disturbing elements should be neutralized to the utmost.—Observations of Brooks's new comet, made at the Paris Observatory with the equatorial of the West Tower, by M. G. Bigourdan. This comet was discovered by Mr. Brooks at the new Observatory of Geneva, State of New York, on August 7, 1888. It was faintly visible in Paris on August 9, and the present observations were taken on the three following days.—On amorphous antimony, by M. F. Hérard. The author has succeeded in obtaining directly the allotropic modification of antimony indicated by Gore, and resulting from the decomposition of a chloride, bromide, or iodide of antimony. It takes the form of a gray powder containing 98·7 per cent. of antimony, with density 6·22 at 0° C., and point of fusion about 614°, whereas crystallized antimony melts at about 440°.—On four new titanates of zinc, by M. Lucien Lévy. Since his communication (*Comptes rendus*, vol. cv. p. 378) on a trititanate of zinc obtained by means of fluorides, the author has obtained four other titanates by fusing titanic acid with mixtures of zinc and potassa sulphates These titanates are here described, analyzed, and reduced to their proper formulas.—M. A. Duponchel has a note on a 24-years' cycle of periodicity in the oscillations of temperature on the surface of the globe, based on the records of mean temperatures in Paris from the year 1765 to 1783, and from 1804 to the present time.

August 20.—M. Janssen in the chair.—Note on the adoption of a legal hour in France, by M. Bouquet de la Grye. The Commission appointed in January by the Bureau des Longitudes to inquire into the best means for establishing a common legal hour sent in its Report on June 4, and the Bureau has now invited the Minister of Public Instruction to support a project of law intended to give effect to the recommendations of the Commission.—On inoculation against Asiatic cholera, by Dr. N. Gamaleïa. The substance of this paper has already appeared in the last number of NATURE (p. 395).—Observations of Faye's comet, rediscovered at Nice on August 9, by M. Perrotin. The observations here recorded were taken on August 9 and 10, when the comet was faintly visible, showing a slight central condensation with enveloping nebulosity of circular form, and nearly 1′ in extent.—Observations of Brooks's new comet, made at the Observatory of Nice with the 0·38m. Gautier equatorial, by M. Charlois. The observations are for August 9 and 10, when the comet had a brightness equal to that of a star of the 9th or 10th magnitude, with a faint tail about 5′ long; position-angle, 270°.—On the satellites of Mars, by M. E. Dubois. The two satellites discovered by Asaph Hall on August 11 and 17, 1877, have since been observed by several astronomers, and their elliptic elements recorded in the *Annuaire du Bureau des Longitudes*. How have they hitherto escaped observation, notwithstanding the favourable conditions presented for detecting them? It is suggested that Phobos and Deimos, as they have been named, may perhaps be two small members of the telescopic planetary zone between Mars and Jupiter, which have recently been drawn within the influence of Mars.—Provisional laws determining the subsidence of the land in certain parts of France, by M. C. M. Goulier. A comparison of the altitudes recorded by former and recent surveys seems to indicate a progressive sinking of the surface in the direction from south to north, where the discrepancy amounts to 0·78m. Although the available data are still insufficient to determine the laws regulating this vertical movement, it appears no longer doubtful that subsidence and upheaval take place not only along the seaboard, but also in the interior of the continents to a much greater extent than has hitherto been suspected.—On the vapour-tensions of solutions made in alcohol, by M. F. M. Raoult. His further experiments here described enable the author to generalize the law already formulated by him (*Comptes rendus*, May 23, 1887) to the effect that one molecule of a non-saline fixed substance dissolved in 100 molecules of any volatile liquid, diminishes its vapour-tension by a constant quantity corresponding to about 0·0105 of its value.—Experiment on the treatment of the potato disease, by M. Prillieux. A mixture of 6 parts of the sulphate of copper and 6 of lime to 100 of water (the "Bordeaux broth") has been applied with complete success to some potato plants at Joinville-le-Pont attacked by Peronospora. But to be efficacious the remedy must be applied either as a prophylactic or in the early stages of the disease.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Building of the British Isles: A. J. Jukes-Browne (George Bell).—Annales de l'Observatoire Impérial de Rio de Janeiro, tome iii.: Passage de Vénus, 1882 (Rio de Janeiro).—Planetary and Stellar Studies: J. E. Gore (Roper and Drowley).—History of Modern Philosophy; Descartes and his School: K. Fischer, translated by J. P Gordy (Unwin).—Encyklopædie der Naturwissenschaften, Erste Abthg., Liefg. 55, 56, 57; Zweite Abthg., Liefg. 48 (Williams & Norgate).—iii. Jahresbericht (1887) der Ornithologischen Beobachtungsstationen im Königreich Sachsen: Dr. A. B. Meyer und Dr. F. Helm (Dresden).—The Species of Ficus of the Indo-Malayan and Chinese Countries, Part 2: G. King (Calcutta).—A New Era of Thought: C. H. Hinton (Sonnenschein).—The Nature of Harmony and Metre: M. Hauptmann; translated and edited by W. E. Heathcote (Sonnenschein).—Magnetical and Meteorological Observations made at the Government Observatory, Bombay, 1886 (Bombay).—The Principles of Manure and Luxuriance in Plant Life: W. K. Fulleylove (Birmingham).—A Propos des Chatiments dans l'Éducation: F. Hément (Paris).—Inō Chūkei, the Japanese Surveyor and Cartographer: C. G. Knott.—Anniversary Address delivered to the Royal Society of New South Wales, May 2, 1888: C. S. Wilkinson.—Proceedings of the Liverpool Naturalists' Field Club, 1887 (Liverpool).—Boletin de la Academia Nacional de Ciencias en Córdoba, Tomo x. Ent. 2a. (Buenos Aires).—Third Annual Report of the City of London College Science Society, 1887–88 (London).—Abstract of Proceedings of the South London Entomological and Natural History Society, 1888 (London).—Journal of Physiology, August (Cambridge).

CONTENTS.

THURSDAY, SEPTEMBER 6, 1888.

GEOLOGICAL NOMENCLATURE.

Les Dislocations de l'écorce terrestre : Essai de Définition et de Nomenclature. Texte en français et en allemand ; Synonymie en français, allemand, et anglais. Par Emm. de Margerie et Dr. Albert Heim. Publié aux frais de la fondation de X. Schnyder de Wartensee. (Zurich : J. Wurster and Co., 1888.)

AT the meeting of the International Congress of Geologists which is to be held in London during the autumn of the present year, many praiseworthy attempts will doubtless be made to bring about some kind of uniformity in the nomenclature adopted by workers in different countries. It is doubtful, however, whether any conferences or discussions are more likely to contribute to this much-desired object than the work now before us. The writers of this essay are singularly well qualified for the important task they have undertaken. Prof. Heim, of Zurich, the author of the well-known "Mechanismus der Gebirgsbildung," and other works on orographic geology, is responsible, as we are informed in the preface, for the scientific discussions ; while M. Margerie has taken charge of the literary portion of the work—a task for which a wide knowledge of geological literature in many languages so admirably fits him.

The book was prepared for press in 1885 and 1886, but considerable difficulties were found in the way of its publication ; there fortunately exists, however, at the disposal of the Municipal Library of Zurich, a fund bequeathed by the late Xavier Schnyder von Wartensee, a musical composer, the yearly proceeds of which may be devoted to the publication of scientific works. The proceeds of this fund for the present year having been very judiciously applied to defray the cost of the book before us, the printing was undertaken by the well-known firm of Wurster and Co. M. Margerie has added a supplement bringing the work as nearly as possible down to the date of publication, but is compelled to state his regret in the preface that some valuable memoirs bearing upon the questions discussed (and notably Mr. Mellard Reade's "Origin of Mountain Ranges," which was some time ago noticed in NATURE) did not reach him in time to be utilized as he could have wished. In spite of these frankly acknowledged omissions, however, everyone who uses this work—and it is one which is almost indispensable to the student of the ever-accumulating mass of geological literature—will acknowledge the thoroughness with which the scientific literature of our own country and of the United States, as well as of France, Germany, Italy, and Scandinavia, has been ransacked by the indefatigable authors.

The work is divided into three principal sections, the first dealing with the dislocations resulting from vertical movements of the earth's crust, the second with those produced by horizontal thrusts, and the third with the internal results of the deformation of rock-masses. Exception may be taken to this distribution of the subject, and indeed no classification of the phenomena that could possibly be suggested would be likely to command universal assent, yet we think no better arrangement of the matter contained in this work could have been well devised. Although there are not wanting cases in which we find links between the comparatively simple vertical displacements of little-disturbed areas and the complicated over-folding and over-faulting of mountain ranges yet in the majority of cases the ordinary faults of the former and the grand and exaggerated reversed faults of the latter are as distinct in their distribution as they appear to have been in their mode of origin.

In the first section, the general characteristics of ordinary faults are discussed, as well as the classification of the different types of such faults and of simple flexures, and then the modes of grouping of such faults and their mode of origin are considered. As many of the English, French, and German terms employed in the definition of faults have originated with miners, and are of a provincial character, the exact sense in which they are used cannot be found explained even in the best dictionaries ; hence a very great service is rendered to the geological reader by the care and thoroughness with which the authors of this essay have sought out and explained the synonymous words in the three languages.

It is when we come to the second section of the work, however, that we are impressed with the fullest sense of our indebtedness to MM. Heim and Margerie for removing obstacles to the mutual appreciation by the geologists of different countries of the labours of their fellow-workers.

More than forty years ago the brothers Rogers, in working out the geology of Pennsylvania, first showed what are the essential features in the structure of great mountain ranges. They described with great clearness the succession of great folds, "the axis-planes" of which had been pushed over into a nearly horizontal position ; and others in which, by a still further movement, fracture had taken place along the axis-plane of the folds, leading to the upper limbs of the heeled-over and compressed arches being driven bodily for vast distances over the lower limbs. They described one of these exaggerated reversed faults or overthrusts in Pennsylvania as extending along a line twenty miles in length, with a displacement of five miles, while another similar rent was traced in Virginia for the distance of eighty miles. Henry Rogers saw clearly how these great dislocations enable us to explain the "fan-structure" and other remarkable appearances that had been described by De Saussure, Studer, and other pioneers in the study of Alpine geology ; while James Hall, Dana, Vose, and other American geologists found in the structure of the Appalachians a key to the great problem of the origin of mountain chains. More recently the investigation of the Western Territories of the United States has supplied the able geologists of America with many beautiful and instructive illustrations of the same phenomena.

The light thrown upon the structure of mountain chains by the study of the Appalachians soon began to influence the geologists of the Old World. Lory, Baltzer, Heim, and others, showed that in Dauphiny and in Switzerland "over-folding" and "over-faulting" are the great characteristics of Alpine structure, and they added much to our knowledge of the causes by which these structures are produced.

At the outset of these investigations upon the structure and origin of mountain chains, English geologists were conspicuous not only by the clearness of their views but by the skilful manner in which they applied the new principles to the explanation of our own mountain masses, especially those of the Scottish Highlands. Daniel Sharpe demonstrated the essential points of resemblance between the structure of the mountains of Scotland and those of Southern Europe; while Scrope and Darwin went still further in insisting that the intimate structure or foliation of the rock-masses of our own and other mountain chains must be attributed to the mechanical effects of the great movements to which they have been subjected. Unfortunately the great influence of Murchison, backed as it was by the authority of the officers of the Geological Survey, threw back the advance of English geology in this direction for nearly a quarter of a century. The doctrines that the rocks of the Highlands were in an essentially undisturbed condition, and that in them the planes of stratification and foliation were coincident, were backed by such a weight of authority, that for a time they overbore all opposition. To the labours of Prof. Lapworth we are indebted for initiating the great reaction against the mischievous teachings of this school; while Messrs. Peach and Horne have more than atoned for the evil done by their predecessors, by the energy and zeal with which they have sought to neutralize the effects of those teachings. It is a fortunate circumstance that these patient researches have been carried on in the very districts which had been appealed to as affording the strongest support to the erroneous interpretations.

In the second section of the work before us the various terms employed by Rogers and the American geologists, by Lory, Baltzer, Heim, Suess, Brögger, Reusch, and other Continental writers, as well as by Lapworth, Geikie, Peach, and Horne, are all brought into clear relation with one another. Where necessary the complicated effects of great mountain movements are illustrated by sketches, and the most invaluable aid is thus afforded to the student who seeks to make himself acquainted with and to compare the remarkable results attained by the workers in distant areas. Especially interesting are the observations upon the intricate phenomena displayed in cases where rocks that have been sheared and foliated during one period of mountain-making are subjected to a second process of the same kind at a long subsequent period. We regret that the space at our command forbids us from following the authors into some of these interesting questions.

The important problems connected with the changes in the internal structure of rocks resulting from the movements to which they have been subjected occupy the authors only so far as is necessary to fix the terms that shall be employed in describing the effects produced. The relative merits of such terms as "pressure metamorphism," proposed by Prof. Bonney; of "pressure-fluxion," by the late Prof. Carvill Lewis; of "dislocations-metamorphism," by Prof. Lossen; of "mechanical metamorphism," by Baltzer; of "metamorphism by friction," by Gosselet; and finally, of "dynamo-metamorphism," recently suggested by Prof. Rosenbusch, are all impartially considered. Whatever be the term eventually chosen to express the important effects produced by the internal movements—the "flowing"—of rock-masses, we can only rejoice that the ideas so ably advocated long ago by Scrope and Darwin are now beginning to meet with such wide and general recognition. Problems which in the days of these pioneers of geological thought were absolutely insoluble are now brought within the range of practical research. Lehmann, Lossen, and a host of other workers are showing us how by the application of microscopic methods the paramorphic changes and the mutual chemical reactions of minerals in a rock subjected to external stresses and internal movements may be clearly followed step by step; while the physical investigations of Daubrée, Tresca, and Spring afford to us the promise that the actual causes of the phenomena so carefully observed will not long remain hidden from our view.

The numerous workers in all the great centres of thought, whose attention and study are now concentrated upon these grand and fascinating problems, will welcome the work before us as supplying a want that has been widely and deeply felt.

JOHN W. JUDD.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Lamarckism *versus* Darwinism.

IN his first letter Dr. Romanes stated that I had accused him without evidence. In the second letter he repeats the statement in other words. The answer to both statements will be found in my last letter.

Dr. Romanes will not have replied to my first letter until he explains or expresses regret for his unfairness to Dr. Weismann.

Oxford, September 3. EDWARD B. POULTON.

The Zodiacal Light and Meteors.

I HAVE had the opportunity of looking at Mr. Maxwell Hall's letter (NATURE, vol. vii. p. 204), referred to by Mr. Mattieu Williams (May 31, p. 102), and find that it will not in the least bear out the suggestion made by the latter. Hall's observation was evidently not of any "spurious zodiacal light," but of the ordinary zodiacal light in the form called by some writers the "zodiacal band," though perhaps especially bright. Its position, also, as observed by Hall, was quite different from that which could be occupied by a stream of meteors from Biela's comet.

As regards Hall's theory, which he there propounds for the form of the zodiacal light, it has not met with acceptance, as writers in general consider the ordinary theory of the zodiacal light, viz. that it consists of a continuous disk, whether of meteors or any other substance, in which the sun is central, is sufficient to account for the appearances described by Maxwell Hall and other observers.

T. W. BACKHOUSE.

Sunderland, August 31.

THE SERVICES OF CATHOLIC MISSIONARIES IN THE EAST TO NATURAL SCIENCE.

M. ARMAND DAVID, the well-known Lazarist missionary and man of science, has published a series of articles in the recent numbers of *Les Missions Catholiques* of Lyons on the services rendered to the natural sciences by the missionaries in the Far East. The following is a summary of these long and instructive articles.

It is a common mistake that Catholic missionaries are engaged in proselytizing, and in proselytizing only. Undoubtedly the original motive has been to convert pagan nations to Christianity; but, as will be shown, they have worked in other channels with very great success. Accounts of scientific work like that of the writer are not common, because the missionaries are so few that they have very little time to devote to anything outside their religious duties. The advantages of missionaries preceding the ordinary travellers are well known, and have been recognized by various learned Societies. It is, however, of Eastern Asia in particular of which M. David proposes to treat—that is, of China, which contains a third part of the population of the earth, and which is attracting more and more attention every day. The enemies of the Catholic clergy compare the present missionaries in China very unfavourably with the Jesuits who shone at Pekin in the seventeenth and eighteenth centuries. It is undoubted that the Jesuit fathers of Pekin bore an exceedingly high reputation in science and art, and that they produced very considerable results in almost every branch of human knowledge. They completed the most colossal geographical work that has ever yet been seen, by making a complete chart of the Chinese Empire. The "Lettres Édifiantes," the "Mémoires des Missionaires Jésuites de Pékin," the great works of Father Duhalde and of Father de Mailla show the immense mass of matter they have written upon almost every subject relating to the Chinese Empire. But, it is asked, why speak of the great achievements of the past? They only accentuate the total absence of any scientific labours at the present time in China. M. David has several answers to this question. (1) Formerly the Academies and learned Societies of Europe could communicate only with the missionaries on questions relating to China; no other travellers had then found their way into the Celestial Empire; and it was to aid this communication that the Catholic kings helped the missionaries with their protection and their money, as well as from religious motives. (2) The missionaries knew that they were compelled, in order to get permission to remain in China, to make their services indispensable to the emperor; and thus they put all their knowledge and skill at his service. (3) Whilst only a small number of missionaries thus resided at Pekin, and gained and kept the confidence of the emperor by their pursuit of astronomy, geography, and the arts, the rest, by the favour in which their brethren stood, got permission to preach throughout China.

St. Francis Xavier, the apostle of India, died without being able to enter China. Father Riccí, who entered it in 1580, led to Pekin quite a phalanx of eminent men, to occupy the posts of honour near the emperor. These high positions did not, however, prevent the missionaries from labouring in the cause of Christianity, and founding many Christian establishments. Amongst them were the Fathers Verbiest, Schall, de Premare, Gaubel, Amyot, and many others. The suppression of the Jesuit order stopped their work in China, and the Lazarists, who were sent to succeed them, and who had in their ranks men like MM. Raux, Ghislain, Hanna, and Lamiot, were themselves soon swept away by a revolution. The persecution soon became general in China, and some priests who were able to elude the edicts and remain in the country at the cost, very often, of their lives, were fully occupied without attending to scientific studies. The same was the case with their immediate successors, who were sent by various Societies to collect and strengthen the scattered congregations. Afterwards when the Anglo-French expedition procured freedom of conscience for the Christians and liberty for the missionaries to remain in China, things were very different from what they had been under the Emperors Kang-hi and Kien-lung. The thread of the scientific labours of the old fathers at Pekin could not be picked up. For, on the one hand, China was now in communication with the rest of the world, and had not the need nor the desire to have recourse to the missions for their learned and scientific men; and, on the other hand, the Christian missionaries and their flocks now enjoyed toleration, and the priests had nothing to gain by imitating their great predecessors in gaining the favour of the emperor. Besides, European diplomatists did not look with a favourable eye on the influence that would be acquired by priests over the emperor if they accepted official posts. The Jesuit fathers, however, who had returned to China when their suppression had been annulled, did not completely separate themselves from their former studies, but continued them as far as their changed condition would allow. For example, in their college of Zikawei, near Shanghai, they succeeded in establishing a very important meteorological observatory, whence Father Dechevrens regularly sends his observations to the men of science all over the world; natural history owes much to the persevering labours of Father Heude, who has published a work on the "Mollusques fluviatiles et terrestres" of Central China, and others on the stags and tortoises of China. The able draughtsman, Father Rhatouis, helped Father Heude by drawing the excellent illustrations of these books, some of which were printed in the Jesuit establishment in China. In other parts of the country, many of these missionaries give themselves up to forming and sending to our Museums collections of plants and animals. At Kwei-chow, Abbé Perny, of the Foreign Missions, put together a very interesting collection of plants, which, with other articles of value, he has presented to the Jardin des Plantes. He introduced into France the great silk-worm that bears his name (*Attacus pernyi*), and which already is reared in the open air on the oak-trees of the more temperate regions of France. On his return from China, Abbé Perny published a Chinese grammar and vocabulary, and many works on the productions of the Far East. From Tibet, Mgr. Chauveau and his successor, Mgr. Biet, and above all M. Desgodins, have sent to Europe many precious documents and several collections of animals, which give us an idea of the physical condition of that almost impenetrable region. M. Furet in Japan, M. Larnaudie in Siam, M. Pourthié in Corea, and M. Bon in Tonquin, and several others, have all in the respective countries of their adoption studied the geography and the natural history, and have sent their scientific collections to enrich our public and private establishments. At Yun-nan, M. Delavay, of the Foreign Missions, has given up for many years all his available time to the study of the plants of this unexplored province with the most remarkable zeal and success. The plants which he has already sent to the French Museum are the most important that have ever been sent from China to Europe, and botanists are surprised at the number of new species they contain. An account of these new species has been prepared by M. Frauchet, and will shortly be published in a big octavo volume. M. David prides himself on being the cause of M. Delavay following these botanical pursuits which have so enriched science. They met accidentally at Hong-Kong, and after some trouble M. David succeeded in inducing him to become a correspondent of the Jardin des Plantes. The Professors of that establishment have been so satisfied with the labours of M. Delavay that they have sent him one of their decorations with several money grants to help him to continue his fruitful researches. A few facts will show the value of the labours of this gentleman. Formerly only four or five Chinese representatives of the class *Rhododendron* were known, but the new species found by M. Delavay, added to those found by M. David at Moupinn, amount to forty-five. So, only one Chinese primrose was known, but now more than thirty new pecies have been classified by M. Delavay. Other missionaries besides those of China are actively engaged in the cause of science; for example, Father Montrouzier has studied the fauna of several of the islands of Oceania, and Fathers Duparquet, Augouard, and Le Roy, have sent from Africa many valuable collections. Our Museums and our naturalists have also received from the interior of America many objects more or less important, but chiefly many remarkable Coleoptera and Lepidoptera from MM. Sipolis, Gaujon, and Dorme, French Lazarists, who are quite at the head of the ardent collectors in the New World. To return to China, through the good offices of the Franciscan missionaries of Shen-si, M. Romanet du Cailland was able to obtain and introduce to France several new species of vine which have been cultivated under the names *Vitis Romaneti*, *Vitis Pagnuccii*, *Spinovitis Davidis*. This last species was found by M. David in a wild state in the central mountains of Tsin-lin, and is notable for having its stems covered with thorns. In spite of its somewhat aromatic flavour, it is well adapted for wine-making.

M. David then proceeds to particularize his own labours, and before doing so he gives a short history of his life, into which we shall not follow him. Shortly after the Anglo-French expedition to China he was ordered by his superiors to proceed to that country. Before setting out he was advised by several members of the Institute, amongst them being MM. Stanislas Julien, E. Milne-Edwards, Élie de Beaumont, and Decaisne, to make periodical reports. When he had settled down at Pekin in the year 1862, he began to explore the surroundings of Pekin to prepare materials for a natural history collection, and to send reports and specimens to the Jardin des Plantes. His first consignment of plants and animals was highly praised by the authorities of this institution, and grants of money were sent him to help him to proceed. The increasing importance of the results obtained in China made the Professors of the Museum believe that it was an Eldorado for naturalists, and accordingly they begged the Superior-General of the Lazarists to permit M. David to explore the lesser-known provinces of China. M. Etienne consented readily, chiefly because the request was made through the Government itself; and the Minister of Public Instruction officially styled M. David's proposed journey a scientific mission, and supplied the necessary funds. With regard to the collections sent home by him, he says that only zoologists can appreciate the great work of M. Milne-Edwards, entitled "Recherches sur les Mammifères," which, with the exception of a single species, treats of Chinese animals. The greater portion of these were sent by M. David, the new species alone amounting to sixty-five. One of the most remarkable of these is the *Semnopithecus roxellana*, a curious monkey with a nose very much turned up and a green face, with his back ornamented with long brown and white hair, whose haunts are in the cold forests of Tibet. It is a sort of counterpart of the long-nosed monkeys of Borneo. Besides this animal, China supplied two others, one of which was capable of bearing the severe winters of the north of Tchely, to which point its habitat extends. Another important discovery of the Tibetan region is the extraordinary *Ursus melanoleucus*, for which there was no generic name. The *Ailuropus melanoleucus* appears to be of great rarity in the very small region it inhabits. All the Museums of the world envy the Jardin des Plantes the possession of four specimens—the only ones M. David met. In Tibet also he saw the *Nectogale elegans*, a new kind of aquatic insectivorous animal, the hair of which assumes all the colours of the rainbow when the little creature is in the water. He also secured several varieties of this animal. In the lofty forests of Moupinn he found the *Budorcas*, a large ruminant of a grayish-white colour, with no tail and with immense horns. The hunters of the country regard this animal as the tiger is regarded in India. In spite of its heavy build it scrambles over the most rugged rocks as lightly as a chamois. In almost every district in China he came on some treasure. The deer with large hoofs and a long tail (*Elaphurus davidianus*) is now pretty well known; but the species is, unfortunately, threatened with extinction in China. In the genus *Mus* alone he got twenty-seven species. He noted down two hundred species of *Mammiferæ*, and in this number there are hardly five or six, omitting the domestic species, which appear identical with their species in Europe.

With regard to the birds of China, M. David has prepared, with the help of M. G. Masson, a book on them, in which he recognizes 807 species either living in China or coming there regularly. Amongst the greatest novelties he mentions the large *Lophophorus* of Tibet, which lives at a height of above 12,000 feet; the three known *Crossoptilon*, of which one is white, another blue, and the third black and white; the *Tragopan*, with a large many-coloured band around the throat, and its head ornamented with two very thin, blue, and fleshy horns; two *Eulophes*, crested pheasants, which are the most appreciated dish by gourmands; the sacred pheasant, with a tail over six feet long; the Amherst pheasant, now become, like the preceding, a common bird in the parks; and a new species of pheasant, dark-coloured, and always living under trees. All these birds, and hundreds of others from the same source, are exhibited in the French Museum. Some of them, according to the method common among naturalists, are named after the discoverer. Thus the *Cygnus davidi*, a very rare swan with red legs, and the *Pterorhinus davidi*, a kind of mocking-bird captured in the woods in the neighbourhood of Pekin; the *Sygrnium davidi*, a nocturnal rapacious bird of Tibet, described by Mr. Sharp, of the British Museum. M. H. Milne-Edwards, Professor at the Sorbonne, has also affixed M. David's name to two new species which he has described, *Carpodacus davidianus* and *Oreopneuste armandi*. China has not our sparrow, chaffinch, goldfinch, or linnet; our warbler, redbreast, and nightingale are unknown; their thrushes, blackbirds, tomtits, and crows, differ very much from ours. In fact, speaking generally, there is only about one-fifth of the Chinese birds found in Europe, and the greater part of these are very different in the two regions. The Eastern *Gallinæ*, *Insectivores*, and *Rapaces*, have scarcely any species like them in our continent. A very remarkable fact is that we find certain groups of birds within certain narrow limits where they are represented by numerous species, whilst they are totally absent from all other parts of the earth, even from those parts where it would be quite possible for them to live. Thus there are forty kinds of the beautiful pheasant class, all grouped around Tibet, while there is not a single member of the class in any other quarter of the globe. So the *Crateropodes*, of which there are thirty or forty species in China, do not appear to have any representatives in Europe. These and other facts furnish M. David with what he considers unanswerable objections to the theory that they were all created *ab origine*. Is it not more reasonable, he asks, to admit that the principal types of plants and animals having once appeared on earth, where and when it pleased Providence, have undergone slow variations which have divided them by degrees into species and varieties? America has upwards of four hundred species of humming-birds, while there is not a single other specimen in the rest of the tropical world, where those little creatures could live equally well. Every class of the animal kingdom, he says, furnishes similar examples and analogous facts.

The subject of reptiles, *Batrachia*, and fishes, which M. David only worked up slightly, has been carefully pursued by M. Duméril, Dr. Savage, and M. E. Blanchard. The last-named gentleman described before the Academy of Sciences, under the name of *Sieboldia davidiana*, an immense salamander which lives on fish and crabs in fresh water. A skeleton of a salamander, more or less resembling this one, has recently been found in Germany, where it was taken for a fossil man. It is the insect world which supplied M. David with the greatest novelties. Great though the collections sent to Europe are, they are but a small fraction of the riches in entomology that China supplies. The Coleoptera have been described by M. Fairmaire, formerly President of the French Entomological Society, and the Lepidoptera by M. Oberthur, of Rennes, who has the finest collection in France, and perhaps in the world. Amongst insects, more even than amongst animals and plants, there is a large number called by the names of the missionaries who sent specimens of them to Europe. For example, *Cicindela desgodinsii*, *Carabus delavayi*, *Cychrus davidi*, *Nebria chaslei*, *Enoplotrupes largeteani*, *Donacia provosti*, &c., in Coleoptera; and in butterflies, *Anthocharis bieti*, *Armandia thaïdina*, &c. With regard to the vegetable kingdom, the first important work we have on the Chinese flora has been finished this year, and styled "Plantæ Davidianæ." It has been printed at the expense of the State, and is in two quarto volumes, illustrated with forty-five very fine plates, and contains a description of all the new species of plants in M. David's collection, and an enumeration of all the plants collected by him. The collection contains a small proportion only of the plants of China. It should only be regarded as a mere skeleton of the magnificent vegetation of the east-central provinces, but it contains the greater portion of the plants to the north of the empire and in the Mongolian mountains. Collections made by English and Russian collectors do not include many of the specimens found by M. David. Perhaps the most remarkable find was the *Davidia involucrata*—a pretty tall tree with large leaves, for the introduction of which an English amateur has offered a big prize. Our European plants are not at all common in the East. No trefoils are found in China, nor heather, nor broom. There are also many plants there which have no representatives in Europe, but which have representatives in America, as, *Pavia*, *Bignonia*, *Aralia*, *Dielytra*. Northern China, with its dry climate, its cold winter, as cold as that of Upsala, and its summer as warm as that of Senegal, has a poor and little-varied vegetation when compared with the centre and west of the empire. The number of Phanerogams collected by M. David in the north of China did not exceed 1500 species, and he doubts if there are many more.

In geography and geology, besides several occasional reports, the "Archives du Muséum" have published full accounts of his first and second journeys of exploration. These voluminous writings are merely journals written for some friends, for whom he wrote day by day everything that seemed worthy of attention, whether botanical, geological, or geographical, in the extensive regions which for five years he travelled over. Itinerary charts, striking altitudes, up to 15,000 feet, the direction and importance of rivers and mountain chains, the position of the lesser known towns and countries, and of the coal and metal mines—all have been noted down by him. From the writings of M. David, M. Elisée Reclus took many of his observations on the Chinese Empire in vol. vii. of his "Géographie Universelle," and especially the natural history portion of that volume. Similarly Baron Richthofen has derived much of the information in his work on geology from M. David. In Mongolia M. David's guide was Sambdatchiemda, the famous ex-lama described by M. Huc, and this leads M. David to speak of the lamas, and tell some stories about them.

M. David describes a curious meteorological phenomenon observed by him when crossing the top of a mountain about 5500 feet high. A storm had just passed, and a little rain had fallen. The clouds were heavy, and lay on the numerous peaks below his feet like an immense sea of silvery white. Little by little the masses of clouds began to move and to split up here and there. They rose slowly and soon came to the right of M. David, who was journeying from south to north. The wind was blowing from the west, and when the clouds reached the summit of the mountain they could not pass over on account of the opposition of the wind, and there they rested, a huge mass of opaque clouds. The sun was setting on the horizon, and threw the image of M. David on the wall of white clouds, where it was surrounded by two rainbows, or rather two complete concentric circles. This phenomenon lasted nearly half an hour. M. David had been six months in Mongolia when the revolt of the Mussulmans broke out and prevented him from penetrating as far as Koukounoor, and even beyond it, as was his intention. These high Mongolian plateaux are of about three thousand feet above the level of the sea. The population is very sparse, and the fauna and flora but little varied. The remarkable animals most frequently seen in this region are the souslik, or yellow antelope, a kind of little marmot analogous to the prairie dog of America, a brownish weevil, and a curious lizard with round head (*Phrynocephalus*) which is seen everywhere rolling its tail in regular cadences. During the summer the open country is covered either with the blue-flowered iris, or with the liquorice (*Glycyrrhiza echinata*) or the yellow rose. M. David found in Mongolia in a wild state, but very rare, a pretty flowering tree, which the Pekinese cultivate as an ornamental plant (*Xanthoceras sorbifolia*), and which he introduced into France with much success. In his journey he satisfied himself of the existence of wild camels, some of which were afterwards captured by the Russian traveller Prjevalski. M. David spent twenty-five months in Western China. He had intended to spend three years, but his health broke down. In that time he travelled over 2500 leagues. He returned thence to Tien-tsin, fortunately for him after the massacres had taken place, his boat having been delayed on the way.

THE AUSTRALASIAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

SYDNEY, *July* 1888.

THE formation of this Association, which already gives promise of being a great success, was first suggested by Prof. Liversidge, of the Sydney University, during the Exhibition in Sydney in 1879, but matters at that time not being considered quite ripe for it, the formation of the Association was again brought forward through the press in the year 1884. It was then suggested that, as it did not seem likely that the British Association would see their way to visit Australia during the Centennial year, an Australasian Association should be formed, on the same lines as the British Association, in order to bring about a federation or union of the members of the various scientific Societies throughout Australasia.

It was also suggested that the first general meeting should be held in Sydney on the one hundredth anniversary of the foundation of the colony, as it was at that time thought there would be an International Exhibition in Sydney to celebrate that event. In furtherance of this object a preliminary meeting of delegates was held in Sydney in November 1886, the project having met with the approbation and support of almost all the learned and scientific Societies of Australasia.

As this meeting the formation of the Australasian Association for the Advancement of Science was agreed to unanimously, the rules of the British Association being adopted until the first general meeting, which it was decided should be held in Sydney during the year 1888.

In accordance with another resolution passed at the meeting of delegates, the election of officers for the year took place in March of the present year, Mr. H. C. Russell, F.R.S., Government Astronomer, being elected President, Sir Edward Strickland, K.C.B., Hon. Treasurer, and Prof. Liversidge, F.R.S., and Dr. George Bennett, Hon. Secretaries.

The formation of the Council was afterwards proceeded with, each learned or scientific Society electing one representative for every hundred of its members; and the Chief Justice, Minister for Public Instruction, the Chancellor and Vice-Chancellor of the Sydney University, the Mayor of Sydney, and the Presidents of the Royal Societies in other colonies were elected Vice-Presidents for the year.

The Presidents of Sections were then elected, the gentlemen chosen being all resident in other colonies than New South Wales; whilst the Secretaries of Sections, as a matter of necessity, were elected from amongst residents in Sydney.

The Association is hence thoroughly Australasian in its character, and the succeeding general meetings are to take place in turn in the capitals of the other colonies, the executive officers being elected year by year by the colony in which the meeting is held.

The first general meeting is to be held at the Sydney University, the opening ceremony, at which His Excellency the Governor will be present, taking place on Tuesday evening, August 28, when the Presidential address will be delivered.

On the following day the Sectional meetings for the reading and discussion of papers will commence, and it is thought that the principal portion of the business will close with the end of the week.

Up to the present time the titles of about ninety papers have been sent in by gentlemen of distinction in science, literature and art, in the different colonies, and it seems probable that this number will be considerably increased before the meeting.

It may therefore be anticipated that the nature of the work done by the Association during the first year of its existence will be of a highly important and useful character.

The more solid work of the meeting is to be lightened by excursions to various places of interest to geologists, botanists, and others; and efforts are being made to provide for the entertainment and comfort of visiting members, as far as possible, so that they may spend their time to the best advantage.

The various steamship companies have arranged to carry members proceeding to Sydney to attend the meeting at a reduction of 20 per cent. on the ordinary rates, and it is anticipated that liberal concessions will also be granted in the railway fares.

The rules, as already mentioned, are practically the same as those of the British Association, and all who join the Association before the first general meeting in August next become original members, without entrance fee, the subscription of £1 entitling members to receive the publications of the Association gratis.

The number of members at the end of July exceeded 400.

PROFESSOR RUDOLF JULIUS EMANUEL CLAUSIUS.

BY the death of Prof. Clausius, which occurred on August 24 last, science has lost another member of the great triumvirate—Rankine, Clausius, and Thomson—who, upon the foundation laid by the experimental work of Davy and Rumford, the theoretical suggestions of Mohr, Séguin, Mayer, and Colding (which, though resting on imperfect data and defective reasoning, were the results of real scientific insight), and the splendid experimental investigations of Joule, founded and built up the great structure known as the science of thermodynamics.

Clausius was born at Cöslin, in Pomerania, on January 2, 1822. While yet at school in Berlin, he gave unmistakable evidence of the bent of his mind towards mathematics and physics, and on the completion of his University course he became Privatdocent in the University of Berlin and Instructor in Natural Philosophy at the School of Artillery. He very soon gave evidence of his power as an original worker, and some of his earliest papers—"On the Nature of those Constituents of the Atmosphere by which the Reflection of the Light within it is effected," and "On the Blue Colour of the Sky, and the Morning and the Evening Red"—contributed to *Poggendorff's Annalen*, were selected for translation in the first volume of Taylor's "Scientific Memoirs."

In 1857 he was appointed Professor of Natural Philosophy at the Polytechnic School of the Helvetic Confederacy at Zürich. Here he continued his researches in various branches of physics, and among these we may mention, to give some idea of the extent and variety of his investigations, "The Influence of Pressure on the Freezing-point," "The Mechanical Equivalent of an Electric Discharge, and the Heating of the Conducting-wire which accompanies it," "Electrical Conduction in Electrolytes," and "The Effect of Temperature on Electric Conductivity." He also published some short papers on some purely mathematical questions, suggested, however, by physical problems, and some papers dealing with points of what is generally known as physical chemistry.

His attention was then directed towards the dynamical theory of gases, owing to the light which it appeared capable of throwing upon questions of thermodynamics. The dynamical or kinetic theory of gases, which has received such extensive developments at the hands of Clerk Maxwell, Boltzmann, and others, was originally suggested by J. Bernouilli about the middle of the last century; but it was Clausius who first placed it upon a secure scientific basis. In 1866 he published a most important paper "On the Determination of the Energy and Entropy of a Body" (translated in the *Philosophical Magazine*), in which the very valuable and suggestive conception of the entropy of a body was first set forth.

In 1869 he was appointed Professor of Natural Philosophy in the University of Bonn.

Among more recent papers of great importance we may mention the following, all of which have been translated in the *Philosophical Magazine*:—"On a New Fundamental Law of Electrodynamics"; "On the Behaviour of Carbonic Acid in relation to Pressure, Volume, and Temperature"; "On the Theoretic Determination of Vapour-pressure and the Volumes of Vapour and Liquid"; "On the Different Systems of Measures for Electric and Magnetic Quantities"; "On the Employment of the Electrodynamic Potential for the Determination of the Ponderomotive and Electromotive Forces"; "On the Theory of Dynamo-electrical Machines"; and "On the Theory of the Transmission of Power by Dynamo-electrical Machines."

When we consider the far-reaching and fundamental character of these and many other investigations, and the very wide field which they cover, we cannot but wonder at the marvellous energy of the great physicist who has passed from among us. The Royal Society catalogue contains a list of no less than seventy-seven papers published up to 1873, and those published subsequently bring the total number up to considerably over a hundred.

In addition to these there is his great treatise on "The Mechanical Theory of Heat," of which the first volume was published in 1864, and a smaller work, "On the Potential Function and the Potential."

It would be impossible to discuss in detail the portions of thermodynamics specially worked out by Clausius, as his work is throughout closely interwoven with that of Rankine and Thomson, but it will be of interest to quote the following from Prof. Rankine, who in his paper "On the Economy of Heat in Expansive Machines,"[1] says:—

[1] "Rankine's Miscellaneous Scientific Papers," p. 300.

"Carnot was the first to assert the law *that the ratio of the maximum mechanical effect to the whole heat expended in an expansive machine is a function solely of the two temperatures at which the heat is respectively received and emitted, and is independent of the nature of the working substance.* But his investigations, not being based on the principle of the dynamical convertibility of heat, involve the fallacy that power can be produced out of nothing.

"The merit of combining Carnot's *law*, as it is termed, with that of the convertibility of heat and power belongs to Mr. Clausius and Prof. William Thomson; and in the shape into which they have brought it, it may be stated thus: *The maximum proportion of heat converted into expansive power by any machine is a function solely of the temperatures at which heat is received and emitted by the working substance, which function for each pair of temperatures is the same for all substances in Nature.*"

None will regret the loss of Prof. Clausius more keenly than the students of the University of Bonn, where he formed a centre of attraction not only as a great investigator, but as a teacher of almost unrivalled ability. The secret of his powers as a teacher may easily be guessed from the study of his published papers and treatises. Their great characteristic is the direct insight which they give into the very heart of the physical principles under discussion. The author, while showing himself a master of mathematical methods, ever keeps the physical meaning of the symbols before the eye of the reader, and never allows his analysis to carry him away into the regions of mere mathematical ingenuity. In this he was a worthy compeer of some of our own great mathematical physicists, like Thomson and Maxwell, and the greater part of his work has the additional advantage, for the majority of students, of being effected by the aid of comparatively simple analysis.

In 1868, Prof. Clausius was elected a Foreign Member of the Royal Society, and in 1879 he was presented with the Copley Medal, the highest distinction at the disposal of the Society. He was decorated with various civil Prussian and Bavarian orders; and after the Franco-German war, during which he had volunteered to serve as caretaker of the wounded, he received the German decoration of the Iron Cross, and the French decoration of the Legion of Honour.

G. W. DE TUNZELMANN.

THE BRITISH ASSOCIATION.

Wednesday Night.

THE meeting of the British Association which opens to-night, after twenty-four years' absence, in Bath, will be the fifty-eighth. At the meeting of 1864, the President was Sir Charles Lyell, and the occasion was rendered memorable by the presence at once of Dr. Livingstone and Bishop Colenso, both at the time filling a large space in the public eye. Though a vast majority of the members of the Association would prefer to visit Bath to either Birmingham or Manchester, the latter towns possess in Owens College and the Town Hall buildings which offer greater conveniences for the meeting of a scientific Congress. In Bath the Sections will be somewhat scattered. The Physical Science Section meets at the St. James's Hall; the Mechanical Section in the Masonic Hall; the Chemical Section in the Friends' Meeting-House; Geology and Biology are housed at the Mineral Water Hospital, with the Blue Coat School for the sub-sections; Geography at the Guildhall, and Anthropology at the Grammar School; while the President's address and some of the popular lectures, as well as the concluding general meeting, will be delivered at the Drill Hall. The Mayor gives a *conversazione* to-morrow in the Assembly Rooms, and the Chairman and Local Committee give another on Tuesday. A large number of foreign visitors, especially geologists for the International Geological Congress to be held in London on the 17th inst., are expected. Amongst those already arrived are Prince Roland Bonaparte; Profs. Dufont, Gilbert, Capellini, Stephenson, Lory, von Koenen, Frazer, Kalkowsky, and Waagen.

The retiring President, Sir Henry Roscoe, M.P., F.R.S., in introducing Sir Frederick Bramwell, the President-Elect, spoke as follows:—

"My Lords, Ladies, and Gentlemen,—Four-and-twenty eventful years in the history of science have passed away since the British Association last visited the city of Bath. Those of us who were present here in 1864 will not soon forget that memorable meeting. It was presided over, as you all will remember, by that veteran geologist, that great forerunner of a new science of life, Sir Charles Lyell, of beloved and venerated memory. Yes, ladies and gentlemen, it was he who prepared the way by his recognition of the true history of our globe for the even more illustrious Darwin. It was he who pointed out that the causes which have modified the earth's crust in the past are, for the most part, those which are now changing the face of Nature. Lyell was a typical example of the expositor of Nature's most secret processes. His work was that of an investigator of science pure and undefiled, and as such, his life and labours stand for ever as an example to all those who love Science for her own sake.

"But the far-seeing founders of this our British Association were as fully alive to the fact as we, in perhaps our more utilitarian age, can be, that, just as man does not live by bread alone, so it is not only by purely scientific discovery that the nations progress, or that science advances. They knew as well as we do that to benefit humanity the application of the results of scientific research to the great problems of every-day life is a necessity. Hence our founders, whilst acknowledging that the basis of our Association can only be securely laid upon the principles of pure science in its various branches, recognized the importance of the application of those principles in the establishment of a Section which should represent one of the most remarkable outcomes of the activity and force of the nation—a Section of Engineering. It is therefore meet and right that in due proportion this great department of our scientific edifice—a department which, perhaps, more than any other, has effected a revolution in our modern social system—should be represented in our Presidential chair.

"Twenty-four years ago it was pure science that we honoured in Sir Charles Lyell: to-day it is applied science to which we show our respect in the person of Sir Frederick Bramwell. It would ill become me, engaged as I have been in the study of subjects far removed from those which fill the life of an active and successful engineer, to venture on this occasion on a eulogium upon the work of my successor, still less is it in my mind to draw any comparison as to the relative importance to be attached to the work of the investigator, such as Lyell, and to that of him who applies the researches of others to the immediate wants of mankind. It is enough for me, as I am sure it will be for you, to remember that both classes of men are needed for the due advancement of science, and to rejoice that as in former years the names of Fairbairn, of Armstrong, and of Hawkshaw, have adorned our list of Presidents, so in the present instance, this branch of science, which represents lines of human activity rendered illustrious by the labours of many great Englishmen, is to-day represented by our eminent President.

"I have the honour of requesting Sir Frederick Bramwell to take the chair, and to favour us with the Presidential address."

INAUGURAL ADDRESS BY SIR FREDERICK BRAMWELL, D.C.L., F.R.S., M.INST.C.E., PRESIDENT.

THE late Lord Iddesleigh delighted an audience, for a whole evening, by an address on "Nothing." Would that I had his talents, and could discourse to you as charmingly as he did to his audience, but I dare not try to talk about "Nothing." I do, however, propose, as one of the two sections of my address, to discourse to you on the importance of the "Next-to-Nothing." The other section is far removed from this microscopic quantity, as it will embrace the "Eulogy of the Civil Engineer," and will point out the value to science of his works."

I do not intend to follow any system in dealing with these two sections. I shall not even do as Mr. Dick, in "David Copperfield," did—have two papers, to one of which it was suggested he should confine his memorial and his observations as to King Charles's head. The result is, you will find, that the importance of the next-to-nothing, and the laudation of the civil engineer, will be mixed up in the most illogical and haphazard way, throughout my address. I will leave to such of you as are of orderly minds, the task of rearranging the subjects as you see fit, but I trust—arrangement or no arrangement—that by the time I have brought my address to a conclusion I shall have convinced you that there is no man who more thoroughly appreciates the high importance of the "next-to-nothing" than the civil engineer of the present day, the object of my eulogy this evening.

If I may be allowed to express the scheme of this address in modern musical language, I will say that the "next-to-nothing" "motive" will commonly usher in the "praise-song" of the civil engineer; and it seems to me will do this very fitly, for in many cases it is by the patient and discriminating attention paid to the effect of the "next-to-nothing" that the civil engineer of the present day has achieved some of the labours of which I now wish to speak to you.

An Association for the Advancement of Science is necessarily one of such broad scope in its objects, and is so thoroughly catholic as regards science, that the only possible way in which it can carry out those objects at all is to segregate its members into various subsidiary bodies, or sections, engaged on particular branches of science. Even when this division is resorted to, it is a hardy thing to say that every conceivable scientific subject can be dealt with by the eight Sections of the British Association. Nevertheless, as we know, for fifty-seven years the Association has carried on its labours under Sections, and has earned the right to say that it has done good service to all branches of science.

Composed, as the Association is, of a union of separate Sections, it is only right and according to the fitness of things that, as time goes on, your Presidents should be selected, in some sort of rotation, from the various Sections. This year it was felt, by the Council and the members, that the time had once more arrived when Section G—the Mechanical Section—might put forward its claim to be represented in the Presidency; the last time on which a purely engineering member filled the chair having been at Bristol in 1875, when that position was occupied by Sir John Hawkshaw. It is true that at Southampton, in 1882, our lamented friend, Sir William Siemens, was President, and it is also true that he was a most thorough engineer and representative of Section G; but all who knew his great scientific attainments will probably agree that on that occasion it was rather the Physical Section A which was represented, than the Mechanical Section G.

I am aware it is said Section G does not contribute much to pure science by original research, but that it devotes itself more to the application of science. There may be some foundation for this assertion, but I cannot refrain from the observation that, when engineers such as Siemens, Rankine, Sir William Thomson, Fairbairn, or Armstrong, make a scientific discovery, Section A says it is made, not in the capacity of an engineer, and therefore does not appertain to Section G, but in the capacity of a physicist, and therefore appertains to Section A—an illustration of the danger of a man's filling two positions, of which the composite Prince-Bishop is the well-known type. But I am not careful to labour this point, or even to dispute that Section G does not do much for *original* research. I do not agree it is a fact, but, for the purposes of this evening, I will concede it to be so. But what then? This Association is for the "Advancement of Science"—the *advancement*, be it remembered; and I wish to point out to you, and I trust I shall succeed in establishing, that for the *advancement* of science it is absolutely necessary there should be the *application* of science, and that, therefore, the Section, which as much as any other (or, to state the fact more truly, which more than any other) in the Association *applies* science, is doing a very large share of the work of *advancing* science, and is fully entitled to be periodically represented in the Presidency of the whole Association.

I trust also I shall prove to you that applications of science, and discoveries in pure science, act and react the one upon the other. I hope in this to carry the bulk of my audience with me, although there are some, I know, whose feelings, from a false notion of respect for science, would probably find vent in the "toast" which one has heard in another place—this "toast" being attributed to the pure scientist—"Here's to the latest scientific discovery: may it never do any good to anybody!"

To give an early illustration of this action and reaction, which I contend occurs, take the well-worn story of Galileo, Torricelli, and the pump-maker. It is recorded that Galileo first, and his pupil Torricelli afterwards, were led to investigate the question of atmospheric pressure, by observing the failure of a pump to raise water by "suction," above a certain level. Perhaps you will say the pump-maker was not applying science, but was working without science. I answer, he was unknowingly applying it, and it was from that which arose in this unconscious application that the mind of the pure scientist was led to investigate the subject, and thereupon to discover the primary fact of the pressure of the atmosphere, and the subsidiary facts which attend thereon. It may appear to many of you that the question of the exercise of pressure by the atmosphere should have been so very obvious, that but little merit ought to have accrued to the discoverer; and that the statement, once made, must have been accepted almost as a mere truism. This was, however, by no means the case. Sir Kenelm Digby, in his "Treatise on the Nature of Bodies," printed in 1658, disputes the proposition altogether, and says, in effect, he is quite sure the failure of the pump to raise water was due to imperfect workmanship of some kind or description, and had nothing to do with the pressure of the air; and that there is no reason why a pump should not suck up water to any height. He cites the boy's sucker, which, when applied to a smooth stone, will lift it, and he says the reason why the stone follows the sucker is this. Each body must have some other body in contact with it. Now, the stone being in contact with the sucker, there is no reason why that contact should be broken up, for the mere purpose of substituting the contact of another body, such as the air. It seems pretty clear, therefore, that even to an acute and well-trained mind, such as that of Sir Kenelm Digby, it was by no means a truism, and to be forthwith accepted, when once stated, that the rise of water on the "suction side" of a pump was due to atmospheric pressure. I hardly need point out that the pump-maker should have been a member of "G." Galileo and Torricelli, led to reflect by what they saw, should have been members of "A" of the then "Association for the Advancement of Science."

But, passing away from the question of the value of the application of science of a date some two and a half centuries ago, let us come a little nearer to our own times.

Electricity (known in its simplest form to the Greeks by the results arising from the friction of amber, and named therefrom; afterwards produced from glass cylinder machines, or from plate machines; and produced a century ago by the "influence" machine) remained, as did the discoveries of Volta and Galvani, the pursuit of but a few, and even the brilliant experiments of Davy did not suffice to give very great impetus to this branch of physical science.

Ronalds, in 1823, constructed an electric telegraph. In 1837 the first commercial use was made of the telegraph, and from that time electrical science received an impulse such as it had never before experienced. Further scientific facts were discovered; fresh applications were made of these discoveries. These fresh applications led to renewed vigour in research, and there was the action and reaction of which I have spoken. In the year 1871 the Society of Telegraph-Engineers was established. In the year 1861 our own Association had appointed a Committee to settle the question of electrical standards of resistance, which Committee, with enlarged functions, continued its labours for twenty years, and of this Committee I had the honour of being a member. The results of the labours of that Committee endure (somewhat modified, it is true), and may be pointed to as one of the evidences of the value of the work done by the British Association. Since Ronalds's time, how vast are the advances which have been made in electrical communication of intelligence, by land lines, by submarine cables all over the world, and by the telephone! Few will be prepared to deny the statement, that pure electrical science has received an enormous impulse, and has been advanced by the commercial application of electricity to the foregoing, and to purposes of lighting. Since this latter application, scores, I may say hundreds, of acute minds have been devoted to electrical science, stimulated thereto by the possibilities and probabilities of this application.

In this country, no doubt, still more would have been done if the lighting of districts from a central source of electricity had not been, since 1882, practically forbidden by the Act passed in that year. This Act had in its title the facetious statement that it was "to facilitate electrical lighting"—although it is an Act which, even modified as it has been this year, is still a great discouragement of free enterprise, and a bar to progress. The other day a member of the House of Commons was saying to me: "I think it is very much to our discredit in England that we should have allowed ourselves to be outrun in the distribution of electric lighting to houses, by the inhabitants of the United States, and by those of other countries." Looking upon him as being one of the authors of the "facetious" Act, I thought it pertinent to quote the case of the French parricide, who, being asked what he has to say in mitigation of punishment, pleads, "Pity a poor orphan"—the parricide and the legislator being both of them authors of conditions of things which they affect to deplore. I will say no more on this subject, for I feel that it would not be right to take advantage of my position here to-night to urge political economy views, which should be reserved for Section F. I will merely, and as illustrative of my views of the value of the application of science to science itself, say there is no branch of physics pursued with more zeal and with more happy results than that of electricity, with its allies, and there is no branch of science towards which the public look with greater hope of practical benefits; a hope that, I doubt not, will be strengthened after we have had the advantage of hearing one of the ablest followers of that science, Prof. Ayrton, who, on Friday next, has been good enough to promise to discourse on "The Electrical Transmission of Power."

One of the subjects which, as much as (or probably more than) any other, occupies the attention of the engineer, and therefore of Section G, is that of (the so-called) prime movers, and I will say boldly that, since the introduction of printing by the use of movable type, nothing has done so much for civilization as the development of these machines. Let us consider these prime movers—and, first, in the comparatively humble function of replacing that labour which might be performed by the muscular exertion of human beings, a function which at one time was looked upon by many kindly but short-sighted men as taking the bread out of the mouth of the labourer (as it was called), and as being therefore undesirable. I remember revisiting my old schoolmaster, and his saying to me, shaking his head: "So you have gone the way I always feared you would, and are making things of iron and brass, to do the work of men's hands."

It must be agreed that all honest and useful labour is honourable, but when that labour can be carried out without the exercise of any intelligence, one cannot help feeling that the result is likely to be intellectually lowering. Thus it is a sorry thing to see unintelligent labour, even although that labour be useful. It is but one remove from unintelligent labour which is not useful; that kind of labour generally appointed (by means of the tread-wheel or the crank) as a punishment for crime. Consider even the honourable labour (for it is useful, and it is honest) of the man who earns his livelihood by turning the handle of a crane, and compare this with the labour of a smith, who, while probably developing more energy by the use of his muscles, than is developed by the man turning the crane-handle, exercises at the same time the powers of judgment, of eye, and of hand in a manner which I never see without my admiration being excited. I say that the introduction of prime movers as a mere substitute for unintelligent manual labour is in itself a great aid to civilization and to the raising of humanity, by rendering it very difficult, if not impossible, for a human being to obtain a livelihood by unintelligent work—the work of the horse in the mill, or of the turnspit.

But there are prime movers and prime movers—those of small dimensions, and employed for purposes where animal power or human power might be substituted, and those which attain ends that by no conceivable possibility could be attained at all by the exertion of muscular power.

Compare a galley, a vessel propelled by oars, with the modern Atlantic liner; and first let us assume that prime movers are non-existent, and that this vessel is to be propelled galley-fashion. Take her length as some 600 feet, and assume that place be found for as many as 400 oars on each side, each oar worked by three men, or 2400 men; and allow that six men under these conditions could develop work equal to one horse-power: we should have 400 horse-power. Double the number of men, and we should have 800 horse-power, with 4800 men at work, and at least the same number in reserve, if the journey is to be carried on continuously. Contrast the puny result thus obtained with the 19,500 horse-power given forth by a large prime mover of the present day, such a power requiring, on the above mode of calculation, 117,000 men at work, and 117,000 in reserve; and these to be carried in a vessel less than 600 feet in length. Even if it were possible to carry this number of men in such a vessel, by no conceivable means could their power be utilized so as to impart to it a speed of twenty knots an hour.

This illustrates how a prime mover may not only be a mere substitute for muscular work, but may afford the means of attaining an end that could not by any possibility be attained by muscular exertion, no matter what money was expended or what galley-slave suffering was inflicted.

Take again the case of a railway locomotive. From 400 to 600 horse-power developed in an implement which, even including its tender, does not occupy an area of more than fifty square yards, and that draws us at sixty miles an hour. Here again, the prime mover succeeds in doing that which no expenditure of money or of life could enable us to obtain from muscular effort.

To what, and to whom, are these meritorious prime movers due? I answer: To the application of science, and to the labours of the civil engineer, using that term in its full and proper sense, as embracing all engineering other than military. I am, as you know, a civil engineer, and I desire to laud my profession and to magnify mine office; and I know of no better means of doing this than by quoting to you the definition of "civil engineering," given in the Charter of the Institution of Civil Engineers—namely, that it is "the art of directing the great sources of power in Nature for the use and convenience of man." These words are taken from a definition or description of engineering given by one of our earliest scientific writers on the subject, Thomas Tredgold, who commences that description by the words above quoted, and who, having given various illustrations of the civil engineer's pursuits, introduces this pregnant sentence:—

"This is, however, only a brief sketch of the objects of civil engineering, the real extent to which it may be applied is limited only by the progress of science; its scope and utility will be increased with every discovery in philosophy, and its resources with every invention in mechanical or chemical art, since its bounds are unlimited, and equally so must be the researches of its professors."

"The art of directing the great sources of power in Nature for the use and convenience of man." Among all secular pursuits, can there be imagined one more vast in its scope, more beneficent, and therefore more honourable, than this? There are those, I know—hundreds, thousands—who say that such pursuits are not to be named as on a par with those of literature; that there is nothing ennobling in them; nothing elevating; that they are of the earth earthy; are mechanical, and are unintellectual, and that even the mere bookworm, who, content with storing his own mind, neither distributes those stores to others nor himself originates, is more worthily occupied than is the civil engineer.

I deny this altogether, and, while acknowledging, with gratitude, that, in literature, the masterpieces of master minds have afforded, and will afford, instruction, delight, and solace for all generations so long as civilization endures, I say that the pursuits of civil engineering are worthy of occupying the highest intelligence, and that they are elevating and ennobling in their character.

Remember the kindly words of Sir Thomas Browne, who said, when condemning the uncharitable conduct of the mere bookworm, "I make not, therefore, my head a grave, but a treasure of knowledge, and study not for mine own sake only, but for those who study not for themselves." The engineer of the present day finds that he must not make his "head a grave," but that, if he wishes to succeed, he must have, and must exercise, scientific knowledge; and he realizes daily the truth that those who are to come after him must be trained in science, so that they may readily appreciate the full value of each scientific discovery as it is made. Thus the application of science by the engineer not only stimulates those who pursue science, but adds him to their number.

Holding, as I have said I do, the view that he who displaces unintelligent labour is doing good to mankind, I claim for the unknown engineer who, in Pontus, established the first water-wheel of which we have a record, and for the equally unknown engineer who first made use of wind for a motor, the title of pioneers in the raising of the dignity of labour, by compelling the change from the non-intelligent to the intelligent.

With respect to these motors—wind and water—we have two proverbs which discredit them: "Fickle as the wind," "Unstable as water."

Something more trustworthy was needed—something that we were sure of having under our hands at all times. As a result, science was applied, and the "fire" engine, as it was first called, the "steam" engine, as it was re-named, a form of "heat" engine, as we now know it to be, was invented.

Think of the early days of the steam-engine—the pre-Watt days. The days of Papin, Savory, Newcomen, Smeaton! Great effects were produced, no doubt, as compared with no fire engine at all; effects so very marked as to extort from the French writer, Belidor, the tribute of admiration he paid to the "fire" engine erected at the Fresnes Colliery by English engineers. A similar engine worked the pumps in York Place (now the Adelphi) for the supply of water to portions of London. We have in his work one of the very clearest accounts, illustrated by the best engravings (absolute working drawings), of the engine which had excited his admiration. These drawings show the open-topped cylinder, with condensation taking place below the piston, but with the valves worked automatically.

It need hardly be said that, noteworthy as such a machine was, as compared with animal power, or with wind or water motors, it was of necessity a most wasteful instrument as regards fuel. It is difficult to conceive in these days how, for years, it could have been endured that at each stroke of the engine the chamber that was to receive the steam at the next stroke was carefully cooled down beforehand by a water injection.

Watt, as we know, was the first to perceive, or, at all events, to cure, this fundamental error which existed prior to his time in the "fire" engine. To him we owe condensation in a separate vessel, the doing away with the open-topped cylinder, and the making the engine double-acting; the parallel motion; the governor; and the engine-indicator, by which we have depicted for us the way in which the work is being performed within the cylinder. To Watt, also, we owe that great source of economic working—the knowledge of the expansive force of steam; and to his prescience we owe the steam-jacket, without which expansion, beyond certain limits, is practically worthless. I have said "prescience"—fore-knowledge—but I feel inclined to say that, in this case, prescience may be rendered "pre-Science," for I think that Watt *felt* the utility of the steam-jacket, without being able to say on what ground that utility was based.

I have already spoken in laudatory terms of Tredgold, as being one of the earliest of our scientific engineering writers, but, as regards the question of steam-jacketing, Watt's prescience was better than Tredgold's science, for the latter condemns the steam-jacket, as being a means whereby the cooling surfaces are enlarged, and whereby, therefore, the condensation is increased.

I think it is not too much to say that engineers who, since Watt's days, have produced machines of such marvellous power—and, compared with the engines of Watt's days, of so great economy—have, so far as principles are concerned, gone upon those laid down by Watt. Details of the most necessary character—necessary to enable those principles to be carried out—have, indeed, been devised since the days of Watt. Although it is still a very sad confession to have to make, that the very best of our steam-engines only utilizes about one-sixth of the work which resides (if the term may be used) in the fuel that is consumed, it is, nevertheless, a satisfaction to know that great economical progress has been made, and that the 6 or 7 pounds of fuel per horse-power per hour consumed by the very best engines of Watt's days, when working with the aid of condensation, is now brought down to about one-fourth of this consumption; and this in portable engines, for agricultural purposes, working without condensation—engines of small size, developing only 20 horse-power; in such engines the consumption has been reduced to as little as 1·85 pound per brake horse-power per hour, equal to 1·65 pound per indicated horse-power per hour, as was shown by the trials at the Royal Agricultural Society's meeting at Newcastle last year—trials in which I had the pleasure of participating.

In these trials Mr. William Anderson, one of the Vice-Presidents of Section G, and I were associated, and, in making our report of the results, we adopted the balance-sheet system, which I suggested and used so long ago as 1873 (see vol. lii., pp. 154 and 155, of the "Minutes of Proceedings of the Institution of Civil Engineers"), and to which I alluded in my address as President of Section G at Montreal.

I have told you that the engineer of the present day appreciates the value of the "next-to-nothings." There is an old house-keeping proverb that, if you take care of the farthings and the pence, the shillings and the pounds will take care of themselves. Without the balance-sheet one knows that for the combustion of 1 pound of coal, the turning into steam of a given quantity of water at a given pressure is obtained. It is seen, at once, that the result is much below that which should be had, but to account for the deficiency is the difficulty. The balance-sheet, dealing with the most minute sources of loss—the farthings and the pence of economic working—brings you face to face with these, and you find that improvement must be sought in paying attention to the "next-to-nothings."

Just one illustration. The balance-sheet will enable you at a glance to answer this among many important questions: Has the fuel been properly burnt—with neither too much air, nor too little?

At the Newcastle trials our knowledge as to whether we had the right amount of air for perfect combustion was got by an analysis of the waste gases, taken continuously throughout the whole number of hours' run of each engine, affording, therefore, a fair average. The analysis of any required portion of gases thus obtained was made in a quarter of an hour's time by the aid of the admirable apparatus invented by Mr. Stead, and, on the occasion to which I refer, manipulated by him. In one instance an excess of air had been supplied, causing a percentage of loss of 6·34 In the instance of another engine there was a deficiency of air, resulting in the production of carbonic oxide, involving a loss of 4 per cent. The various percentages of loss, of which each one seems somewhat unimportant, in the aggregate amounted to 28 per cent., and this with one of the best boilers. This is an admirable instance of the need of attention to apparently small things.

I have already said that we now know the steam-engine is really a heat engine. At the York meeting of our Association I ventured to predict that, unless some substantive improvement were made in the steam-engine (of which improvement, as yet, we have no notion), I believed its days, for small powers, were numbered, and that those who attended the centenary of the British Association in 1931 would see the present steam-engines in museums, treated as things to be respected, and of antiquarian interest to the engineers of those days, such as are the open-topped steam cylinders of Newcomen and of Smeaton to ourselves. I must say I see no reason, after the seven years which have elapsed since the York meeting, to regret having made that prophecy, or to desire to withdraw it.

The working of heat engines, without the intervention of the vapour of water, by the combustion of the gases arising from coal, or from coal and from water, is now not merely an established fact, but a recognized and undoubted, commercially economical, means of obtaining motive power. Such engines, developing from 1 to 40 horse-power, and worked by the ordinary gas supplied by the gas mains, are in most extensive use in printing-works, hotels, clubs, theatres, and even in large private houses, for the working of dynamos to supply electric light. Such engines are also in use in factories, being sometimes driven by the gas obtained from "culm" and steam, and are giving forth a horse-power for, it is stated, as small a consumption as 1 pound of fuel per hour.

It is hardly necessary to remind you—but let me do it—that, although the saving of half a pound of fuel per horse-power appears to be insignificant, when stated in that bald way, one realizes that it is of the highest importance when that half-pound turns out to be 33 per cent. of the whole previous consumption of one of those economical engines to which I have referred.

The gas-engine is no new thing. As long ago as 1807 a M. de Rivaz proposed its use for driving a carriage on ordinary roads. For anything I know, he may not have been the first proposer. It need hardly be said that in those days he had not illuminating gas to resort to, and he proposed to employ hydrogen. A few years later a writer in *Nicholson's Journal*, in an article on "Flying Machines," having given the correct statement that all that is needed to make a successful machine of this description is to find a sufficiently light motor, suggests that the direction in which this may be sought is the employment of illuminating gas, to operate by its explosion on the piston of an engine. The idea of the gas-engine was revived, and formed the subject of a patent by Barnett in the year 1838. It is true this gentleman did not know very much about the subject, and that he suggested many things which, if carried out, would have resulted in the production of an engine which could not have worked; but he had an alternative proposition which would have worked.

Again, in the year 1861, the matter was revived by Lenoir, and in the year 1865 by Hugon, both French inventors. Their engines obtained some considerable amount of success and notoriety, and many of them were made and used; but in the majority of cases they were discarded as wasteful and uncertain. The Institution of Civil Engineers, for example, erected a Lenoir in the year 1868, to work the ventilating fan, but after a short time they were compelled to abandon it and to substitute an hydraulic engine.

At the present time, as I have said, gas-engines are a great commercial success, and they have become so by the attention given to small things, in popular estimation—to important things, in fact, with which, however, I must not trouble you. Messrs. Crossley Brothers, who have done so much to make the gas-engine the commercial success that it is, inform me that they are prosecuting improvements in the direction of attention to detail, from which they are obtaining greatly improved results.

But, looking at the wonderful petroleum industry, and at the multifarious products which are obtained from the crude material, is it too much to say that there is a future for motor engines, worked by the vapour of some of the more highly volatile of these products—true vapour—not a gas, but a condensable body, capable of being worked over and over again? Numbers of such engines, some of as much as 4 horse-power, made by Mr. Yarrow, are now running, and are apparently giving good results; certainly excellent results as regards the compactness and lightness of the machinery. For boat purposes they possess the great advantage of being rapidly under way. I have seen one go to work within two minutes of the striking of the match to light the burner.

Again, as we know, the vapour of this material has been used as a gas in gas-engines, the motive power having been obtained by direct combustion.

Having regard to these considerations, was I wrong in predicting that the heat engine of the future will probably be one independent of the vapour of water? And, further, in these days of electrical advancement, is it too much to hope for the direct production of electricity from the combustion of fuel?

As the world has become familiar with prime movers, the desire for their employment has increased. Many a householder could find useful occupation for a prime mover of ¼ or ½ horse-power, working one or two hours a day; but the economical establishment of a steam-engine is not possible until houses of very large dimensions are reached, where space exists for the engine, and where, having regard to the amount of work to be done, the incidental expenses can be borne. Where this cannot be, either the prime mover, with the advantages of its use, must be given up as a thing to be wished for, but not to be procured, or recourse must be had to some other contrivance—say to the laying on of power, in some form or another, from a central source.

I have already incidentally touched upon one mode of doing this—namely, the employment of illuminating gas, as the working agent in the gas-engine; but there are various other modes, possessing their respective merits and demerits—all ingenious, all involving science in their application, and all more or less in practical use—such as the laying on of special high-pressure water, as is now being extensively practised in London, in Hull, and elsewhere. Water at 700 pounds pressure per inch is a most convenient mode of laying on a large amount of power, through comparatively small pipes. Like electricity, where, when a high electromotive force is used, a large amount of energy may be sent through a small conductor, so with water, under high pressure, the mains may be kept of reasonable diameters, without rendering them too small to transmit the power required through them

Power is also transmitted by means of compressed air, an agent which, on the score of its ability to ventilate, and of its cleanliness, has much to recommend it. On the other hand, it is an agent which, having regard to the probability of the deposition of moisture in the form of "snow," requires to be worked with judgment.

Again, there is an alternative mode for the conveyance of power by the exhaustion of air—a mode which has been in practical use for over sixty years.

We have also the curious system pursued at Schaffhausen, where quick-running ropes are driven by turbines, these being worked by the current of the River Rhine; and at New York, and in other cities of the United States, steam is laid on under the streets, so as to enable domestic steam-engines to be worked, without the necessity of a boiler, a stoker, or a chimney, the steam affording also means of heating the house when needed.

Lastly, there is the system of transmitting power by electricity, to which I have already adverted. I was glad to learn, only the other day, that there was every hope of this power being applied to the working of an important subterranean tramway.

These distributions from central sources need, as a rule, statutory powers to enable the pipes or wires to be placed under the roads; and, following the deplorable example of the Electrical Facilities Act, it is now the habit of the enlightened Corporation and the enterprising town clerk of most boroughs to say to capitalists who are willing to embark their capital in the plant for the distribution of power from a central source—for their own profit no doubt, but also, no doubt, for the good of the community—"We will oppose you in Parliament, unless you will consent that, at the end of twenty-one years, we may acquire compulsorily your property, and may do so, if it turns out to be remunerative, without other payment than that for the mere buildings and plant at that time existing." This is the way English enterprise is met, and then English engineers are taunted, by Englishmen—often by the very men who have had a share in making this "boa-constrictor" of a "Facilities Act"—that their energy is not to be compared with that which is to be found in the United States and other countries. Again, however, I must remember that I am not addressing Section F.

There is one application of science, by engineers, which is of extreme beauty and interest, and that cannot be regarded with indifference by the agriculturists of this country. I allude to the heat-withdrawing engines (I should like to say, "cold-producers," but I presume, if I did, I should be criticized), which are now so very extensively used for the importation of fresh meat, and for its storage when received here. It need hardly be said, that that which will keep cool and sweet the carcasses of sheep will equally well preserve milk, and many other perishable articles of food. We have in these machines daily instances that, if you wish to make a ship's hold cold, you can do it by burning a certain quantity of coals—a paradox, if ever there was one.

In this climate of ours, where the summer has been said to consist of "three hot days and a thunderstorm," there is hardly need to make a provision for cooling our houses, although there is an undoubted need for making a provision to heat them. Nevertheless, those of us who have hot-water heating arrangements for use in the winter would be very glad indeed if, without much trouble or expense, they could turn these about, so as to utilize them for cooling their houses in summer. Mr. Loftus Perkins, so well known for his labours in the use of very high-pressure steam (600 to 1000 pounds on the inch), and also so well known for those most useful high-pressure warming arrangements which, without disfiguring our houses by the passage of large pipes, keep them in a state of warmth and comfort throughout the winter, has lately taken up the mode of, I will say it, producing "cold" by the evaporation of ammonia, and, by improvements in detail, has succeeded in making an apparatus which, without engine or pumps, produces "cold" for some hours in succession, and requires, to put it in action, the preliminary combustion of only a few pounds of coke or a few feet of gas.

As I have said, our climate gives us but little need to provide or employ apparatus to cool our houses, but one can well imagine that the Anglo-Indian will be glad to give up his punkah for some more certain, and less draughty, mode of cooling.

I now desire to point out how, as the work of the engineer grows, his needs increase. New material, or better material of the old kind, has to be found to enable him to carry out these works of greater magnitude. At the beginning of this century, stone, brick, and timber were practically the only materials employed for that which I may call standing engineering work —*i.e.* buildings, bridges, aqueducts, and so on—while timber, cast iron, and wrought iron were for many years the only available materials for the framing and principal parts of moving machines and engines, with the occasional use of lead for the pipes and of copper for pipes and for boilers.

As regards the cast iron, little was known of the science involved (or that ought to be involved) in its manufacture. It was judged of by results. It was judged of largely by the eye. It was "white," it was "mottled," it was "gray." It was known to be "fit for refining," fit for "strong castings," or fit for castings in which great fluidity in the molten metal was judged to be of more importance than strength in the finished casting. With respect to wrought iron, it was judged of by its results also. It was judged of by the place of its manufacture—but when the works of the district were unknown, the iron, on being tested, was classed as "good fibrous," although some of the very best was "steel-like," or "bad," "hot-short," or "cold-short." A particular district would produce one kind of iron, another district another kind of iron. The ore, the flux, and the fuel were all known to have influence, but to what extent was but little realized; and if there came in a new ore, or a new flux, it might well be that for months the turn-out of the works into which these novelties had been introduced would be prejudiced. Steel again—that luxury of the days of my youth—was judged by the eye. The wrought bars, made into "blister" steel by "cementation," were broken, examined, and grouped accordingly. Steel was known, no doubt, to be a compound of iron and carbon, but the importance of exactness in the percentage was but little understood, nor was it at all understood how the presence of comparatively small quantities of foreign matter might necessitate the variation of the proportions of carbon. The consequence was that anomalous results every now and then arose to confound the person who had used the steel, and, falsifying the proverb "true as steel," steel became an object of distrust. Is it too much to say that Bessemer's great invention of steel made by the "converter," and that Siemens's invention of the open-hearth process, reacted on pure science, and set scientific men to investigate the laws which regulate the union of metals and of metalloids? and that the labours of these scientific men have improved the manufacture, so that steel is now thoroughly and entirely trusted? By its aid engineering works are accomplished which, without that aid, would have been simply impossible. The Forth Bridge, the big gun, the compound armour of the ironclad with its steel face, the projectile to pierce that steel face—all equally depend upon the "truth" of steel as much as does the barely visible hair-spring of the chronometer which enables the longitude of the ship in which it is carried to be ascertained. Now, what makes the difference between trustworthy and untrustworthy steel for each particular purpose? Something which, until our better sense comes to our aid, we are inclined to look upon as ridiculously insignificant—a "next-to-nothing." Setting extraneous ingredients aside, and considering only the union of iron and carbon, the question whether there shall be added or deducted one-tenth of 1 per cent. (pardon my clumsy way of using the decimal system) of carbon is a matter of great importance in the resulting quality of the steel. This is a striking practical instance of how apparently insignificant things may be of the highest importance. The variation of this fraction of a percentage may render your boiler steel untrustworthy, may make the difference between safety in a gun and danger in a gun, and may render your armour-piercing projectile unable to pierce even the thinnest wrought-iron armour.

While thus brought incidentally to the subject of guns, let me derive from it another instance of the value of small things. I have in my hand a piece of steel ribbon. It is probable that only those who are near to me can see it. Its dimensions are one-fourth by one-sixteenth of an English inch, equal to an area of one sixty-fourth of a square inch. This mode of stating the dimensions I use for the information of the ladies. To make it intelligible to my scientific friends, I must tell them that it is approximately ·00637 of a metre by approximately ·00159 of a metre, and that its sectional area is ·0000101283 (also approximately) of a square metre. This insignificant (and speaking in reference to the greater number of my audience), practically invisible piece of material—that I can bend with my hand, and even tie into knots—is, nevertheless, not to be despised. By it one reinforces the massive and important-looking A-tube of a 9·2-inch gun, so that from that tube can be projected with safety a projectile weighing 380 pounds at a velocity, when leaving the muzzle, of between one-third and one-half of a mile in a second, and competent to traverse nearly 12½ miles before it touches the ground. It may be said, "What is the use of being able to fire a projectile to a distance which commonly is invisible (from some obstacle or another) to the person directing the gun?" I will suggest to you a use. Imagine a gun of this kind placed by some enemy who, unfortunately, had invaded us, and had reached Richmond. He has the range-table for his gun; he, of course, is provided with our Ordnance maps, and he lays and elevates the gun at Richmond, with the object of striking, say, the Royal Exchange. Suppose he does not succeed in his exact aim. The projectile goes 100 yards to one side or to the other; or it falls 250 yards short, or passes 250 yards over; and it would be "bad shooting" indeed, in these days, if nearly every projectile which was fired did not fall somewhere within an area such as this. In this suggested parallelogram of 100,000 square yards, or some 20 acres, there is some rather valuable property; and the transactions which are carried on are not unimportant. It seems to me that business would not be conducted with that calmness and coolness which are necessary for success, if, say, every five minutes, a 380-pound shell fell within this area, vomiting fire, and scattering its walls in in hundreds of pieces, with terrific violence, in all directions. Do not suppose I am saying that similar effects cannot be obtained from a gun where wire is not employed. They can be. But my point is, that they can also be obtained by the aid of the insignificant thing which I am holding up at this moment—this piece of steel ribbon, which looks more suitable for the framework of an umbrella.

I have already spoken to you, when considering steel as a mere alloy of iron and carbon, as to the value of even a fraction of 1 per cent. of the latter; but we know that in actual practice steel almost always contains other ingredients. One of the most prominent of these is manganese. It had for years been used, in quantities varying from a fraction of 1 per cent. up to 2·5 per cent., with advantages as regards ductility, and as regards its ability to withstand forging. A further increase was found not to augment the advantage: a still further increase was found to diminish it; and here the manufacturer stopped, and, so far as I know, the pure scientist stopped, on the very reasonable ground that the point of increased benefit appeared to have been well ascertained, and that there could be no advantage in pursuing an investigation which appeared only to result in decadence. But this is another instance of how the application of science reacts in the interests of pure science itself. One of our steel manufacturers, Mr. Hadfield, determined to pursue this apparently barren subject, and in doing so discovered this fact—that, while with the addition of manganese in excess of the limit before stated, and up to as much as 7 per cent., deterioration continued, after this latter percentage was passed improvement again set in.

Again, the effects of the addition of even the very smallest percentages of aluminium upon the steel with which it may be alloyed are very striking and very peculiar, giving to the steel alloy thus produced a very much greater hardness, and enabling it to take a much brighter and more silver-like polish. Further, the one-twentieth part of 1 per cent. of aluminium, when added to molten wrought iron, will reduce the fusing-point of the whole mass some 500°, and will render it extremely fluid, and thus enable wrought iron (or what are commercially known as "mitis" castings of the most intricate character) to be produced.

No one has worked more assiduously at the question of the effect of the presence of minute quantities, even traces, of alloys with metals than Prof. Roberts-Austen, and he appears, by his experiments, to be discovering a general law, governing the effect produced by the mixture of particular metals, so that, in future, it is to be hoped, when an alloy is, for the first time, to be attempted, it will be possible to predict with reasonable certainty what the result will be, instead of that result remaining to be discovered by experiment.

I have just, incidentally, mentioned aluminium. May I say that we engineers look forward, with much interest, to all processes tending to bring this metal, or its alloys, within possible commercial use?

One more instance of the effect of impurities in metals. The engineer engaged in electrical matters is compelled, in the course of his daily work, frequently to realize the importance of the "next to-nothing." One striking instance of this is afforded by the influence which an extremely minute percentage of impurity has on the electrical conductivity of copper wire; this conductivity being in some cases reduced by as much as 50 per cent., in consequence of the admixture of that which, under other circumstances, would be looked upon as insignificant.

Reverting to the question of big guns. According to the present mode of manufacture, after we have rough-bored and turned the A-tube (and perhaps I ought to have mentioned that by the A-tube is meant the main piece of the gun, the innermost layer, if I may so call it, that portion which is the full length of the gun, and upon which the remainder of the gun is built up)—after, as I have said, we have rough-bored and turned this A-tube, we heat it to a temperature lying between certain specified limits, but actually determined by the behaviour of samples previously taken, and then suddenly immerse it perpendicularly into a well some 60 feet deep, full of oil, the oil in this well being kept in a state of change by the running into it, at the bottom, of cold oil conveyed by a pipe proceeding from an elevated oil tank. In this way the steel is oil-hardened, with the result of increasing its ultimate tensile strength, and also with the result of raising its so-called elastic limit. In performing this operation it is almost certain that injurious internal strains will be set up—strains tending to produce self-rupture of the material. Experiments have been carried out in England, by Captain Andrew Noble, and by General Maitland of the Royal Gun Factory, by General Kalakoutsky, in Russia, and also in the United States, to gauge what is the value, as represented by dimensions, of these strains, and we find that they have to be recorded in the most minute fractions of an inch, and yet, if the steel be of too "high" a quality (as it is technically called), or if there has been any want of uniformity in the oil-hardening process, these strains, unless got rid of or ameliorated by annealing, may, as I have said, result in the self-rupture of the steel.

I have spoken of the getting rid of these strains by annealing, a process requiring to be conducted with great care, so as not to prejudice the effects of the oil-hardening. But take the case of a hardened steel projectile, hardened so that it will penetrate the steel face of compound armour. In that case annealing cannot be resorted to, for the extreme hardness of the projectile must not be in the least impaired. The internal strains in these projectiles are so very grave that for months after they are made there is no security that they will not spontaneously fracture. I have here the point of an 8-inch projectile, which projectile weighs 210 pounds; this with others was received from the makers as long ago as March of this year, and remained an apparently perfect and sound projectile until about the middle of August—some five months after delivery, and, of course, a somewhat longer time since manufacture—and between August 6 and 8 this piece which I hold in my hand, measuring $3\frac{3}{4}$ inches by $3\frac{1}{2}$ inches, spontaneously flew off from the rest of the projectile, and has done so upon a surface of separation which, whether having regard to its beautiful regularity, or to the conclusions to be drawn from it as to the nature of the strains existing, is of the very highest scientific interest. Many other cases of self-rupture of similar projectiles have been recorded.

Another instance of the effect of the "next-to-nothing" in the hardening and tempering or annealing of steel. As we know, the iron and the carbon (leaving other matters out of consideration) are there. The carbon is (even in tool-steel) a very small proportion of the whole. The steel may be bent, and will retain the form given to it. You heat it and plunge it in cold water; you attempt to bend it and it breaks; but if, after the plunging in cold water, you temper it by carefully reheating it, you may bring it to the condition fit either for the cutting-tool for metal, or for the cutting-tool for wood, or for the watch-spring; and these important variations of condition which are thus obtained depend upon the "next-to-nothing" in the temperature to which it is reheated, and therefore in the nature of the resulting combination of the ingredients of which the steel is composed.

Some admirable experiments were carried out on this subject by the Institution of Mechanical Engineers, with the assistance of one of our Vice-Presidents, Sir Frederick Abel, and the subject has also been dealt with by an eminent Russian writer.

There is, to my mind, another and very striking popular instance (if I may use the phrase) of the importance of attention to detail—that is, to the "next-to-nothing." Consider the bicycles and tricycles of the present day—machines which afford the means of healthful exercise to thousands, and which will, probably within a very short time, prove of the very greatest possible use for military purposes. The perfection to which these machines have been brought is almost entirely due to strict attention to detail; in the selection of the material of which the machines are made; in the application of pure science (in its strictest sense) to the form and to the proportioning of the parts, and also in the arrangement of these various parts in relation the one to the other. The result is that the greatest possible strength is afforded with only the least possible weight, and that friction in working has been reduced to a minimum. All of us who remember the hobby-horse of former years, and who contrast that machine with the bicycle or tricycle of the present day, realize how thoroughly satisfactory is the result of this attention to detail—this appreciation of the "next-to-nothing."

Let me give you another illustration of the importance of small things, drawn from gunnery practice.

At first sight one would be tempted to say that the density of the air on the under side of a shot must, notwithstanding its motion of descent, be so nearly the same as that of the air upon the upper side as to cause the difference to be unworthy of consideration, but we know that the projectiles from rifled guns tend to travel sideways as they pass through the air, and that the direction of their motion, whether to the right or to the left, depends on the 'hand' of the rifling. We know also, that the friction against liquid or against gaseous bodies varies with the densities of these bodies, and it is believed that, minute as is the difference in density to which I have referred, it is sufficient to determine the lateral movement of the projectile. This lateral tendency must be allowed for, in these days of long ranges, in the sighting and laying of guns, if we desire accuracy of aim, at those distances at which it is to be expected our naval engagements will have to be commenced, and perhaps concluded. We can no longer afford to treat the subject as Nelson is said to have treated it, in one of his letters to the Secretary of the Admiralty, who had requested that an invention for laying guns more accurately should be tried. Nelson said he would be glad to try the invention, but that, as his mode of fighting consisted in placing his ship close alongside that of the enemy, he did not think the invention, even if it were successful, would be of much use to him.

While upon the question of guns, I am tempted to remark upon that which is by no means a small thing (for it is no less than the rotation of the earth), which in long-distance firing may demand attention, and that to an extent little suspected by the civilian.

Place the gun north and south, say in the latitude of London, and fire a 12-mile round such as I have mentioned, and it will be found that, assuming the shot were passing through a vacuum, a lateral allowance of more than 200 feet must be made to compensate for the different velocity of the circumference of the earth at 12 miles north or south of the place where the gun was fired, as compared with the velocity of the circumference of the earth at that place itself—the time of flight being in round numbers one minute.

At the risk of exciting a smile, I am about to assert that engineering has even its poetical side. I will ask you to consider with me whether there may not be true poetry in the feelings of the engineer who solves a problem such as this:—Consider this rock, never visible above the surface of the tide, but making its presence known by the waves which rise around it: it has been the cause of destruction to many a noble vessel which had completed, in safety, its thousands of leagues of journey, and was, within a few score miles of port, then dashed to pieces upon it! Here is this rock. On it built a lighthouse. Lay your foundations through the water, in the midst of the turmoil of the sea; make your preparations; appear to be attaining success, and find the elements are against you, and that the whole of your preliminary works are ruined or destroyed in one night; but again commence, and then go on and go on until at last you conquer; your works rise above ordinary tide-level; then upon these sure foundations, obtained it may be after years of toil, erect a fair shaft, graceful as a palm and sturdy as an oak; surmount it with a light, itself the produce of the highest application of science; direct that light by the built-up lens, again involving the highest application of science; apply mechanism, so arranged that the lighthouse shall from minute to minute reveal to the anxious mariner its exact name and its position on the coast. When you have done all this, will you not be entitled to say to yourself, "It is I who have for ever rendered innocuous this rock which has been hitherto a dread source of peril"? Is there no feeling, do you think, of a poetical nature excited in the breast of the engineer who has successfully grappled with a problem such as this?

Another instance: the mouth of a broad river, or, more properly speaking, the inlet of the sea, has to be crossed at such a level as not to impede the passage of the largest ships. Except in one or two places the depth is profound, so that multiple foundations for supporting a bridge become commercially impossible, and the solution of the problem must be found by making, high in the air, a flight of span previously deemed unattainable. Is there no poetry here? Again, although the results do not strike the eye in the same manner, is there nothing of poetry in the work that has to be thought out and achieved when a wide river or an ocean channel has to be crossed by a subterranean passage? Works of great magnitude of this character have been performed with success, and to the benefit of those for whose use they were intended. One of the greatest and most noble of such works, encouraged, in years gone by, by the Governments of our own country and of France, has lately fallen into disfavour with an unreasoning public, who have not taken the pains to ascertain the true state of the case.

Surely it will be agreed that the promotion of ready intercourse and communication between nations constitutes the very best and most satisfactory guarantee for the preservation of peace: when the peoples of two countries come to know each other intimately, and when they, therefore, enter into closer business relations, they are less liable to be led away by panic or by anger, and they hesitate to go to war the one with the other. It is in the interests of both that questions of difference which may arise between them should be amicably settled, and having an intimate knowledge of each other, they are less liable to misunderstand, and the mode of determination of their differences is more readily arranged. Remember, the means of ready intercourse and of communication, and the means of easy travel, are all due to the application of science by the engineer. Is not therefore his profession a beneficent one?

Further, do you not think poetical feeling will be excited in the breast of that engineer who will in the near future solve the problem (and it certainly will be solved when a sufficiently light motor is obtained) of travelling in the air—whether this solution be effected by enabling the self-suspended balloon to be propelled and directed, or perhaps, better still, by enabling not only the propulsion to be effected and the direction to be controlled, but by enabling the suspension in the air itself to be attained by mechanical means?

Take other functions of the civil engineer—functions which, after all, are of the most important character, for they contribute directly to the prevention of disease, and thereby not only prolong life, but do that which is probably more important—afford to the population a healthier life while lived.

In one town, about which I have full means of knowing, the report has just been made that in the year following the completion of a comprehensive system of sewerage, the deaths from zymotic diseases had fallen from a total of 740 per annum to a total of 372—practically one-half. Has the engineer no inward satisfaction who knows such results as these have accrued from his work?

Again, consider the magnitude and completeness of the water-supply of a large town, especially a town that has to depend upon the storing up of rain water: the prevision which takes into account, not merely the variation of the different seasons of the year, but the variation of one year from another; that, having collated all the stored-up information, determines what must be the magnitude of the reservoirs to allow for at least three consecutive dry years, such as may happen; and that finds the sites where these huge reservoirs may be safely built.

All these—and many other illustrations which I could put before you if time allowed—appear to me to afford conclusive evidence that, whether it be in the erection of the lighthouse on the lonely rock at sea; whether it be in the crossing of rivers, or seas, or arms of seas, by bridges or by tunnels; whether it be the cleansing of our towns from that which is foul; whether it be the supply of pure water to every dwelling, or the distribution of light or of motive power; or whether it be in the production of the mighty ocean steamer, or in the spanning of valleys, the piercing of mountains, and affording the firm, secure road for the express train; or whether it be the encircling of the world with telegraphs—the work of the civil engineer is not of the earth earthy, is not mechanical to the exclusion of science, is not unintellectual; but is of a most beneficent nature, is consistent with true poetical feeling, and is worthy of the highest order of intellect.

SECTION A.

MATHEMATICAL AND PHYSICAL SCIENCE.

Opening Address by Prof. G. F. Fitzgerald, M.A., F.R.S., President of the Section.

The British Association in Bath, and especially we here in Section A, have to deplore a very great loss. We confidently anticipated profit and pleasure from the presence in this chair of one of the leading spirits of English science, Dr. Schuster. We deplore the loss, and we deplore the cause of it. It is always sad when want of strength makes the independent dependent, and it is doubly sad when a life's work is thereby delayed; and to selfish humanity it is trebly sad when, as in this case, we ourselves are involved in the loss. And our loss is great. Dr. Schuster has been investigating some very important questions. He has been studying electric discharges in gases, and he has been investigating the probably allied question of the variations of terrestrial magnetism. We anticipated his matured pronouncements upon these subjects, and also the advantage of his very wide general information upon physical questions, and the benefit of his judicial mind while presiding here.

As to myself, his substitute, I cannot express how much gratified I feel at the distinguished honour done me in asking me to preside. It has been one of the ambitions of my life to be worthy of it, and I will do my best to deserve your confidence; man can do no more, and upon such a subject "the less said the soonest mended."

I suppose most former occupants of this chair have looked over the addresses of their predecessors to see what sort of a thing was expected from them. I find that very few had the courage to deliver no address. Most have devoted themselves to broad general questions, such as the relations of mathematics to physics, or more generally deductive to inductive science. On the other hand, several have dealt each with his own specialty. On looking back over these addresses my attention was specially arrested by the first two past Presidents of this Section whose bodily presence we cannot have here. They were Presidents of Section A in consecutive years. In 1874, Provost Jellett occupied this chair; and in 1875, Prof. Balfour Stewart occupied it. Both have gone from us since the last meeting of this Association. Each gave a characteristic address. The Provost, with the clearness and brilliancy that distinguished his great intellect, plunged through the deep and broad questions surrounding the mechanism of the universe, and with impassioned earnestness claimed on behalf of science the right to prosecute its investigations until it attains, if it ever does attain, to a mechanical explanation of all things. This intrepid honesty, to carry to their utmost the principles of whose truth he was convinced, the utter abhorrence of the shadow of double-dealing with truth, was eminently characteristic of one whom all, but especially we of Trinity College, Dublin, will long miss as a lofty example of the highest intellectual keenness and honesty, and mourn as the truest-hearted friend, full of sympathy and Christian charity. In 1875, Prof. Stewart gave us a striking example of the other class of address in a splendid exposition of the subject he did so much to advance—namely, solar physics. He brought together from the two great storehouses of his information and speculation a brilliant store, and displayed them here for the advancement of science. Him, too, all science mourns. Though, from want of personal acquaintance, I am unequal to the task of bringing before you his many abilities and great character, you can each compose a fitting epitaph for this well-known great one of British science. In this connection I am only expressing what we all feel when I say how well timed was the Royal bounty recently extended to his widow. At the same time, the niggardly recognition of science by the public is a disgrace to the enlightenment of the nineteenth century. What Chancellor or General with his tens of thousands has done that for his country and mankind that Faraday, Darwin, and Pasteur have done? The "public" now are but the children of those who murdered Socrates, tolerated the persecution of Galileo, and deserted Columbus.

In a Presidential address on the borderlands of the known delivered from this chair the great Clerk Maxwell spoke of it as an undecided question whether electro-magnetic phenomena are due to a direct action at a distance or are due to the action of an intevening medium. The year 1888 will be ever memorable as the year in which this great question has been experimentally decided by Hertz in Germany, and, I hope, by others in England.

It has been decided in favour of the hypothesis that these actions take place by means of an intervening medium. Although there is nothing new about the question, and although most workers at it have long been practically satisfied that electro-magnetic actions are due to an intervening medium, I have thought it worth while to try and explain to others who may not have considered the problem, what the problem is and how it has been solved. A Presidential address such as this is not for specialists—it is for the whole Section; and I would not have thought of dealing with this subject, only that its immediate consequences reach to all the bounds of physical science, and are of interest to all its students.

We are all familiar with this, that when we do not know all about something there are generally a variety of explanations of what we do know. Whether there is anything of which there are in reality a variety of explanations is a deep question, which some have connected with the freedom of the will, but which I am not concerned with here. A notable example of the possibility of a variety of explanations for us is recorded in connection with an incident said to have occurred in the neighbouring town of Clifton, where a remarkable meteorological phenomenon, as it appeared to an observing scientist, was explained by others as a bull's-eye lantern in the hands of Mr. Pickwick. Another kind of example is the old explanation of water rising in a pump, that "Nature abhors a vacuum," as compared with the modern one. Nowadays, when we know as little about anything, we say, "It is the property of electricity to attract." This is really little or no advance on the old form, and is merely a way of stating that we know a fact but not its explanation. There are plenty of cases still where a variety of explanations are possible. For example, we know of no *experimentum crucis* to decide whether the people I see around me are conscious or are only automata. There are other questions which have existed, but which have been experimentally decided. The most celebrated of these are the questions between the caloric and kinetic theories of heat, and between the emission and undulatory theories of light. The classical experiments by which the case has been decided in favour of the kinetic theory of heat and the undulatory theory of light are some of the most important experiments that have ever been performed. When it was shown that heat disappeared whenever work appeared, and *vice versâ*, and so the caloric hypothesis was disproved; when it was shown that light was propagated more slowly in a dense medium than in a rare, the sciences of light and heat were revolutionized. Not but that most who studied the subject had given their adhesion to the true theory before it was finally decided in general estimation. In fact, Rumford's and Davy's experiments on heat, and Young and Fresnel's experiments on light, had really decided these questions long before the erroneous views were finally abandoned. I hope that science will not be so slow in accepting the results of experiment in respect of electro-magnetism as it was in the case of light and heat, and that no Carnot will throw back science by giving plausible explanations on a wrong hypothesis. Rowland's experiment proving an electro-magnetic action between electric charges depending on their absolute and not relative velocities has already proved the existence of a medium relative to which the motion must take place, but the connection is rather metaphysical, and is too indirect to attract general attention. The importance of these striking experiments was that they put the language of the wrong hypothesis out of fashion. Elementary text-books that halted between two opinions, and, after the manner of text-books, leant towards that enunciated in preceding text-books, had all perforce to give prominence to the true theory, and the whole rising generation began their researches from a firm and true stand-point. I anticipate the same results to follow Hertz's experimental demonstration of a medium by which electro-magnetic actions are produced. Text-books which have gradually been invoking lines of force, in some respects to the aid of learners and in others to their bewilderment, will now fearlessly discourse of the stresses in the ether that cause electric and magnetic force. The younger generation will see clearly in electro-magnetic phenomena the working of the all-pervading ether, and this will give them a firm and true stand-point for further advances.

And now I want to spend a short time in explaining to you how the question has been decided. An illustrative example may make the question itself clearer, and so lead you to understand the answer better. In colloquial language we say that balloons, hot air, &c., rise because they are light. In old times this was stated more explicitly, and therefore much more clearly. It was said that they possessed a quality called "levity." "Levity" was opposed to "heaviness." Heaviness made things tend downwards, levity made things tend upwards. It was a sort of action at a distance. At least, it would have required such an hypothesis if it had survived until it was known that heaviness was due to the action of the earth. I expect levity would have been attributed to the direct action of heaven. It was comparatively recently in the history of mankind that the rising of hot air, flames, &c., was attributed to the air. Everybody knew that there was air, but it was not supposed that the upward motion of flames was due to it. We now know that this and the rising of balloons are due to the difference of pressure at different levels in the air. In a similar way we have long known that there is an ether, an all-pervading medium, occupying all known space. Its existence is a necessary consequence of the undulatory theory of light. People who think a little, but not much, sometimes ask me, "Why do you believe in the ether? What's the good of it?" I ask them, "What becomes of light for the eight minutes after it has left the sun and before it reaches the earth?" When they consider that, they observe how necessary the ether is. If light took no time to come from the sun, there would be no need of the ether. That it is a vibratory phenomenon, that it is affected by matter it acts through—these could be explained by action at a distance very well. The phenomena of interference would, however, require such complicated and curious laws of action at a distance as practically to put such an hypothesis out of court, or else be purely mathematical expressions for wave propagation. In fact, anything except propagation in time is explicable by action at a distance. It is the same in the case of electro-magnetic actions. There were two hypotheses as to the causes of electro-magnetic actions. One attributed electric attraction to a property of a thing called electricity to attract at a distance, the other attributed it to a pull exerted by means of the ether, somewhat in the way that air pushes balloons up. We do not know what the structure of the ether is by means of which it can pull, but neither do we know what the structure of a piece of india-rubber is by means of which it can pull; and we might as well ignore the india-rubber, though we know a lot about the laws of its action, because we do not know its structure, as to ignore the ether because we do not know its structure. Anyway, what was wanted was an experiment to decide between the hypothesis of direct action at a distance and of action by means of a medium. At the time that Clerk Maxwell delivered his address no experiment was known that could decide between the two hypotheses. Specific inductive capacity, the action of intervening matter, the delay in telegraphing, the time propagation of electro-magnetic actions by means of conducting material—these were known, but he knew that they could be explained by means of action at a distance, and had been so explained. Waves in a conductor do not necessarily postulate action through a medium such as the ether. When we are dealing with a conductor and a thing called electricity running over its surface, we are, of course, postulating a medium on or in the conductor, but not outside it, which is the special point at issue. Clerk Maxwell *believed* that just as the same air that transmits sound is able by differences of pressure—*i.e.* by means of its energy per unit volume—to move bodies immersed in it, so the same ether that transmits light causes electrified bodies to move by means of its energy per unit volume. He believed this, but there was no experiment known then to decide between this hypothesis and that of direct action at a distance. As I have endeavoured to impress upon you, no *experimentum crucis* between the hypotheses is possible except an experiment proving propagation in time, either directly, or indirectly by an experiment exhibiting phenomena like those of the interference of light. A theorist may speak of propagation of actions in time without talking of a medium. This is all very well in mathematical formulæ, but, as in the case of light we must consider what becomes of it after it has left the sun and before it reaches the earth, so every hypothesis assuming action in time really postulates a medium whether we talk about it or not. There are some difficulties surrounding the complete interpretation of some of Hertz's experiments. The conditions are complicated, but I confidently expect that they will lead to a decision on most of the outstanding questions on the theory of electro-magnetic action. However, there is no doubt that he has observed the interference of electro-magnetic waves quite analogous to those of light, and that he has proved that electro-magnetic actions are propagated in air with the velocity of light. By a beautiful device Hertz has produced rapidly alternating currents of such frequency that their wave-length is only about 2 metres. I may pause for a minute to call your attention to what that means. These waves are propagated three hundred thousand kilometres in a second. If they vibrated three hundred thousand times a second, the waves would be each a kilometre long. This rate of vibration is much higher than the highest audible note, and yet the waves are much too long to be manageable. We want a vibration about a thousand times as fast again with waves about a metre long. Hertz produced such vibrations, vibrating more than a hundred million times a second. That is, there are as many vibrations in one second as there are seconds—in a day? No, far more. In a week? No, more even than that. The pendulum of a clock ticking seconds would have to vibrate for four months before it would vibrate as often as one of Hertz's vibrators vibrates in one second. And how did he detect the vibrations and their interference? He could not see them; they are much too slow for that; they should go about a million times as fast again to be visible. He could not hear them; they are much too quick for that. If they went a million times more slowly they would be well heard. He made use of the principle of resonance. You all understand how by a succession of well-timed small impulses a large vibration may be set up. It explains many things, from speech to spectrum analysis. It is related that a former Marquess of Waterford used the principle to overturn lamp-posts—his ambition soared above knocker-wrenching. So that it is a principle known to others besides scientific men. Hertz constructed a circuit whose period of vibration for electric currents was the same as that of his generating vibrator, and he was able to see sparks, due to the induced vibration, leaping across a small air-space in this resonant circuit. The well-timed electrical impulses broke down the air-resistance just as those of my Lord of Waterford broke down the lamp-post. The combination of a vibrating generating circuit with a resonant receiving circuit is one that I spoke of at the meeting of the British Association at Southport as one by which this very question might be studied. At the time I did not see any feasible way of detecting the induced resonance: I did not anticipate that it could produce sparks. By its means, however, Hertz has been able to observe the interference between waves incident on a wall and the reflected waves. He placed his generating vibrator several wave-lengths away from a wall, and placed the receiving resonant circuit between the generator and the wall, and in this air-space he was able to observe that at some points there were hardly any induced sparks, but at other and greater distances from his generator they reappeared, to disappear again in regular succession at equal intervals between his generator and the wall. It is exactly the same phenomenon as what are known as Lloyd's bands in optics, which are due to the interference between a direct and a reflected wave. It follows hence that, just as Young's and Fresnel's researches on the interference of light prove the undulatory theory of optics, so Hertz's experiment proves the ethereal theory of electro-magnetism. It is a splendid result. Henceforth I hope no learner will fail to be impressed with the theory—hypothesis no longer—that electro-magnetic actions are due to a medium pervading all known space, and that it is the same medium as the one by which light is propagated, that non-conductors can, and probably always do, as Prof. Poynting has taught us, transmit electro-magnetic energy. By means of variable currents energy *is* propagated into space with the velocity of light. The rotation of the earth is being slowly stopped by the diurnal rotation of its magnetic poles. This seems a hopeful direction in which to look for an explanation of the secular precession of terrestrial magnetism. It is quite different from Edlund's curious hypothesis that free space is a perfect conductor. If this were true, there would be a pair of great antipoles outside the air, and terrestrial magnetism would not be much like what it is, and I think the earth would have stopped rotating long ago. With alternating currents we *do* propagate energy through non-conductors. It seems almost as if our future telegraph-cables would be pipes. Just as the long sound-waves in speaking-tubes go round corners, so these electro-magnetic waves go round corners if they are not too sharp. Prof. Lodge will probably have something to tell us on this point in connection with lightning-conductors. The silvered glass-bars used by surgeons to conduct light are exactly what I am describing. They are a glass, a non-conducting, and therefore transparent, bar surrounded by a conducting, and therefore opaque, silver sheath, and they transmit the rapidly alternating currents we call light. There would not be the same difficulty in utilizing the energy of these electro-magnetic waves as in utilizing radiant heat. Having all the vibrations of the same period we might utilize Hertz's resonating circuits, and in any case the second law of thermodynamics would not trouble us when we could practically attain to the absolute zero of these, as compared with heat, long-period vibrations.

We seem to be approaching a theory as to the structure of the ether. There are difficulties from diffusion in the simple theory that it is a fluid full of motion, a sort of vortex-sponge. There were similar difficulties in the wave theory of light owing to wave propagation round corners, and there is as great a difficulty in the jelly theory of the ether arising from the freedom of motion of matter through it. It may be found that there is diffusion, or it may be found that there are polarized distributions of fluid kinetic energy which are not unstable when the surfaces are fixed: more than one such is known. Osborne Reynolds has pointed out another, though in my opinion less hopeful, direction in which to look for a theory of the ether. Hard particles are abominations. Perhaps the impenetrability of a vortex would suffice. Oliver Lodge speaks confidently of a sort of chemical union of two opposite kinds of elements forming the ether. The opposite sides of a vortex-ring might perchance suit, or maybe the ether, after all, is but an atmosphere of some infra-hydrogen element: these two latter hypotheses may both come to the same thing. Anyway we are learning daily what sort of properties the ether must have. It must be the means of propagation of light; it must be the means by which electric and magnetic forces exist; it should explain chemical actions, and, if possible, gravity.

On the vortex-sponge theory of the ether there is no real difficulty by reason of complexity why it should not explain chemical actions. In fact, there is every reason to expect that very much more complex actions would take place at distances comparable with the size of the vortices than at the distances at which we study the simple phenomena of electro-magnetism. Indeed, if vortices can make a small piece of a strong elastic solid, we can make watches and build steam-engines and any amount of complex machinery, so that complexity can be no essential difficulty. Similarly the instantaneous propagation of gravity, if it exists, is not an essential difficulty, for vortices each occupy all space, and they act on one another simultaneously everywhere. The theory that material atoms are simple vortex-rings in a perfect liquid otherwise unmoving is insufficient, but with the innumerable possibilities of fluid motion it seems almost impossible but that an explanation of the properties of the universe will be found in this conception. Anything purporting to be an explanation founded on such ideas as "an inherent property of matter to attract," or building up big elastic solids out of little ones, is not of the nature of an ultimate explanation at all; it can only be a temporary stopping-place. There are metaphysical grounds, too, for reducing matter to motion and potential to kinetic energy.

These ideas are not new, but it is well to enunciate them from time to time, and a Presidential address in Section A is a fitting time. Besides all this, it has become the fashion to indulge in quaint cosmical theories and to dilate upon them before learned Societies and in learned journals. I would suggest, as one who has been bogged in this quagmire, that a successor in this chair might well devote himself to a review of the cosmical theories propounded within the last few years. The opportunities for piquant criticism would be splendid.

Returning to the sure ground of experimental research, let us for a moment contemplate what is betokened by this theory that in electro-magnetic engines we are using as our mechanism the ether, the medium that fills all known space. It was a great step in human progress when man learnt to make material machines, when he used the elasticity of his bow and the rigidity of his arrow to provide food and defeat his enemies. It was a great advance when he learnt to use the chemical action of fire, when he learnt to use water to float his boats and air to drive them; when he used artificial selection to provide himself with food and domestic animals. For two hundred years he has made heat his slave to drive his machinery. Fire, water, earth, and air have long been his slaves, but it is only within the last few years that man has won the battle lost by the giants of old, has snatched the thunderbolt from Jove himself, and enslaved the all-pervading ether.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY W. BOYD DAWKINS, M.A., F.R.S., F.G.S., F.S.A., PROFESSOR OF GEOLOGY AND PALÆONTOLOGY IN OWENS COLLEGE, PRESIDENT OF THE SECTION.

IN taking the chair occupied twenty-four years ago in this place by my honoured master, Prof. Phillips, I have been much perplexed as to the most fitting lines on which to mould my address. It was open to me to deal with the contributions to our knowledge since our last meeting in Manchester in such a manner as to place before you an outline of our progress during the last twelve months. But this task, difficult in itself, is rendered still more so by the special circumstances of this meeting, attended, as it is, by so large a number of distinguished geologists, assembled from nearly every part of the world for the purposes of the Geological Congress. It would be presumptuous of me, in the presence of so many specialists, to attempt to summarize and co-ordinate their work. Indeed, we stand too near to it to be able to see the true proportions of the various parts. I will merely take this opportunity of offering to our visitors, in the name of this Section and of English geologists in general, a hearty welcome to our shores, feeling that not only will our science be benefited enormously by the simplification of geological nomenclature, but that we ourselves shall derive great advantage by a closer personal contact than we have enjoyed hitherto.

Our science has made great strides during the last twenty-four years, and she has profited much from the development of her sisters. The microscopic analysis of the rocks has opened out a new field of research, in which physics and chemistry are in friendly rivalry, and in which fascinating discoveries are being made almost day by day as to metamorphism, and the crushing and shearing forces brought to bear upon the cooling and contracting crust while the earth was young. The deep-sea explorations have revealed the structure and the deposits of the ocean abysses; and the depths supposed to be without life, like the fabled deserts in the interior of Africa, are now known to teem with varied forms glowing with the richest colours. From a comparison of these deposits with the stratified rocks we may conclude that the latter are marginal, and deposited in depths not greater than 1000 fathoms, or the shore end of the Globigerina ooze, and most of them at a very much less depth, and that consequently there is no proof in the geological record of the ocean depths having ever been in any other than their present places.

In North America the geological survey of the Western States has brought to light an almost unbroken series of animal remains, ranging from the Eocene down to the Pleistocene age. In these we find the missing links in the pedigree of the horse, and sufficient evidence of transitional forms to cause Prof. Flower to restore to its place in classification the order Ungulata of Cuvier. These may be expected to occupy the energies of our kinsmen on the other side of the Atlantic for many years, and to yield further proof of the truth of the doctrine of evolution. The use of this word reminds me how much we have grown since 1864, when evolution was under discussion, and when biological, physical, and geological laboratories could scarcely be said to have existed in this country. Truly may the scientific youth of to-day make the boast—

Ἡμεῖς μὲν πατέρων μέγ' ἀμείνονες εὐχόμεθ' εἶναι—

"We are much better off than our fathers were;" while we, the fathers, have the poor consolation of knowing that when they are fathers their children will say the same of them. There is reason to suppose that our science will advance more swiftly in the future than it has in the past, because it has more delicate and precise methods of research than it ever had before, and because its votaries are more numerous than they ever were.

In 1864 the attention of geologists was mainly given to the investigations of the later stages of the Tertiary period. The bent of my pursuits inclines me to revert to this portion of geological inquiry, and to discuss certain points which have arisen during the last few years in connection with the classificatory value of fossils, and the mode in which they may be best used for the co-ordination of strata in various parts of the world.

The principle of homotaxy, first clearly defined by Prof. Huxley, has been fully accepted as a guiding principle in place of synchronism or contemporaneity, and the fact of certain groups of plants and animals succeeding one another in a definite order, in countries remote from each other, is no longer taken to imply that each was living in the various regions at the same time, but rather, unless there be evidence to the contrary, that they were not. While, however, there is a universal agreement on this point among geologists, the classificatory value of the various divisions of the vegetable and animal kingdoms is still under discussion, and, as has been very well put by my predecessor in this chair at Montreal, sometimes the evidence of one class of organic remains points in one direction, while the evidence of another class points in another and wholly different direction, as to the geological horizon of the same rocks. The flora, put into the witness-box by the botanist, says one thing, while the Mollusca or the Vertebrata say another thing in the hands of their respective counsel. There seems to be a tacit assumption that the various divisions of the organic world present the same amount of variation in the rocks, and that consequently the evidence of every part of it is of equal value.

It will not be unprofitable to devote a few minutes to this question, premising that each case must be decided on its own merits, without prejudice, and that the whole of the evidence of the flora and fauna must be considered. We will take the flora first.

The Cryptogamic flora of the later Primary rocks shows but slight evidence of change. The forests of Britain and of Europe generally, and of North America, were composed practically of the same elements—Sigillaria, Calamites, and conifers allied to the Ginkho—throughout the whole of the Carboniferous (16,336 feet in thickness in Lancashire and Yorkshire) and Devonian rocks, and do not present greater differences than those which are to be seen in the existing forests of France and Germany. They evidently were continuous both in space and time, from their beginning in the Upper Silurian to their decay and ultimate disappearance in the Permian age. This disappearance was probably due to geographical and climatic changes, following the altered relations of land to sea at the close of the Carboniferous age, by which Secondary plants, such as *Voltzia* and *Walchia*, were able to find their way by migration from an area hitherto isolated. The Devonian formation is mapped off from the Carboniferous, and this from the Permian, but to a slight degree by the flora, and nearly altogether by the fauna. While the fauna exhibits great and important changes, the flora remained on the whole the same.

The forests of the Secondary period, consisting of various conifers and cycads, also present slight differences as they are traced upwards through the Triassic and Jurassic rocks, while remarkable and striking changes took place in the fauna, which mark the division of the formations into smaller groups. As the evidence stands at present, the cycads of the Lias do not differ in any important character from those of the Oolites or the Wealden, and the *Salisburia* in Yorkshire in the Liassic age is very similar to that of the Island of Mull in the Early Tertiary, and to that (*Salisburia adiantifolia*) now living in the open air in Kew Gardens.

Nor do we find evidence of greater variation in the Dicotyledonous forests, from their first appearance in the Cenomanian stage of the Cretaceous rocks of Europe and America, through the whole of the Tertiary period down to the present time. In North America, the flora of the Dakota series so closely resembles the Miocene of Switzerland, that Dr. Heer had no hesitation in assigning it in the first instance to the Miocene age. It consists of more than a hundred species, of which about one-half are closely allied to those now living in the forests of North America—sassafras, tulip, plane, willow, oak, poplar, maple, beech, together with *Sequoia*, the ancestor of the giant redwood of California. The first palms also appear in both continents at this place in the geological record.

In the Tertiary period there is an unbroken sequence in the floras, as Mr. Starkie Gardner has proved, when they are traced over many latitudes, and most of the types still survive at the present day, but slightly altered. If, however, Tertiary floras of different ages are met with in one area, considerable differences are to be seen, due to progressive alterations in the climate and altered distribution of the land. As the temperature of the northern hemisphere became lowered, the tropical forests were pushed nearer and nearer to the equator, and were replaced by plants of colder habit from the northern regions, until ultimately, in the Pleistocene age, the Arctic plants were pushed far to the south of their present habitat. In consequence of this, Mr. Gardner concludes that "it is useless to seek in the Arctic regions for Eocene floras as we know them in our latitudes, for during the Tertiary period the climatic conditions of the earth did not permit their growth there. Arctic fossil floras of temperate and therefore Miocene aspect are, in all probability, of Eocene age, and what has been recognized in them as a newer or Miocene facies is due to their having been first studied in Europe in latitudes which only became fitted for them in Miocene times. When stratigraphical evidence is absent or inconclusive, this unexpected persistence of plant types or species throughout the Tertiaries should be remembered, and the degrees of latitude in which they are found should be well considered before conclusions are published respecting their relative age."

This view is consistent with that held by the leaders in botany —Hooker, Dyer, Saporta, Dawson, and Asa Gray (whose recent loss we so deeply deplore)—that the North Polar region is the centre of dispersal, from which the Dicotyledons spread over the northern hemisphere. If it be true—and I, for one, am prepared to accept it—it will follow that for the co-ordination of the subdivisions of the Tertiary strata in various parts of the world the plants are uncertain guides, as they have been shown to be in the case of the Primary and Secondary rocks. In all cases where there is a clash of evidence, such as in the Laramie lignites, in which a Tertiary flora is associated with a Cretaceous fauna, the verdict, in my opinion, must go to the fauna. They are probably of the same geological age as the deposit at Aix-la-Chapelle.

I would remark, further, before we leave the floras behind us, that the migration of new forms of plants into Europe and America took place before the arrival of the higher types in the fauna, after the break-up of the land at the close of the Carboniferous period, and after the great change in geography at the close of the Neocomian. The Secondary plants preceded the Secondary vetebrates by the length of time necessary for the deposit of the Permian rocks, and the Tertiary plants preceded the Tertiary vertebrates by the whole period of the Upper Cretaceous.

Let us now turn to the fauna.

Prof. Huxley, in one of his many addresses which have left their mark upon our science, has called attention to the persistence of types revealed by the study of palæontology, or, to put it in other words, to the singularly little change which the ordinal groups of life have undergone since the appearance of life on the earth. The species, genera, and families present an almost endless series of changes, but the existing orders are for the most part sufficiently wide, and include the vast series of fossils without the necessity of framing new divisions for their reception. The number of these extinct orders is not equally distributed through the animal kingdom. Taking the total number of orders at 108, the number of extinct orders in the Invertebrata amounts only to 6 out of 88, or about 7 per cent., while in the Vertebrates it is not less than 12 out of 40, or 30 per cent. These figures imply that the amount of ordinal change in the fossil Vertebrates stands to that in the Invertebrata in the ratio of 30 to 7. This disproportion becomes still more marked when we take into account that the former had less time for variation than the latter, which had the start by the Cambrian and Ordovician periods. It follows also that as a whole they have changed faster.

The distribution of the extinct orders in the animal kingdom, taken along with their distribution in the rocks, proves further that some types have varied more than others, and at various places in the geological record. In the Protozoa, Porifera, and Vermes there are no extinct orders; among the Cœlenterates one—the Rugosa; in the Echinodermata three—Cystideans, Edriasterida, and Blastoidea; in the Arthropoda two—the Trilobita and Eurypterida. All these, with the solitary exception of the obscure order Rugosa, are found only in the Primary rocks. Among the Pisces there are none; in the Amphibia one; the Labyrinthodonts ranging from the Carboniferous to the Triassic age. Among the Reptilia there are at least six of Secondary age—Plesiosauria, Ichthyosauria, Dicynodontia, Pterosauria, Theriodontia, Deinosauria; in the Aves two—the Saururæ and Odontornithes, also Secondary. In the Mammalia the Amblypoda, Tillodontia, Condylarthra, and Toxodontia represent the extinct orders—the three first Early Tertiary, and the last Pleistocene. It is clear, therefore, that, while the maximum amount of ordinal variation is presented by the Secondary Reptilia and Aves, all the extinct orders in the Tertiary are Mammalian.

If we turn from the extinct orders to the extinct species, it will also be found that the maximum amount of variation is presented by the plants, and all the animals, excepting the Mammalia, in the Primary and Secondary periods.

The general impression left upon my mind by these facts is that, while all the rest of the animal kingdom had ceased to present important modifications at the close of the Secondary period, the Mammalia, which presented no great changes in the Secondary rocks, were, to quote a happy phrase of Prof. Gaudry, "en pleine évolution" in the Tertiary age. And when, further, the singular perfection of the record allows us to trace the successive and gradual modifications of the Mammalian types from the Eocene to the close of the Pleistocene age, it is obvious that they can be used to mark subdivisions of the Tertiary period, in the same way as the reigns of kings are used to mark periods in human history. In my opinion they mark the geological horizon with greater precision than the remains of the lower members of the animal kingdom, and in cases such as that of Pikermi, where typical Miocene forms, such as Deinotheria, are found in a stratum above an assemblage of marine shells of Pliocene age, it seems to me that the Mammalia are of greater value in classification than the Mollusca, some of the species of which have been living from the Eocene down to the present day.

Yet another important principle must be noted. The fossils are to be viewed in relation to those forms now living in their respective geographical regions. The depths of the ocean have been where they are now since the earliest geological times, although continual geographical changes have been going on at their margins. In other words, geographical provinces must have existed even in the earlier geological periods, although there is reason to believe that they did not differ so much from each other as at the present day. It follows from this that the only just standard for comparison in dealing with the fossils, and especially of the later rocks, is that which is offered by the fauna and flora of the geographical province in which they are found. The non-recognition of this principle has led to serious confusion. The fauna, for example, of the Upper Sivalik formation has been very generally viewed from the European stand-point and placed in the Miocene, while, judged by the stand-point of India, it is really Pliocene. A similar confusion has followed from taking the Miocene flora of Switzerland as a standard for the Tertiary flora of the whole of the northern hemisphere.

It now remains for us to see how these principles may be applied to the co-ordination of Tertiary strata in various parts of the world. In 1880 I proposed a classification of the European Tertiaries, in which, apart from the special characteristic fossils of each group, stress was laid on the gradual approximation of various groups to the living Mammalia. The definitions are the following:—

DIVISIONS.	CHARACTERISTICS.
1. Eocene, or that in which the higher Mammalia (Eutheria) now on the earth were represented by allied forms belonging to existing orders and families. Oligocene.	Extinct orders. Living orders and families. No living genera.
2. Miocene, in which the alliance between fossil and living Mammals is closer than before.	Living genera. No living species.
3. Pliocene, in which living species of Mammals appear.	Living species few. Extinct species predominant.
4. Plistocene, in which living species of Mammals preponderate.	Living species abundant. Extinct species present. Man present.
5. Prehistoric, or that period outside history in which Man has multiplied exceedingly on the earth and introduced the domestic animals.	Man abundant. Domestic animals present. Wild Mammals in retreat. One extinct Mammal.
6. Historic, in which the events are recorded in history.	Records.

These definitions are of more than European significance. The researches of Leidy, Marsh, and Cope prove that they apply equally to the Tertiary strata of North America. The

Wasatch Bridger and Uinta strata contain representatives of the orders Cheiroptera and Insectivora, the sub-orders Artio- and Perissodactyla, and the families Vespertilionidæ and Tapiridæ; but no living genera.[1] The Mammalia are obviously in the same stage of evolution as in the Eocenes of Europe, although there are but few genera, and no species common to the two.

The White River and Loup Fork groups present us with the living genera *Sciurus*, *Castor*, *Hystrix*, *Rhinoceros*, *Dicotyles*, and others; but no living species, as is the case with the Miocenes of Europe. In the Pliocenes of Oregon the first living species appear, such as the Beaver, the Prairie Wolf, and two Rodents (*Thomomys clusius* and *T. talpoides*), while in the Pleistocene river deposits and caves, from Eschscholtz Bay in the north to the Gulf of Mexico in the south, there is the same grouping of living with extinct species as in Europe, and the same evidence in the glaciated regions that the Mammalia occupied the land after the retreat of the ice.

If we analyze the rich and abundant fauna yielded by the caves and river deposits both of South America and of Australia, it will be seen that the Pleistocene group in each is marked by the presence of numerous living species in each, the first being remarkable for their gigantic extinct Edentata, and the second for their equally gigantic extinct Marsupials.

The admirable work of Mr. Lydekker allows us also to see how these definitions apply to the fossil Mammalia of India. The Miocene fauna of the Lower Sivaliks has yielded the living genera *Rhinoceros* and *Manis*, and no living species.

The fauna of the Upper Sivaliks, although it has only been shown, and that with some doubt, to contain one living Mammal, the Nilghai (*Boselaphus tragocamelus*), stands in the same relation to that of the Oriental Region as that of the Pliocenes of Europe to that of the Palæarctic Region, and is therefore Pliocene. And lastly, the Narbada formation presents us with the first traces of Palæolithic Man in India in association with the living one-horned Rhinoceros, the Nilghai, the Indian Buffalo, two extinct Hippopotami, Elephants, and others, and is Pleistocene.

It may be objected to the Prehistoric and Historic divisions of the Tertiary period that neither the one nor the other properly fall within the domain of geology. It will, however, be found that in tracing the fauna and flora from the Eocene downwards to the present day there is no break which renders it possible to stop short at the close of the Pleistocene. The living plants and animals were in existence in the Pleistocene age in every part of the world which has been investigated. The European Mollusca were in Europe in the Pliocene age. The only difference between the Pleistocene fauna, on the one hand, and the Prehistoric, on the other, consists in the extinction of certain of the Mammalia at the close of the Pleistocene age in the Old and New Worlds, and in Australia. The Prehistoric fauna in Europe is also characterized by the introduction of the ancestors of the present domestic animals, some of which, such as the Celtic shorthorn (*Bos longifrons*), sheep, goat, and domestic hog, reverted to a feral condition, and have left their remains in caves, alluvia, and peat-bogs over the whole of the British Isles and the Continent. These remains, along with those of Man in the Neolithic, Bronze, and Iron stages of culture, mark off the Prehistoric from the Pleistocene strata. There is surely no reason why a cave used by Palæolithic Man should be handed over to the geologist, while that used by men in the Prehistoric age should be taken out of his province, or why he should be asked to study the lower strata only in a given section, and leave the upper to be dealt with by the archæologist. In these cases the ground is common to geology and archæology, and the same things, if they are looked at from the stand-point of the history of the earth, belong to the first, and, if from the stand-point of the history of Man, to the second.

If, however, there be no break of continuity in the series of events from the Pleistocene to the Prehistoric ages, still less is there in those which connect the Prehistoric with the period embraced by history. The historic date of a cave or of a bed of alluvium is as clearly indicated by the occurrence of a coin as the geological position of a stratum is defined by an appeal to a characteristic fossil. The gradual unfolding of the present order of things from what went before compels me to recognize the fact that the Tertiary period extends down to the present day. The Historic period is being recorded in the strata now being formed, exactly in the same way as the other divisions of the Tertiary have left their mark in the crust of the earth, and history is incomplete without an appeal to the geological record. In the masterly outline of the destruction of Roman civilization in Britain the historian of the English Conquest was obliged to use the evidence, obtained from the upper strata, in caves which had been used by refugees from the cities and villas; and among the materials for the future history of this city there are, to my mind, none more striking than the proof, offered by the silt in the great Roman bath, that the resort of crowds had become so utterly desolate and lonely in the ages following the English Conquest as to allow of the nesting of the wild duck.

I turn now to the place of Man in the geological record, a question which has advanced but little since the year 1864. Then, as now, his relation to the glacial strata in Britain was in dispute. It must be confessed that the question is still without a satisfactory answer, and that it may well be put to "a suspense account." We may, however, console ourselves with the reflection that the River-drift Man appears in the Pleistocene strata of England, France, Spain, Italy, Greece, Algiers, Egypt, Palestine, and India along with Pleistocene animals, some of which were pre-glacial in Britain. He is also proved to have been post-glacial in Britain, and was probably living in happy, sunny, southern regions, where there was no ice, and therefore no Glacial period, throughout the Pleistocene age.

It may further be remarked that Man appears in the geological record where he might be expected to appear. In the Eocene the Primates were represented by various Lemuroids (*Adapis*, *Necrolemur*, and others) in the Old and New Worlds. In the Miocene the Simiadæ (*Dryopithecus*, *Pliopithecus*, *Oreopithecus*) appear in Europe, while Man himself appears, along with the living species of Mammalia, in the Pleistocene Age, both in Europe and in India.

The question of the antiquity of Man is inseparably connected with the further question: "Is it possible to measure the lapse of geological time in years?" Various attempts have been made, and all, as it seems to me, have ended in failure. Till we know the rate of causation in the past, and until we can be sure that it has been invariable and uninterrupted, I cannot see anything but failure in the future. Neither the rate of the erosion of the land by sub-aërial agencies, nor its destruction by oceanic currents, nor the rate of the deposit of stalagmite or of the movement of the glaciers, has as yet given us anything at all approaching a satisfactory date. We only have a sequence of events recorded in the rocks, with intervals the length of which we cannot measure. We do not know the exact duration of any one geological event. Till we know both, it is surely impossible to fix a date, in terms of years, either for the first appearance of Man or for any event outside the written record. We may draw cheques upon "the bank of force" as well as "on the bank of time."

Two of my predecessors in this chair, Dr. Woodward and Prof. Judd, have dealt with the position of our science in relation to biology and mineralogy. Prof. Phillips in 1864 pointed out that the later ages in geology and the earlier ages of mankind were fairly united together in one large field of inquiry. In these remarks I have set myself the task of examining that side of our science which looks towards history. My conception of the aim and results of geology is that it should present a universal history of the various phases through which the earth and its inhabitants have passed in the various periods, until ultimately the story of the earth, and how it came to be what it is, is merged in the story of Man and his works in the written records. Whatever the future of geology may be, it certainly does not seem likely to suffer in the struggle for existence in the scientific renascence of the nineteenth century.

[1] The genus *Vesperugo* has not been satisfactorily determined.—Cope, "Report of Geol. Survey of the Territories: Tertiary Vertebrata," i., 1884.

NOTES.

MAJOR-GENERAL PRJEVALSKY started on Thursday last on his fifth journey of exploration in Tibet, with the intention of penetrating, if possible, into Lhassa, the capital. The General, with his officers and Cossacks, will this time take advantage of the new Central Asian railway as far as Samarcand, whence they will proceed to Semiretchinsk, and so to the Tibetan table-lands. General Prjevalsky will, it is thought, on this occasion have the best chance ever afforded him of entering the forbidden residence of the Dalai Lama.

COLONEL HEAVISIDE, of the Indian Survey Department, has retired after more than twenty years' service in the Department, during which he had charge of several important geodetic and geographical operations, notably the completion and extension of the series of pendulum observations formerly carried on by Captain Basevi.

A SERIOUS earthquake, which was felt throughout both islands, occurred in New Zealand on the morning of the 1st instant. There were five distinct shocks, extending over the space of nearly half an hour. At Christchurch the spire of the Cathedral was destroyed, and other buildings were damaged. The inhabitants at first fled from their homes, but returned later when the danger appeared over. Another shock has since been reported from Westport, on the south-west coast of the Nelson district.

DURING the month of August at the Granton Marine Station, the use of which was kindly granted by Dr. Murray of the *Challenger*, Mr. Patrick Geddes and Mr. T. Arthur Thomson conducted a class of over thirty students of both sexes—teachers, medical students, and others from various parts of Scotland and England—through a course of lectures and laboratory work in botany and zoology. The work at Granton was supplemented by visits to the Botanical Gardens, Museum, &c., and by field and marine excursions, including a day's dredging in the Firth of Forth. This is the second year of the course, and it is meant to be continued in future years.

A CORRESPONDENT of the *Daily News* gives the following account of the recent eruption of Bandai-San in Northern Japan :—"The rumbling and trembling of the earth have now stopped, but the mountain still belches forth smoke, and there are evidences that mighty subterranean forces are still at work. The place where the disaster occurred has been and is greatly changing, mountains have risen where there were none before, and large lakes appearing where once there were only rice fields. This being so, it is with the greatest difficulty that guides can be procured, as none can tell where a road now leads and how far it is passable. Landmarks are obliterated, and villages which but a week ago nestled among the rich and plentiful vegetation of the mountain-side are now beneath twenty feet of ash and cinders. The wounded are receiving treatment in the school-house at Inawashiro, but their condition is terrible. Some have fractured skulls, the majority broken limbs, while others are fearfully burned. Five villages have been totally buried. The state of the bodies recovered resembles the appearance of victims of a huge boiler explosion. Many are cut to pieces, and others parboiled, so that it is difficult to distinguish sex. But the most ghastly sights which met the eye of the helpers were bodies dangling on the branches of blackened and charred trees. Thrown into the air by the awful violence of the eruption, their descent had in many cases been arrested by the trees, and there the victims hung, their bodies exposed to the cruel and well-nigh ceaseless rain of red-hot cinders and burning ashes. From appearances death speedily relieved them from their agony, yet, short as the time was, their sufferings must have been past belief. In other places flesh hangs from the branches of trees as paper from London telegraph wires. Bandai-San is composed of five separate peaks, of which the largest is called Great Bandai. The second is a perfectly smooth mountain. The third is called Kushigamine, and is the second in height. The fourth is called the Middle or Northern Bandai, and is the one which broke forth ; while the fifth, which is called the Small Bandai, is close to the fourth. Great Bandai is only covered with white ashes, but No. 2 has been greatly shaken, while all the trees above the centre of the mountain have been destroyed. From No. 3 large stones and boulders have been hurled to the bottom, and from half-way down the mountain its sides are covered with bluish earth. No. 4, from which the eruption really occurred, has been entirely blown away, the lighter pieces ejected from it being swept away over the neighbouring mountains, whilst the heavier pieces were carried some five or seven miles, and have formed a table-land at its base, covered with stones and ashes. No report has been received as to any foreigners having been within the fatal region at the time of the occurrence."

M. CHEVREUL entered his 103rd year last week. On Tuesday he was able to walk through the Sanitary Exhibition at the Palace of Industry.

THE twenty-fifth annual meeting of the British Pharmaceutical Association is being held in Bath this week. On Monday evening the President, Mr. F. Baden Benger, and other officers of the Conference held a reception at the Grand Hotel, followed by a *conversazione*. The opening meeting took place on Tuesday morning. The Presidential address dealt largely with the progress of the Association since its establishment, and with the preliminary education of pharmacists.

THE thirty-seventh meeting of the American Association for the Advancement of Science was held at Cleveland, Ohio, on August 15 and following days. *Science* states that the meetings were not as well attended as in past years, but the whole gathering was nevertheless successful. The largest attendance of members appears to have been 303. The scientific departments at Washington were well represented, and the most prominent scientific men of the country were present. According to the secretary's report, the financial condition of the Association is excellent. The research fund, consisting of the contributions of life members, amounts to more than 4400 dollars. The subject of the address of Prof. Langley, the retiring President, was the history of the theory of radiant heat, which we hope to reprint *in extenso*, if space permits, on a future occasion. Prior to the meeting, advantage was taken of the presence of a number of American geologists to take the preliminary steps for the establishment of an American Geological Society. In its general report of the meeting, *Science* refers specially to a lecture delivered by Prof. Stanley Hall. "It was the first time that the new psychology had been given a place on the programme of the Association. . . . Prof. Hall gave a brief review of the scope of experimental psychology. He dwelt on the researches made in the study of psychologic physiology, and on the functions of brain and nerves ; he mentioned the methods of psychophysic inquiries, and the important bearing of ethnological studies upon psychological questions. He concluded his sketch, which was listened to with the greatest attention, with a reference to the study of hypnotism, which is one of the most promising fields of psychic research." Major Powell is the President for the current year, and Prof. Mendenhall for next year.

MR. COOK, the President of the Section of Geology and Geography, took for the subject of his address the International Geological Congress, and the part of American geologists in it. He recalls the fact that in 1876 the Association originated the Congress of Geologists in Paris in 1878 for the settling of obscure points relating to geological classification and nomenclature ; since that time similar Congresses have been held in Bologna and Berlin, and one is about to be held in London, but, says Mr. Cook, a meeting of the Congress must be held in the United States, and American geology must be fully represented, before any conclusion can be reached which will be accepted by the scientific world, and therefore an attempt will be made at the London Congress to have the meeting of 1891 held in the United States. The discussion on the important topics here mentioned should not be regarded as closed until after the American meeting, and he defines the business of American geologists, prior to the meeting, to be the preparation of a case which will fairly "present the claims of American geology to representation in a general system of geology."

THE Session of the Central Institution of the City and Guilds of London Institute will commence on October 2. The Clothworkers', Siemens's, Mitchell, and Institute's Scholarships will be competed for at an examination held on September 25 to 28. According to the Annual Report for the past year there has again been a large increase in the total number of candidates examined. In 1887, 5508 were examined, of whom 3090 passed; in 1888, 6166 were examined, of whom 3510 passed. The number of centres increased in the same period from 216 to 240, while another subject, viz. practical bread-making, was added to the list of subjects, which now number 49. This year, for the second time, examinations were held in New South Wales, candidates presenting themselves from Sydney, Bathurst, and Newcastle. The worked papers, as well as specimens of the hand-work of the candidates, were forwarded to this country in time for the inclusion of the results in the present Report. The number of colonial candidates has increased from 48 to 51, and the number of those who have passed from 31 to 34. 10,404 students were receiving instruction in the United Kingdom in 475 classes, in 183 different towns. Last year the corresponding numbers were 8613 students, 365 classes, and 121 towns; and these figures do not include the students at the Finsbury Technical College, the Yorkshire College, Leeds, and other Colleges the Professors of which do not receive grants on results, and the candidates from which are classed as "external." With the establishment of new Polytechnic Institutions in different parts of London, it is anticipated that there will be a large increase in the number of students in the technical classes registered by the Institute and in the number of candidates for examination. In most of the chemical subjects the number of candidates is diminishing, and the majority have received their instruction in institutions which obtain no help from the Institute by way of payment on results.

THE most interesting paper in the recent number of the Journal of the Anthropological Society of Bombay is Mr. Fawcett's account of the Saoros or Sowrahs of the Ganjam Hill Tracts. A good deal of Mr. Fawcett's paper is devoted to the investigation of the religious ideas, sacrifices, and funeral rites of the Saoros, and his account furnishes an interesting illustration of several well-known phenomena of early forms of religious belief. The objects of worship fall into two classes: malevolent deities, such as Jalia, Kanni, and Laukan, the sun, and ancestral spirits. Every human being possesses a *kulba*, or soul, which departs from the body at death, but which still retains the ordinary tastes of the Saoro—*e.g.* for tobacco and liquor—and which must be satisfied, or it will haunt the living. In the more primitive parts of the country, everything a man possesses—weapons, cloths, his reaping-hook, and some money—are burnt with him; but this is falling out of use. A hut is built for the *kulba* to dwell in, and food is placed there; but the more important ceremony is the *guar*, which occurs later, the great feature of which is the erection of a stone to the memory of the deceased. Near each village, clusters of such stones, standing upright in the ground, may be seen. The *guar* gives the *kulba* considerable satisfaction; but it is not quite satisfied till the *karja* is celebrated: this being a great biennial feast to the dead, when, after the sacrifice of many buffaloes and the consumption of much liquor, every house in which there has been a death is burnt; the *kulba* is finally driven away to the jungle or the hill-side. Sacrifices are made to appease deities or *kulbas* who have done harm, and in every paddy-field, when the paddy is sprouting, as well as at harvest, an offering of a goat must be made. It does not appear, however, that human sacrifice, once so common among the Khonds, was ever practised by the Saoros. Like all other savages, the Saoros have their priests, or diviners, called *kudangs*, whose occupation seems to be partly hereditary. The *kudang*, like the modern medium, is able to interview the spirit of the deceased and to ascertain his wishes. The method of divination usually practised is that of dropping from a leaf-cup grains of rice, uttering the name of a deity as each falls, and so ascertaining which divinity is the cause of the disease or other calamity. A similar practice has long been known to be in force among the Khonds, though Mr. Fawcett does not mention the fact. An account is given of an exorcism witnessed by the author, in the case of a boy who had suffered much from fever, which was supposed to be caused by the sun. The *kudang* told Mr. Fawcett afterwards that he had given the deity a good talking to and turned him out. "No fear of that deity returning to the boy after what he had said to him!" The *kudangs*, however, it must be added, generally work like ordinary mortals, and even when they are called in to officiate as priests they do not seem, from the account given of their fasting and exertions, to get their rewards for nothing.

EUROPE cannot compete with the United States in the loftiness of its stations for taking meteorological observations. There are only two stations on the European continent which reach any very great height, being about 10,000 feet and 11,000 feet respectively. Among the stations in America is Pike's Peak, which has an altitude of 14,100 feet—or only about 1600 feet lower than the summit of Mont Blanc—and exceeding by more than 3000 feet any meteorological station in Europe. These great heights are much more accessible in the United States than in Europe, there being five stations in America where a height of 11,000 feet or more is reached by railroads built fo facilitating mining work. The highest of those in North America Mount Lincoln, in Colorado, the mining works on which are 14,297 feet above the sea-level, and it has a meteorological station conducted by Harvard College. Another station is placed part way up the mountain, at a height of 13,500 feet. In the Andes Range, in Peru, continuous meteorological observations are also carried on, the loftiest point for this purpose being 14,300 feet above the level of the sea.

A CORRESPONDENT of the *Daily News* in Lucerne sends to that paper an account of an electric mountain railway—the first of its kind—which has recently been opened to the public at the Burgenstock, near Lucerne. Hitherto it has been considered impossible to construct a funicular mountain railway with a curve; but the new line up the Burgenstock has achieved that feat under the superintendence of Mr. Abt, the Swiss electrical engineer. The rails describe one grand curve formed upon an angle of 112°, and the journey is made as steadily and smoothly as upon any of the straight funiculars previously constructed. A bed has been cut, for the most part out of the solid rock, in the mountain-side from the shore of the Lake of Lucerne to the height of the Burgenstock—1330 feet above its level, and 2860 feet above the level of the sea. The total length of the line is 938 metres, and it commences with a gradient of 32 per cent., which is increased to 58 per cent. after the first 400 metres, and this is maintained for the rest of the journey. A single pair of rails is used throughout, with the exception of a few yards at half distance to permit the two cars to pass. Through the opposition of the Swiss Government, each car is at the present time only allowed to run the half distance, and they insist upon the passengers changing, in order, as they say, to avoid collision or accident. A number of journeys were made up and down the mountain in company with an engineer, and the experience is sufficient to prove that the prohibition is altogether unnecessary. The motive power, electricity, is generated by two dynamos, each of 25 horse-power, which are worked by a water-wheel of 125 horse-power, erected upon the River Aar at its mouth at Buochs three miles away. Only one man is required to manage the train, and the movement of the cars is completely under his control. One dynamo is sufficient to perform the work of haul-ing up and letting down the cars containing fifty or sixty persons. At the end of the journey, completed in about fifteen minutes, at an ordinary walking speed, the car moves gently against a spring buffer, and is locked by a lever, without noise and without jolting the passengers. This interesting undertaking has been carried out at a cost of £25,000.

MR. E. T. DUMPLE, writing in the *Geological Bulletin* of Texas, brings out a very interesting fact, and one which may shed some light upon the question of who were the builders of the shell-mounds of the coast regions of Texas. During the great storm of 1886, which so nearly destroyed Sabine Pass, one of these shell-mounds, which was near a certain house on the river-bank, and the locality of which was exactly known, was destroyed or carried away by the violence of the waves, and rebuilt nearly half a mile farther up stream than it formerly stood. It is therefore possible that these so-called Indian shell-mounds, which are composed almost entirely of shells, with fragments of pottery, and sometimes a crumbling bone or two, were not built, as has been supposed, by Indian tribes who lived on shell-fish, but are entirely due to the action of the water; and the presence of the Indian relics may be easily accounted for by remembering that these mounds are usually found in low ground, and, being high and dry, would naturally be selected as camping-places by the Indians in their hunting and fishing expeditions.

THE Vienna Correspondent of the *Times* records a curious relic of mediæval superstition in Austria. The Burgomaster of Zuraki, in Galicia, has just instituted a prosecution before the Criminal Court of Solotwina against a man named Jean Kowale-ink for having, "by his malicious sorceries and incantations, caused a hailstorm to devastate the fields of Zuraki on July 28." The damages occasioned by Kowalesink's uncanny power over the elements are laid at 6000 florins.

WE are glad to report that the Central Meteorological Observatory of Mexico has recommenced the publication of its *Boletin Mensual*, and in a more convenient form than before. This publication had been discontinued since December 1885. It contains only a summary of the observations made at twenty or thirty stations, but the hope is expressed that the publication of the observations made at certain hours will be soon undertaken, and that the arrears will also be taken up, as the observations have been regularly made. The Bulletins for the first five months of this year have been received.

THE Report of the Meteorological Commission of the Cape of Good Hope, for the year 1887, states that "the whole service has assumed a satisfactory character." Monthly and yearly summaries are given for twenty-nine stations, and for a large number of rainfall stations. As an inducement to observers, they are presented with the instruments with which they have made a series of satisfactory observations for a continuous period of five years. Summarized reports are sent daily to each coast port, and are there entered on a sketch-map for the benefit of the seafaring community. We observe, however, that in counting the number of wet days, a rainy day is taken as one upon which 0·03 inch is recorded, whereas a quantity of 0·01 inch is the standard generally adopted in this country. The Commission express the hope that in time they may be able to issue storm warnings.

IN June last an interesting archæological discovery was made at Sönderby, on the west coast of Jutland. It consisted of about thirty urns of clay found in a moss at a depth of 3 feet. They occupied an area 4 feet wide and 10 feet long. Formerly there was a shallow lake here. Most of the vessels rested upon rough stones, but there was no trace of stone walls or roof; they varied from 2 to 8 inches in height. In most of them lay ashes and remnants of calcined bones, whilst the bottom was lined with some reed-like kind of grass. Some of the urns had lids, but others appear to have been placed in the earth open. Most of them were very simple in form, with smooth sides, but on some of the larger there were three knobs at the sides, and attempts at rough ornamentation. No metal or stone implement was found. In the same moss some huge oak trunks were also dug out.

A KIEL schoolmaster, Herr Spiedt, has excavated a so-called "Viking mound" in the south of Jutland, close to the old frontier between Denmark and Prussia. In the eastern edge remains of a skeleton were found, and in the centre an oaken coffin, nailed with iron nails, containing the skeleton of a tall powerful man was found; but no ornaments, weapons, or objects of any kind. The head pointed to the north-west. It was close to this mound that a Runic stone was found some years ago with the following inscription in runes: "King Svein set (raised) stone after (on the death of) Skarde, his homestead companion (probably meaning boy companion), who travelled west, and died in Hedeby." King Svein is the famous King Svein with the Double Beard, who ascended the thrones of England and Denmark on the death of his brother, King Canute, and his friend was one Skarde, who fought for him in this country. Hedeby was the ancient name for the town of Schleswig. It is believed that the skeleton is that of Skarde.

THE "Class-book of Elementary Chemistry," which Mr. W. W. Fisher, Aldrichian Demonstrator of Chemistry at Oxford, is preparing for the Clarendon Press Series, is nearly ready, and will be published in a few days.

THE additions to the Zoological Society's Gardens during the past week include a Small Hill Mynah (*Gracula religiosa*) from India, presented by Mr. Alexander Robertson; a Common Sheldrake (*Tadorna vulpanser*), British, presented by the Rev. H. H. Slater; an Avocet (*Recurvirostra avocetta*) from Holland, presented by Mr. J. Hoogerduyn; two Common Chameleons (*Chamæleon vulgaris*) from North Africa, presented by Mr. J. Alfred Lockwood; a Sea Anemone (*Bolvara eques*), a British Coral (*Caryophyllaca*, sp. inc.) from British Seas, presented by the Marine Biological Station, Plymouth, per Mr. G. C. Bourne; a Brown Bear (*Ursus arctos* ♂), European, a White-backed Piping Crow (*Gymnorhina leuconota*) from Australia, twelve Mandarin Ducks (*Æx galericulata*, 6 ♂, 6 ♀) from China, deposited; two White-headed Parrots (*Pionus senilis*) from Mexico, four Oyster-catchers (*Himantopus ostralegus*) from Holland, purchased.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *SEPTEMBER* 9–15.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on September 9

Sun rises, 5h. 28m.; souths, 11h. 57m. 1·8s.; sets, 18h. 26m.: right asc. on meridian, 11h. 12·9m.; decl. 5° 4′ N. Sidereal Time at Sunset, 17h. 43m.

Moon (at First Quarter September 12, 22h.) rises, 9h. 19m.; souths, 14h. 55m.; sets, 20h. 19m.: right asc. on meridian, 14h. 11·1m.; decl. 7° 48′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	6 45 ...	12 49 ...	18 53 ...	12 4·7 ...	0 2 N.
Venus ...	6 57 ...	12 59 ...	19 1 ...	12 14·9 ...	0 23 S.
Mars ...	12 25 ...	16 30 ...	20 35 ...	15 46·7 ...	21 30 S.
Jupiter ...	12 16 ...	16 34 ...	20 52 ...	15 51·0 ...	19 31 S.
Saturn ...	2 21 ...	9 55 ...	17 29 ...	9 10·2 ...	17 5 N.
Uranus ...	8 8 ...	13 43 ...	19 18 ...	12 58·9 ...	5 38 S.
Neptune..	21 1*...	4 48 ...	12 35 ...	4 2·4 ...	18 59 N.

* Indicates that the rising is that of the preceding evening.

Occultation of Star by the Moon (visible at Greenwich).

Sept.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
14	50 Sagittarii	6	22 58	0 4†	134 308

† Occurs on the following morning.

Sept.	h.	
11	10	Mars in conjunction with and 6° 7′ south of the Moon.
11	10	Jupiter in conjunction with and 3° 55′ south of the Moon.
11	14	Mars in conjunction with and 2° 12′ south of Jupiter.

Variable Stars.

Star.	R.A.	Decl.			
	h. m.	° ′		h. m.	
Algol	3 0·9	40 31 N.	Sept. 12,	2 37	*m*
			,, 14,	23 25	*m*
ζ Geminorum	6 57·5	20 44 N.	,, 13,	21 0	*M*
U Monocerotis	7 25·5	9 33 S.	,, 15,		*M*
Z Virginis	14 4·3	12 46 S.	,, 9,		*M*
δ Libræ	14 55·0	8 4 S.	,, 13,	21 16	*m*
U Coronæ	15 13·6	32 3 N.	,, 9,	3 23	*m*
U Ophiuchi	17 10·9	1 20 N.	,, 9,	2 0	*m*
		and at intervals of		20 8	
W Sagittarii	17 57·9	29 35 S.	Sept. 15,	15 0	*M*
T Herculis	18 4·9	31 0 N.	,, 13,		*m*
β Lyræ	18 46·0	33 14 N.	,, 14,	4 0	*M*
η Aquilæ	19 46·8	0 43 N.	,, 14,	4 0	*M*
S Sagittæ	19 50·9	16 20 N.	,, 11,	3 0	*m*
			,, 14,	3 0	*M*
X Cygni	20 39·0	35 11 N.	,, 15,	4 0	*m*
T Vulpeculæ	20 46·7	27 50 N.	,, 11,	0 0	*M*
			,, 12,	2 0	*m*
S Cephei	21 36·6	78 7 N.	,, 9,		*m*
δ Cephei	22 25·0	57 51 N.	,, 11,	0 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ε Persei	60°	37° N.	Swift; streaks.
,, α Tauri	72	15 N.	Swift; streaks.
	354	38 N.	Very swift.

GEOGRAPHICAL NOTES.

THE elaborate Report of Mr. Bourne on his journey in South-Western China, which has recently been laid before Parliament, and to which we referred recently in connection with the ethnology of the non-Chinese races of this region, is of much geographical interest. Part of Mr. Bourne's journey was already traversed in the reverse direction by Mr. A. R. Colquhoun, and described by him in his well-known work, "Across Chrysê." This observation applies to the route from Yunnan Fu, the capital of the province of that name, to Ssu-mao, and thence along the Tonquin frontier to Nanning on the West or Canton River. But Mr. Bourne traversed the region between Chung-king and Yunnan Fu, which, however, as it lies on one of the high roads across China into Burma, is not unfamiliar to Western readers, and he also crossed diagonally the province of Kweichow—one of the least known provinces in the Chinese Empire—from Nanning in Kwangsi to Chung-king in Szechuen. Here he travelled along unbeaten tracks for many weeks; but even where travellers had been before—and at best European travellers in Southern and South-Western China are extremely few and far between—his intimate knowledge of China and the Chinese, and the advantages which his official mission gave him, make his observations of exceptional value. He has also established the connection between the rivers of Northern Tonquin and the river system of Southern China. In regard to the seven route-sketches, which accompany the Report, of the different sections of the journey, Mr. Bourne explains that although the rate of travel (about 20 miles a day) precluded the idea of a running survey, it was easy to take notes of the prominent features of the country, as he walked nearly the whole way. These notes, which took the form of route-sketches, would, with an occasional position determined astronomically, have made it possible to give a much better idea of the country than the maps convey; but his record of astronomical observations, "which had cost him many a night's vigil," and portions of his route-sketches, were lost on the occasion of some riots in Chung-king, during which his house was attacked and looted. But the route-sketches of the last part of the journey were fortunately saved, and supply materials for a better map. There is likewise a vast number of careful meteorological observations. It is to be feared that the instinctive repulsion of the natural man to Blue-books, regardless of their contents, will prevent Mr. Bourne's Report from receiving the attention which it deserves. On a moderate computation, it would furnish materials for half a dozen works of travel such as those with which the public is made acquainted every year, which have their little day and cease to be. We have to go back to the Reports of Mr. Bourne's predecessors, Messrs. Baber and Hosie, to find any record of travel in China of equal interest and value.

Science reports that two important Expeditions left Rio de Janeiro in June for exploration and work in two of the least-known parts of the Brazilian territory. The first, sent out by the Ministry of War, under the command of Captain Bellarmino Mendonça, is to open a road from the town of Guarapuaba, on the frontier of the settled portion of the province of Parana, to the confluence of the Rivers Parana and Iguassu, and to found a military colony at the latter point. A road is also to be opened along the Parana River from the mouth of the Iguassu to the navigable portion of the river above the Sete Quedas Fall, and from this point to Guarapuaba, *viâ* the valley of the Piquiri. The founding of a colony at the mouth of the Iguassu, where the Argentines are already establishing themselves, will, aside from its military importance, prove of great value in peopling the valley of the Upper Parana, which has been deserted since the time of the expulsion of the Jesuits. By means of the Lower Parana the colony will have free water communication with Buenos Ayres and other markets of the Argentine Republic, where two of its natural products, lumber and matte, will find a ready sale. This will give at once to the proposed colony a commercial importance far beyond that of a purely military station, and will doubtless lead to the rapid spread of population along the Upper Parana and its tributaries, with their hundreds of miles of navigable waters. The second Expedition, consisting of three military engineers, Capt. Lourenço Telles, and Lieuts. Miranda and Villeray, is sent out under the auspices of the Sociedade de Geographia de Rio de Janeiro, the expenses being borne by the Ministry of Agriculture. It is to proceed to Cuiaba in the province of Mato Grosso, pass by land to the head-waters of the Paranapinga, and descend that river and the Sao Manoel or Tres Barras to the Tapajos, returning to Rio de Janeiro *viâ* Para by the Tapajos and Amazonas. This exploration will thus be a valuable complement to that of the Tapajos by Chandless, as the Sao Manoel and Paranapinga are almost absolutely unknown.

THE current number of the Proceedings of the Royal Geographical Society opens with a paper by Commodore Markham on Hudson's Strait as a navigable channel, which is a condensed narrative of former voyages from the time of Sebastian Cabot, coupled with an account of the author's own observations. Commodore Markham comes to the conclusion that the Strait is perfectly navigable and free from ice in August and later in the season. Mr. Portman has a most interesting paper on the Little Andamans, while General Walker discusses the well-worn theme of the hydrography of South-Eastern Tibet. The Persian *farsakh* cannot be of much value as a precise measure of length, for in a very learned paper, which concludes the number, General Houtum Schindler, of the Persian Telegraph Service, concludes that it is 3·915 miles, while in a footnote he gives the estimates of eight other authorities all differing from his own and from each other.

THE first number of vol. ix. of the *Bulletin* of the Paris Geographical Society is occupied with M. Maunoir's annual summary of the progress of geography and exploration during 1887. The work is as full and careful as these annual reviews by the same author usually are. The second number is wholly devoted to a record of the commemoration of the centenary of the death of Laperouse. The grand-nephew of the great navigator writes on his private life, and reproduces a number of his private and official letters. Lieutenant Courcel describes his voyage, and Vice-Admiral Paris recounts the history of the discovery of the remains of the expedition. The appendixes contain numerous papers relating to Laperouse and his companions, including a bibliography of works relating to the hero himself.

NOTES ON METEORITES.[1]

II.

Chemical Analysis.

We have seen that the main difference between the specimens of these bodies which have been collected is that some of them are mainly iron, some of them are mainly stone, and that there is a passage between these two conditions represented by falls in which we have a paste of iron including stony fragments.

We have now to enter into some points connected with their chemical constitution somewhat more in detail.

Of the chemical elements which are at present recognized as such, about one-fourth are found by chemical analysis to exist in meteorites. These, according to the tables given by Maskelyne,[2] Fletcher,[3] Smith, and others are as follows:—

Those that occur most constantly are:—

Hydrogen	Carbon
Iron	Oxygen
Nickel	Silicon
Magnesium	Phosphorus
Cobalt [4]	Sulphur;
Copper [4]	
Manganese	
Calcium	
Aluminium	

while the following occur less frequently or in smaller quantities:—

Lithium	Arsenic
Sodium	Antimony
Potassium	Chlorine
Strontium	Nitrogen.
Titanium	
Chromium	
Tin	

Of these elementary bodies only hydrogen, nitrogen, and carbon occur in an elementary condition.

Hydrogen and nitrogen are asserted to be occluded as gases by the stones. Carbon exists both in the form of graphite and diamond.

From the above lists it will be seen that among the elements most common in meteorites are recognized many which have a very wide distribution and exist in great quantities in the surface and envelopes of our planet. But this is true only of the elements.

Many mineral compounds terrestrially common are absent; perhaps the most striking case of all is the absolute absence of free quartz, whether crystallized or not, from meteorites, while terrestrially it is the most prevalent compound known, and enters into the composition of such common rocks as trachyte, felsite, syenite, gneiss, and granite.

Again, many of the chemical combinations met with are unknown to terrestrial mineralogy. The chemical compounds found in meteorites which are new to our mineralogy may be briefly referred to. Some are combinations with sulphur, as follows:—

Sulphur +	Iron	= Troilite
+	Calcium	= Oldhamite
+	{Calcium, Titanium}	= Osbornite
+	{Iron, Chromium}	= Daubréelite.

Phosphides of iron and nickel, forming varieties of so-called schreibersite, are met with.

It has already been stated that carbon in some form or other exists in most meteorites. Some of them are partly composed of this element compounded with hydrogen and oxygen.

This exists as a white or a yellowish crystallizable matter, soluble in ether and partly so in alcohol, and exhibiting the characters and the composition of one or more hydrocarbonaceous bodies with high melting-points.

The meteorites of Alais and Cold Bokkeweld are instances of this group. The former is of a black colour both internally and externally, is combustible, and contains sulphates of magnesium, calcium, sodium, and potassium, which are all soluble in water. The latter, after being experimented upon, left a residue which gave out a very bituminous smell; this substance was yellow, and it was found that it was only another form of carbon in a state of intimate mixture, amounting to about 1·67 per cent.

Some carbonaceous stones are dark gray in colour, have little lustre, and are soft; they contain no visible meteoric iron, but an abundance of light gray rounded bodies, among which are occasionally some with a dull metallic lustre and of a greenish-yellow colour, and others of a dark gray compact substance and of earthy character.[1]

Various *alloys* of nickel and iron also occur.

The different alloys which play the most important part have, according to Meunier, the following composition:—

	Density.	Formula.
Tænite	7·380	Fe_6Ni
Plessite	7·850	$Fe_{10}Ni$
Kamacite	7·652	$Fe_{14}Ni$
Braunine	(?)	$Fe_{16}Ni$

Among other minerals we may name—
Lawrencite, protochloride of iron;
Maskelynite, with the composition of labradorite;
Silica (as asmanite).

We now come to the common ground.

The following compounds are identical in composition and crystallographic character with minerals found on our globe:—

Magnetic pyrites	Fe_7S_8.
Magnetite	Fe_3O_4.
Chromite	$(Fe,Cr)_3O_4$.

Silicates, viz.—

Olivine varieties.
Enstatite and bronzite.
Diopside and augite.
Anorthite and labradorite.
Breunnerite.

Among gaseous compounds, the oxides of carbon have been detected in many meteorites, and it is asserted that these gases have been occluded by them in the same manner as the elementary gases hydrogen and nitrogen.

In the "irons" we deal chiefly with nickel-iron, magnesium, manganese, and copper, as metals.

In the "stones" we deal with combinations of magnesium, iron, oxygen, and silicon. One of the most usual substances is called olivine, and sometimes the olivine is in a slightly changed form, in which the quantity of iron is increased, and we get bronzite. Nickel-iron, manganese, and other substances are also found in the stones.

Chemical analysis of the irons has established in them, taken as a whole, the existence of the following mineral species.

(1) The general metallic mass, which consists of certain alloys, in which iron and nickel predominate to such an extent that the term nickel-iron is by common consent applied to it.

The nickel-iron is an alloy or compound special to meteorites, and the irons are chiefly composed of it. The tracery to which I have referred, observed on the metallic surface heated with acids, was discovered by Widmanstätten. The figures are caused by the crystallization of the mass: with the iron and nickel magnesium is always associated, *so that we get magnesium in all meteoric irons* as well as in the stones.

(2) Compounds of iron and carbon, principally campbelline and chalypite (Fe_2C).

(3) Troilite $(FeNi)_7S_8$, generally appearing as kidney-shaped masses.

(4) Schreibersite (Fe_4Ni_2P).

(5) Graphite.

(6) Stony grains, generally magnesium and iron silicates.

(7) Occluded gases.

[1] Continued from p. 428.
[2] Nature, vol. xii. p. 505.
[3] "Introduction to Study of Meteorites," p. 30.
[4] With regard to the presence of cobalt and copper, Dr. L. Smith says ("Mineralogy and Chemistry," p. 352):—"In every analysis that I have made of meteoric irons (over one hundred different specimens) cobalt has been invariably found, along with a minute quantity of copper."—Flight, "History of Meteorites," p. 164.

[1] Flight, *op. cit.* p. 211.

(8) The crust or varnish. This has been found to be due entirely to the oxidation of the metal. The formula of the crust of the Toluca meteorite is $Fe_2O_3(FeNi)O$, according to Meunier.

The quantities of occluded gases vary considerably. Hydrogen is the first to come out when a vacuum is produced, and in the cold—that is, when the tube containing the meteorite is not heated.

Thus, Graham found in the Lenarto meteorite, and in a comparative experiment with clean horse-shoe nails made of iron :—[1]

	Meteorite.	Nails.
Hydrogen	85·68	35·0
Carbonic oxide... ...	4·46	50·3
Carbonic acid	—	7·7
Nitrogen	9·86	7·0
	100·00	100·0

Mallet subsequently found in the meteorite picked up in Augusta County—[2]

Hydrogen	85·68
Carbonic oxide	4·46
Nitrogen	9·86

Dr. A. Wright subsequently determined the composition of the gases given off at different temperatures, using the Iowa meteorite. The results were as follows :—

	Hydrogen.	Carbonic oxide.	Carbonic acid.	Nitrogen
Cold	49	14	35	—
At 100° C. ...	4·54	0 (?)	95·46	—
At 200° C. ...	5·86	1·82	92·32	—
Red heat ...	87·53	0	5·56	6

As regards the so-called occluded gases, iron and stony meteorites, according to Wright, show a marked distinction. While the gases of the Lenarto iron contained 85·68 per cent. of hydrogen, those obtained from cosmical masses of the stony kind, such as the Iowa meteorite, are characterized by the presence of carbonic acid, which constitutes nine-tenths of the gas evolved at the temperature of boiling water, and about one-half of that given off at a low red heat.

This view of Wright's has been called in question by Mallet, who refers to his examination of the gases of the iron of Augusta Co., Virginia, where the ratio of the oxides of carbon to hydrogen is 4·3, and to his having pointed out in 1872 that hydrogen could no longer be regarded as the characteristic gaseous ingredient of meteoric iron.[3]

In the siderites, the iron varies from 80 to 98 per cent., and the nickel from 6 to 10 per cent. Sometimes the nickel is found in larger quantities, as in the iron of d'Octibbeha Co., Mississipi, found in the year 1854, which contained as much as 59 per cent., while the iron was only 37 per cent.

There is a singular circumstance connected with the varnish of stony meteorites which was observed by Reinsch in the meteorite of Krähenberg. The grains of metallic iron and troilite contained in the varnish show no signs of oxidation. In the meteorite of Morbihan, also, grains of nickel-iron project not only through the smooth inner but also the rough outer crust. It has been suggested that the surface of these meteorites was vitrified before it entered our air, or at all events those lower strata of it in which oxygen is abundant.[4]

In many cases minute chemical analysis has been most useful in showing that meteorites which have been found in different localities really belong to the same fall.

Prof. Nordenskjöld, on examining the Ställdalen meteorites (Sweden, June 28, 1876), found that they resembled some eight or nine others which he had before examined, although they were entirely unconnected as regards their date of appearance ; and that together they would form a well-marked group, but which, he observes, will probably be found to be only one among many similar groups of aërolites which will hereafter be detected.

The following short table brings together in a compact form the chief substances met with in meteorites. It will indicate the cause of the continued reference to the spectra of magnesium iron, and manganese in what follows.

Siderites.

Nickel-iron, manganese, copper.
Troilite = FeS.
Graphite.
Schreibersite = iron and nickel phosphide, with which magnesium is always associated.
Daubréelite = iron and chromium sulphide.

Siderolites.

Chondritic—

(α) Non-carbonaceous ... Olivine = chrysolite = peridot = $(MgFe)_2O_4Si$ = SiO_2 41·3, MgO 50·9, FeO 7·7.
Enstatite MgO_3Si = SiO_2 60, MgO 40.
Bronzite = enstatite, in which some magnesium is replaced by iron.
Nickel-iron, manganese.
Troilite.
Chromite = iron protoxide 32, chromium sesquioxide 68, + aluminium and magnesium.
Augite = pyroxene, SiO_2 55, CaO 23, MgO 16, MnO 0·5, FeO 4.
Silicate of calcium, sodium, and aluminium.

(β) Carbonaceous ... Carbon in combination with H and O.
Sulphates of Mg, Ca, Na, and K.

Non-chondritic—

Troilite.
Olivine.
Enstatite.
Bronzite.
Augite.
Anorthite

SPECTRAL ANALYSIS.

It is imperative that we should know what spectroscopic phenomena are presented by meteorites when they are exposed to temperatures either high or low, such that luminous effects are produced, however the heat which is associated with luminosity is caused.

To this end a great many investigations have been made, and one method of investigation has been the following.

A small portion of any particular meteorite, or still better some dust or filings is inserted in an end-on tube, which is placed in front of a spectroscope, so that a spectroscopic record of the luminosity may be obtained. The tube is at the same time attached to a Sprengel pump, so that in this way a vacuum can be obtained, and is supplied with poles, so that an electric current can be sent through it. Supposing that such bodies as meteorites exist in free space, we must understand that they exist practically in a vacuum, so that it is a fair thing to begin the laboratory work by getting as nearly a vacuum as possible. The next thing to do is to try the effect of the lowest temperature, and for that purpose the central part of the tube containing the little fragments is heated by a Bunsen burner.

If any effect is produced by this application of heat it will after some little time be evidenced by the commencement of a spectrum or by some change in the pre-existing one. What has been found is that there is scarcely any meteorite which can be examined in this way which does not give off a sufficient quantity of hydrogen to allow the hydrogen spectrum, when a feeble electric current is made to travel along the tube, to be very beautifully visible.

If the temperature of the meteoric particles is kept sufficiently low, we see practically the spectrum of hydrogen alone. That is a demonstration of the very well known fact that with those bodies generally acknowledged to enter into the composition of meteorites, hydrogen is always associated.

If under these same conditions the temperature is increased, the spectrum of carbon begins to be visible, indicating that associated with the hydrogen there is some compound or com-

[1] Graham, "Chemical and Physical Researches," p. 283.
[2] *Chemical News*, June 21, 1872.
[3] Flight, *op. cit* p. 80.
[4] Flight, *Geol. Mag.*, January 1875.

pounds of carbon in the meteorite which require a higher temperature to bring them out, but which come out when that higher temperature is employed. The carbonaceous structure of some meteorites has already been determined on other grounds.

If we carry the heating a little further still, and instead of leaving the particles relatively cold and dark while the current is passing we apply a higher temperature outside the tube by means of the Bunsen burner, then we get the luminous vapours of some constituents of the meteorite added to the spectra of hydrogen and carbon.

What luminous vapours do we get first, and which last? The experiment is a very interesting one, and may certainly be carried on in a tube such as that described until a pretty considerable development of the spectrum is obtained. The first substance which makes itself visible obviously after the hydrogen and carbon when particles of a meteorite are treated in this way is magnesium derived from the olivine, that substance which exists in the greatest quantity in the stones, and in the schreibersite, which exists in the irons.

From such a method of research as this we can pass to one in which, by means of the oxy-coal-gas flame, we can determine the spectrum of any vapour given off, provided any vapour is given off, at a still higher temperature. That work has been done, and the main result is that in the case of an "iron," the first substance to make its appearance is manganese, and the next substance to make itself obvious is iron.

Here a very important remark must be made. The substance which will give us the predominant spectrum at lowest temperature must be that substance the volatility of which at that temperature is greatest. If, however complicated the chemical constitution of one of these meteorites may be, there is one substance which volatilizes out of it more readily than another at a low temperature, that substance will be the first to give us its characteristic spectrum at that temperature—and in fact we may get the spectrum of that substance alone, although its percentage in the meteorite may be extremely small. It is therefore an important result to find that in meteorites in which the quantity of iron is very considerable it is always the manganese that makes itself visible first, because its volatility is greater than that of iron. The point to bear in mind is that when we pass to the temperature of the oxy-coal-gas flame we get predominant evidence of the existence of manganese, and afterwards of iron.

Many diagrams of observations made in this way have been constructed of the oxy-coal-gas flame of meteorites and of olivine, and not only the flame but the "glow,"—glow being the name given to the luminosity produced in the tube under the conditions stated. There are some points of similarity, and other points of difference. One of the results which is most constant is a line at 500 on the wave-length scale which appears to run through all the observations until we come to deal with such meteorites as the Limerick and Nejed. On the other hand some lines and flutings do not make their appearance generally.

If we wish to extend our inquiry into the function of a still higher temperature we can use the electric arc; that also has been done. For this purpose specimens of iron meteorites have been cut into poles, the spectra of which have been observed and photographed, so that the vapours produced have been the vapours of the pure iron meteorites; that is to say, a small portion of a meteorite has *not* been placed in an impure carbon pole, so that the impurities of the carbon would be observed and photographed with the pure vapours of the meteorites. In addition to this method—in the case of the stony meteorites—the lower pole after its spectrum has been well studied has been utilized in this way: the upper pole remaining constant as an iron pole, pretty big particles of various stony meteorites have been inserted into the lower pole, and the added result has been recorded. Further, composite photographs of the spectra of many meteorites have been obtained. Half a dozen different stony meteorites have been rendered incandescent by their insertion into the lower pole during the exposure of a single photographic plate.

It is pretty obvious that if we can get detailed information on such points as these, and provided there are meteorites in space at the temperatures at which we are able to determine their spectra in the laboratory, such data should be of extreme value, for at present we know of no reason why the spectra should differ according to locality.

J. NORMAN LOCKYER.

(*To be continued.*)

MOLECULAR PHYSICS: AN ATTEMPT AT A COMPREHENSIVE DYNAMICAL TREATMENT OF PHYSICAL AND CHEMICAL FORCES.[1]

II.

§ 6. *Double Refraction.*

ACCORDING to the theories both of Fresnel and of Neumann, double refraction is explained on the assumption that the elasticity of the ether in crystals which exhibit this phenomenon is different in different directions. The elasticity is proportional to the square of the velocity of propagation, and if a, b, c are the ratios of the elasticities, parallel to the principal axes of the crystal, of the ether within it to its density, the velocity in any direction α, β, γ will be given by the equation—

$$v^2 = a^2 \cos^2 \alpha + b^2 \cos^2 \beta + c^2 \cos^2 \gamma \quad . \quad . \quad . \quad . \quad (18)$$

According to the author's theory, the elasticity of the ether is the same in every direction, so that any difference in the velocities of propagation in different directions must be due to the mutual action between the ether and the molecules of the crystal being a function of the direction, and therefore the values of the quantities c_i for the molecules of the crystal, and hence also the value of μ, must depend on the direction.

Assuming, for simplicity, that the molecules have a single shell only, it follows from (8) and (9) that—

$$\mu^2 = \frac{1}{v^2} = \frac{\rho}{l} - \frac{c_1 T^2}{l} \left\{ 1 + \frac{c_1}{\frac{m_1}{T^2} - c_1 - c_2} \right\}$$

$$= \frac{1}{l} \left\{ \rho - c_1 T^2 \left[1 + c_1 \frac{T^2}{m_1} \frac{\kappa_1^2 R_1}{\kappa_1^2 - T^2} \right] \right\} \quad . \quad . \quad . \quad (19)$$

where $\kappa_1^2 = m_1/(c_1 + c_2)$ and $R_1 = m_1/\kappa_1^2(c_1 + c_2)$.

Let the values of c_i and μ for a second direction be c_i^1 and μ^1, then

$$\mu^{1^2} = \frac{\rho}{l} - \frac{c_1^1 T^2}{l} - \frac{c_1^{1^2} T^4}{(c_1^1 + c_2^1)\left(\frac{m_1}{c_1^1 + c_2^1} - T^2\right)} \quad . \quad . \quad (20)$$

Now, as Thomson has pointed out, the dispersion accompanying double refraction is of very small amount, so that the difference $\mu^2 - \mu^{1^2}$ must be sensibly independent of T.

If T were less than κ, $\mu^2 - \mu^{1^2}$ would, from (12), be proportional to T^2. It must therefore be assumed that the critical period is at the extreme blue end of the spectrum, which will give T greater than κ_1 for all the rays. Then from (12*a*)—

$$\mu^2 - \mu^{1^2} = \frac{c_1^2 m_1}{l(c_1 + c_2)^2} - \frac{c_1^{1^2} m_1}{l(c_1^1 + c_2^1)^2}$$

$$- \left(c_1 - c_1^1 - \frac{c_1^2}{c_1 + c_2} + \frac{c_1^{1^2}}{c_1^1 + c_2^1} \right) \frac{T^2}{l}$$

$$+ \frac{c_1^2}{(c_1 + c_2)^3} - \frac{c_1^{1^2}}{(c_1^1 + c_2^1)^3} \frac{m_1^2}{l T^2} + \quad . \quad . \quad . \quad . \quad (21)$$

In order that the coefficient of T^2 may be small, c_1 and c_1^1 must be small and nearly equal. The other terms of the series will then be also very small, especially if T is large in comparison with m_1, and the series may, to a first approximation, be replaced by its constant term.

Now let it be assumed that the manner in which c_1 and c_2 depend on the direction α, β, γ, is determined by an equation of the form—

$$\left(\frac{c_1}{c_1 + c_2}\right)^2 = C_1 \cos^2 \alpha + C_2 \cos^2 \beta + C_3 \cos^2 \gamma \quad . \quad . \quad (22)$$

Then from (19) and (12*a*)—

$$v^2 = \frac{1}{\mu^2} = \left(\frac{l}{\rho} - \frac{m_1}{\rho} C_1\right) \cos^2 \alpha + \left(\frac{l}{\rho} - \frac{m_1}{\rho} C_2\right) \cos^2 \beta$$

$$+ \left(\frac{l}{\rho} - \frac{m_1}{\rho} C_3\right) \cos^2 \gamma,$$

an equation of the same form as (18), and which therefore gives a wave-surface identical with Fresnel's. It must, of course, be

[1] A Paper read before the Physico-Economic Society of Königsberg, by Prof. F. Lindemann, on April 5, 1888. Continued from p. 407.

assumed that the axes of the molecules in the crystal are all parallel.

Thomson arrived at a different result, which the author attributes to his having assumed the product of the denominators $c_1 + c_2 - m_1/T^2$ and $c_1{}^1 + c_2{}^1 - \frac{m_1}{T^2}$ to be sensibly a constant, and therefore considered only the manner in which T enters into the numerators.

It is easy to see that similar results will be obtained for molecules consisting of any number of shells.

§ 7. *Spectra of Chemical Compounds.*

In considering chemical compounds it is necessary to make a clear distinction between atoms and molecules, and henceforward the author uses the term atom to denote a system of shells such as is described in § 1, and employs the term molecule only for a combination of two or more atoms having their external shells close together. The author restricts his investigations to diatomic molecules.

A molecule will then be capable of executing stationary vibrations without disturbing the ether, similar to those of an atom, and will therefore also have its critical periods; but their values, in the case of the molecule, will depend on the direction of the disturbance. A diatomic molecule may be considered approximately as consisting of a series of concentric prolate spheroidal shells having their longer axes coincident with the lines joining the centres of the spheres.

There will be two principal series of critical periods, corresponding respectively to disturbances propagated in the direction of the longest axis or of any of the shortest axes. If the direction of propagation of a disturbance differs slightly from one of these axes, the corresponding lines of the spectrum will only be slightly displaced, and in this way well-defined bright lines will be replaced by bright bands sharply defined on one side and indistinctly on the other. If two of these bands overlap on their indistinct sides, a band may be produced of equal brightness throughout, and having both its sides sharply defined.

This gives an explanation of the well-known experimental fact that the spectra of chemical compounds usually consist of bright fluted bands, sometimes accompanied by distinct bright lines, and not of bright lines only. Conversely, if the spectrum of a gas contains bright bands, it will be natural to infer that it is a chemical compound. This would lead us to suppose that oxygen, sulphur, nitrogen, phosphorus, carbon, and silicon are really compound bodies—a conclusion which receives independent confirmation from other points of view.

The theory does not lead to any simple law, such as has often been sought after, for determining the spectrum of a compound from the spectra of its constituents, but it throws a good deal of light on the subject generally.

The differential equations to determine the motions of the shells within an atom differ from equations (1) only in virtue of the core itself being supposed to be in motion, so that the last of these equations will become—

$$\frac{m_j}{4\pi^2}\frac{d^2x_j}{dt^2} = c_j(x_{j-1} - x_j) - c_{j+1}(x_j - x_{j+1}) \quad . \quad . \quad (23)$$

the difference consisting only in the presence of x_{j+1}, which was supposed equal to zero in equations (1).

If we discard the assumption that the mass of the core is so great relatively to that of the shells in an atom that the centre of gravity of the system may be identified with that of the core, the condition $x_{j+1} = 0$ will be replaced by the more general one—

$$m_1x_1 + m_2x_2 + \ . \ . \ . \ + m_{j+1}x_{j+1} = 0 \quad . \quad . \quad . \quad (24)$$

which determines the value of d^2x_{j+1}/dt^2, which is wanting in the system (23).

From (2), (3), and (23) we obtain the system—

$$\begin{aligned} -c_1\xi &= a_1x_1 + c_2x_2 \\ -c_2x_1 &= a_2x_2 + c_3x_3 \ . \ . \ . \ . \ . \ . \ . \ . \quad (25) \\ &. \ . \ . \ . \ . \ . \ . \ . \ . \\ c_jv_{j-1} &= a_jx_j + c_{j+1}x_{j+1}\ ; \end{aligned}$$

where, as before, $a_i = m_i/T^2 - c_i - c_{i+1}$.

These, together with (24), form a set of $j + 1$ linear equations, which are sufficient to determine the $j + 1$ unknown quantities $x_1, x_2, \ . \ . \ . \ x_{j+1}$ in terms of the given quantities ξ and T^2.

Replacing ξ, m, x, c, j by η, n, y, e, κ respectively, we obtain a similar set of equations to determine the vibrations of the second atom. If the outer shells of these two atoms are in contact, x_1 must be equal to y_1, unless the disturbance is such as to effect a separation, x_i and y_i being corresponding displacements from the common centre of gravity. Writing x for the common displacement of the shells in contact, equations (25) assume the form—

$$\begin{aligned} -e_1\eta &= b_1x + e_2y_2 \\ -e_2x &= b_2y_2 + e_3y_3 \\ &. \ . \ . \ . \ . \ . \ . \ . \\ -e_\kappa y_{\kappa-1} &= b_\kappa y_\kappa + e_{\kappa+1}y_{\kappa-1}. \end{aligned}$$

The condition that the common centre of gravity of the two atoms may remain at rest will therefore be—

$$\begin{aligned} (m_1 + n_1)x + m_2x_2 + m_3x_3 + \ . \ . \ . \ + m_{j+1}x_{j+1} \\ + n_2y_2 + \ . \ . \ . \ + n_{\kappa+1}y_{\kappa+1} = 0 \ . \ . \ . \ . \ (27) \end{aligned}$$

(25), (26), and (27) form a system of $j + \kappa + 1$ equations to determine the same number of unknowns, x, x_2, . . . x_{j+1}, η_2 . . . $\eta_{\kappa+1}$, in terms of the known quantities ξ, η, and T^2. ξ is determined as before by equation (2), and gives the vibration of the ether at the point where the ray impinges on the first atom. The axis of a molecule may be at any angle with the impinging ray, and η will give the ether vibration at the point where a ray parallel to the first strikes the second atom. For a given period and wave-length, ξ and η will therefore in general be in different phases.

In the case of vibrations parallel to the axis of the molecule we shall have $\xi = \eta$, supposing all the parallel rays impinging on the molecule to be in the same phase. The ratio x/ξ, required for the determination of μ^2 will then be the quotient of the second and first minors (viz. the coefficients of u_1 and u) of the determinant of order $j + \kappa + 2$ given below, in which the first row is completed by arbitrary quantities.

$$\begin{array}{ccccccccccccc} u & u_1 & u_2 & u_3 & u_4 & \ldots & u_{j+1} & v_2 & v_3 & v_4 & \ldots & v_{\kappa+1} \\ 0 & m_1 + n_1 & m_2 & m_3 & m_4 & \ldots & m_{j+1} & n_2 & n_3 & n_4 & \ldots & n_{\kappa+1} \\ c_1 & a_1 & c_2 & 0 & 0 & \ldots & 0 & 0 & 0 & 0 & \ldots & 0 \\ 0 & c_2 & a_2 & c_3 & 0 & \ldots & 0 & 0 & 0 & 0 & \ldots & 0 \\ 0 & 0 & c_3 & a_3 & c_4 & \ldots & 0 & 0 & 0 & 0 & \ldots & 0 \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ 0 & 0 & 0 & 0 & 0 & \ldots & c_{j+1} & 0 & 0 & 0 & \ldots & 0 \\ e_1 & b_1 & 0 & 0 & 0 & \ldots & 0 & e_2 & 0 & 0 & \ldots & 0 \\ 0 & e_2 & 0 & 0 & 0 & \ldots & 0 & b_2 & e_3 & 0 & \ldots & 0 \\ 0 & 0 & 0 & 0 & 0 & \ldots & 0 & e_3 & b_3 & e_4 & \ldots & 0 \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ 0 & 0 & 0 & 0 & 0 & \ldots & 0 & 0 & 0 & 0 & \ldots & e_{\kappa+1} \end{array}$$

This will always be the case applicable to the determination of the light emitted by a molecule.

The equation $\xi = 0$, which determines the critical periods of the molecule, will then be obtained by equating the coefficient of u to zero, and as a_i and b_i are linear functions of T^{-2}, the resulting equation will be of the order $j + \kappa$. Therefore, for vibrations parallel to the axis, the number of critical periods of a diatomic molecule is equal to the sum of the numbers of critical periods of its constituent atoms. This number may be diminished if $x = 0$ while x/ξ and u_2 remain finite.

If a single ray only is considered, as at the limits of illumination, η may be taken equal to zero for any given value of ξ; it is only necessary to put $e_1 = 0$ in the first column of the determinant. This will, however, not affect the equation $\xi = 0$.

If the impinging ray is parallel to the axis of the molecule, in which case the vibrations will be perpendicular to it, the two atoms will be differently affected by the vibrations of the ether, for, in the case of the first atom, ξ is again determined by (2), or more generally by the equation—

$$\xi = a\cos\left(\frac{2\pi t}{T} - \frac{X}{\lambda}\right),$$

where X is the abscissa of the atom; and if r and s are the radii of the two atoms we shall have for the second atom—

$$\eta = a\cos\left(\frac{2\pi t}{T} - \frac{X + r + s}{\lambda}\right).$$

Now the radii of the atoms are supposed to be very small comparison with the wave-length λ, so that ξ and η will be nearly equal, and therefore we may write—

$$\eta = \xi \left\{ 1 + \frac{r + s}{\lambda} \tan\left(\frac{2\pi t}{T} - \frac{X}{\lambda}\right) \right\}.$$

As a first approximation we may take $\xi = \eta$, and then the vibrations will be the same as those parallel to the axis. Since, however, the centre of gravity remains fixed, the vibration must be a pendulous one about this centre, which introduces a fresh set of considerations. The proper vibrations of the molecule would still be given by $\xi = 0$ and $\eta = 0$, but, owing to the pendulous vibration, these would not completely determine the motion. The difference in the action of light in different directions, and the corresponding fluted nature of the spectrum, would appear to depend essentially on considerations of this kind.[1]

In the case of a triatomic molecule, we obtain three sets of linear equations of the same form as (25) and (26), together with one of the form (27); it is, however, unnecessary to pursue this further.

§ 8. *Production of Chemical Compounds by the Effect of Light and Heat.*

When an atom of any gas strikes in its course against an atom of some other gas, the question which presents itself is whether the two will unite to form a single molecule or not. The internal equilibrium of each atom will be disturbed by the impact, so that the resultant of the internal forces of the system formed by the two atoms will in general have a value different from zero. Let this resultant be transferred parallel to itself until it passes through the centre of gravity, as is allowable from a theorem of dynamics, then it will increase its velocity of translation. The total energy of the system must, however, remain constant, so that the energy of the internal atomic vibrations must be diminished by exactly the same amount as that by which the energy of the motion of the centre of gravity is increased. After the impact the internal vibrations will at first be of a very irregular character; but under the action of the light rays they will ultimately attain a condition of stationary equilibrium, supposing such to be possible with the diminished energy. When it is possible its stability will be greater, the greater the diminution in the internal energy.

Consider, for example, the formation of hydric chloride gas by the action of light on a mixture of chlorine and hydrogen, accompanied as it is by a measurable development of heat. Both these gases exhibit strong bright lines in the blue portion of the spectrum, and, in the case of hydrogen, also in the ultra-violet. Vibrations of corresponding critical periods will therefore easily be excited, which will greatly increase the internal energy of the atoms. When an atom of chlorine now impinges upon one of hydrogen, they will remain in contact for a finite, though exceedingly short interval. During this interval the mechanical theorem relative to the motion about the centre of gravity is applicable, since there will be no external forces acting on the pair of atoms during their common rectilinear motion. Let it be assumed further that the energy of the molecule formed by the union of the two atoms is, under the existing conditions, less than the sum of their separate energies, viz. that the critical vibrations of the molecule are less sensitive to the action of light than those of the separate atoms, then the spherical atomic shells will tend to execute resultant vibrations proper to the molecule according to § 7, so that the chlorine and hydrogen will unite to form hydric chloride. No energy can of course be lost, so that the difference between the internal energy of the molecules and that of the separate atoms will be added to that of the translatory motion, and will therefore become sensible in the form of heat.

It will be noted that no special chemical affinity between chlorine and hydrogen has to be assumed, but two elements may be said to have a chemical affinity whenever the energy of the resultant molecular vibration is, under the given conditions, less than that of the separate atomic vibrations.[2]

[1] Bunsen's observations (*Poggendorff's Annalen*, vol. cxxviii.) on crystals of certain didymium salts show that there is actually a difference in the absorption of light in different directions.

[2] A chemical compound may therefore be regarded as produced in a manner similar to the variation of a species on the Darwinian theories of adaptation and natural selection. A species undergoes variation such as to increase its suitability to its environment. In exactly the same way two atoms will unite to form a molecule, when they thereby become less sensitive to the influence of their surroundings than they would be separately. Accidental conditions are of no more importance in determining the formation of chemical compounds, than the voluntary actions of individuals in determining the variation of a species.

The given conditions may depend on light, heat, or electromotive force, though the consideration of the last-named may be eliminated (see § 16). An example of the action of heat is given by the formation of water from hydrogen and oxygen. The hydrogen burns with a blue flame. Both the elements give bright lines in the red portion of the spectrum, hydrogen at 6562, and oxygen at 6171,[1] so that their internal energy can easily be increased by the action of heat, so that combination will take place, and this is accompanied by a considerable development of heat. Water being a very stable compound with respect to the action of heat, we should expect it to give chiefly blue lines. This has not hitherto been proved by direct experiment, but it appears to be indicated by the blue colour and intense heat of the hydrogen flame.

Since the heat of combustion which is usually developed during the formation of oxides arises from a diminution in the internal energy of the atoms, we should infer that (1) the stability of an oxide will be greater the greater its heat of combustion; (2) the spectrum of the oxide will not extend so far towards the red end of the spectrum as the spectra of the constituents.

The former inference is confirmed by the researches of Favre and Silbermann; the latter is found to be justified for the oxides of aluminium, lead, carbon, copper, and strontium (the ultra-red portion of the spectrum in the case of strontium should be specially noted), but it cannot be expected to hold good so universally as the former.

§ 9 *Molecular Theory of Chemistry.*

In modern chemistry the term molecule is used to denote the smallest mass of a substance which can exist separately. This conception of a molecule is essentially different from that set forth in § 7 of this paper. The chemical molecule may be simply an atom, as in the cases of mercury and cadmium, but this is not the case for the molecules considered by the author. On the author's theory, each atom is supposed capable of separate existence, which agrees with chemical phenomena when the atoms are considered in the isolated, or so-called nascent condition, but appears to be in conflict with them in that Mariotte's (Boyle's) law, and the comparison of the weights of equal volumes of various elements in the gaseous state, appear to point to the conclusion that their chemical molecules consist of two or more atoms.

This only applies to elements in the gaseous state and under the ordinary conditions of pressure and temperature, and it is quite conceivable that in high vacua and at a high temperature, as for example in a Geissler tube, the atoms of diatomic molecules may exist separately, a dissociation taking place similar to that which is invariably found to occur in the case of chemical compounds under similar circumstances (see § 10). The ordinary hypothesis must therefore be regarded as simply expressing that under ordinary circumstances the atoms of diatomic molecules tend to unite in pairs to form chemical molecules.

According to § 8, it must therefore be assumed that the diatomic molecules of certain elements are less sensitive to the external influences of light and heat than the separate atoms, and that the internal energy of such a molecule is less than the sum of the internal energies of its two constituent atoms. Suppose that ξ is again determined by (2) and that $x_i = \alpha_i \cos 2\pi t/T$, then the quantities α_i must be determined from the equations (25) and (24). The internal energy of an atom will therefore be

$$E = \tfrac{1}{2}(m_1\alpha_1^2 + m_2\alpha_2^2 + \ldots + m_{j+1}\alpha_{j+1}^2).$$

The energy of a second atom of the same substance under identical external conditions will have the same value. If the two atoms are placed in contact, the new values of x_i must be determined from (25), (26), and (27). In this case, however, we have $y_i = x_i$, $a_i = b_i$, $c_i = e_i$, $m_i = n_i$, so that (26) and (27) become identical, and (27) reduces to (24), with the distinction, however, that the quantities x_i now represent the displacements relatively to the common centre of gravity, instead of relatively to the centre of gravity of the single atom. It therefore follows that, approximately, the critical vibration periods of a molecule consisting of two similar atoms will be identical with those of the separate atoms.

Now the energy of the molecule is just double that of either of the constituent atoms, so that the union of the atoms cannot be due to a decrease in the internal energy. It is easy to understand, however, that when two atoms have once combined they

[1] See B.A. Reports, 1884, 1885, and 1886.

will not separate again, except under special circumstances; but so far the fact that different gases behave differently in this respect remains unexplained. If two spherical bodies collide, they will remain in contact only if perfectly inelastic, otherwise they will fly off in opposite directions.

In the latter case the elastic forces are due to the displacement of the molecules of the spheres from their positions of equilibrium. If the colliding bodies are two of Thomson's atoms, similar elastic forces will be called into play by a displacement of their outer shells. If the mass m_1 of each of the outer shells is very large compared with that of the inner ones, the outer shells will remain nearly at rest after the collision, while the inner ones will be thrown into violent vibration; indeed it follows from (24) that x_1 will be very small. The atoms will therefore behave very nearly as if they were inelastic, and may remain long enough in contact to assume a new condition of equilibrium by uniting to form a single molecule. Exactly the reverse will happen if m_1 is small compared with the mass of the inner shells.

We must therefore assume that in diatomic chemical molecules the masses of the outer shells are very large compared to the sums of the masses of the interior shells, while in the monatomic molecules the masses of the outer shells are comparatively small.

We might now inquire why it is that in general more than two atoms do not unite in this manner. To which the answer is that the more complicated the structure of a molecule, the more easily will it be broken up by the impacts of other molecules. We must therefore assume that in the case of diatomic molecules the violence and frequency of the impacts, even under ordinary circumstances, are sufficient to break up any molecules which may be formed containing more than two atoms; while in the case of other elements, such as arsenic and phosphorus, the impacts are unable to break up the tetratomic molecules, even at the high temperature of vaporization.

In virtue of these considerations it appears that the formation of a chemical compound, such as hydric chloride, is not such a simple process as it was supposed to be in § 7. The impacts will frequently produce diatomic molecules of hydrogen and of chlorine respectively. The final condition of equilibrium will, however, be arrived at on the same principle as before—namely, that the molecules of hydric chloride are the least sensitive to the action of light. Tetratomic molecules of hydric chloride, will not be permanently formed, as the impacts, increased in violence and frequency by the heat developed, will break them up. Similar considerations apply to the formation of water.

The formation of these simple compounds is, therefore, accompanied by, and due to the simultaneous breaking up of the original diatomic molecules of the elements present.

Double decompositions will take place in an exactly similar manner, and considerations of the same kind apply to solid and liquid bodies, in which, however, the impacts will be very much less frequent.

We also see that the broadening of the bands in the spectrum of a gas, especially when due to a lowering of temperature, does not necessarily show that the gas is a compound, as it may be due to the union of previously dissociated similar atoms into molecules.

§ 10. *Dissociative Action of Light and Heat.*

The fact that the same compounds which are formed by the action of heat are again broken up when the temperature is further increased, and, indeed, the dissociation of every chemical compound at a sufficiently high temperature, is in apparent contradiction to the conclusions of § 8. In the case of compounds formed by the action of light it is quite possible that the internal energy due to the action of heat may be greater than that of the atoms at the same temperature. In general, it may be that when the two constants c_i (§ 1) combine to form one, the corresponding critical vibrations are only produced at a much higher temperature, and may then give rise to dissociation. Since, however, all compounds are dissociated at sufficiently high temperatures, there must be some other causes at work. We may suppose that in gases at very high temperatures the molecules are broken up simply by the violence of the impacts, and this process would be facilitated by the molecules not being spherical in form.

The dissociative action of light observed in certain cases cannot of course have a similar general explanation, and must not be attributed to special chemical properties of light of certain wave-lengths, but to the values of the internal constants of the molecules being of a kind specially favourable to such action. Thus, as the author points out, we are led to the point of view expressed by Lockyer,[1] as follows:—

"The causes which are given in the text-books, showing us the maxima of heat, light, and chemical action, are, I fancy, merely causes showing us, as it were, the absorption spectra of those substances by which the maxima have been determined—whether they be lamp-black, the coating of the retina, or salts of silver, and are really altogether independent of the nature of light."

§ 11. *Fluorescence.*

It has been pointed out in § 4 how critical vibrations may be excited in a molecule by external disturbances, causing the molecule to emit light of a certain wave-length. The disturbance was supposed to be due to the action of heat, but from what has gone before it is clear that they may be produced by ether vibrations if only the molecule or atom is very sensitive to light vibrations. For as soon as the impact of light waves of a certain (critical) vibration period has raised the internal energy of the molecule to its maximum value, the molecule itself—that is to say, its centre of gravity—will begin to execute vibrations; the different molecules will strike against one another, and the result of these encounters will be to produce vibrations of the other critical periods of the molecule, which will be different from the vibration period of the impinging light.

The substance will therefore emit rays different from those which have fallen upon it. As a matter of fact some substances having such special sensitiveness have been observed,[2] and are known as fluorescent substances. The phenomena of fluorescence must therefore be attributed to the absorption of light, as was pointed out by Stokes.

A fluorescent body is to be regarded as one in which the molecular constants c_i have such values that the corresponding light vibrations can be easily excited by external impulses. Fluorescent substances must, in agreement with Stokes's conclusions, be regarded as being exceptionally sensitive.

The theory does not lead to the law which has usually been asserted, that the emitted light must necessarily be of longer wave-length than the impinging light, and therefore the theory is not inconsistent with Lömmel's observations on naphthalin red.

Fluor-spar exhibits the phenomena of fluorescence to an exceptional degree. It may be that fluorine itself is exceptionally sensitive to the action of light, and that the formation of the mineral has not altogether destroyed this sensitiveness. If this be so, it would explain the impossibility of preventing fluorine from entering into combination with any substance with which it is in contact.

G. W. DE TUNZELMANN.

(*To be continued.*)

THE FORESTRY SCHOOL IN SPAIN.

In a Report to the Foreign Office which has just been published the British Ambassador at Madrid states that Mr. Gosling, First Secretary to the Embassy, has had the opportunity of studying the excellent School of Forestry established at the Escurial, and as great interest is now taken in forestal science in England, and as efforts are being made to establish a British National School of Forestry, he sends the information collected by Mr. Gosling at an institution which, he thinks, is well adapted as a type for a similar institution in England.

Forestal legislation in Spain dates as far back as the close of the fifteenth century—that is, in the reign of Ferdinand and Isabella—and there is reason to believe that reckless destruction of the rich forests was checked from time to time by Royal ordinances. At the close of the sixteenth century Madrid was surrounded by dense forests; in fact, the city arms—a bear climbing up a green tree—bear out the old chroniclers when they speak of the forests which lay around the city, which must have existed in the time of Charles V. So far is this from being the case at present that for the most part the districts around Madrid are treeless and unproductive, and as a consequence exposed to the furious mountain storms, and unsheltered in the scorching summer, whence comes the extreme unhealthi-

[1] "Studies in Spectrum Analysis," p. 110.

[2] Thomson mentioned, "Lectures on Molecular Dynamics," p. 280, that his theory of absorption would account for the phenomena of fluorescence, but he did not follow up the subject.

ness for any person with a delicate constitution. While Spanish rule in South America carefully protected the forests from destruction, it permitted this to go on almost unchecked at home. Towards the end of the last century the great agrarian lawyer and reformer, Jovellanos, who was the first to call the attention of Spain to the disastrous effects which were being produced by the want of supervision of the forests, wrote a pamphlet entitled "Informe de la Sociedad economica de Madrid, al real y supremo Consejo de Castilla, en el expediente de ley agraria extendido por su individuo de numero Don Melchor Gaspar de Jovellanos à nombre de la Santa encargada de su formacion, y con arreglo à sus opiniones." This pamphlet paved the way for the present excellent system of forestry. Special ordinances were passed in the year 1835 for the foundation of a school of forest engineers, but at the time no practical steps were taken; but ten years later, when domestic troubles had to some extent passed away, the "Escuela especial de Ingenieros de Montes" (School of Forestry) was firmly established and was followed by the formation of a corps of forest engineers. The first School of Forestry was situated at Villaviciosa, not far from Madrid, and was under the control of Señor Bernardo de la Torre Rojas, who is still styled "el padre de la Escuela Española de Montes." In 1869 the school was transferred from Villaviciosa to the Escurial, part of which had been granted by the Government in the preceding year for that purpose. This institution is now under the direction of Señor Bragat y Viñals, and there are nine professors and three assistants under him, all of whom must have served five years on the staff of forest engineers. The annual salaries of these officers amount to £1400, and appear in the annual Budget of the Minister of "Fomento," which Department includes public works, industry and commerce, agriculture, public instruction. The total yearly cost of the school is £1700. The following are the subjects taught by the professors, each group having a professor: (1) forestal legislation; (2) political economy, forestal meteorology; (3) applied mechanics and forestal construction; (4) topography and geodesy; (5) chemistry, mineralogy, and geology (applied); (6) botany; (7) sylviculture, (8) zoology and forestal industries; (9) classification of forests and their valuation. The instruction is free, but the books and instruments are charged for. The vacation depends on circumstances. If the practical work is completed, the months of August and September are given; four days in December and three during the Carnival are given—that is, in all about nine weeks. The number of students is practically unlimited. The school is open to all who pass the preliminary examination—that is, to all who show proficiency in Spanish and Latin grammar, geography, and Spanish history, elements of natural history, of theoretical mechanics, geometry, and its relations to projections and perspective, physics, chemistry, lineal, topographical, and landscape drawing, and an elementary knowledge of French and German. Immediately on entrance to the school, particular attention is paid to topography, chemistry (practical), and mathematics (applied). The topography course includes the object of topography, and the difference between it and geodesy; the rules of triangulation and methods of demonstrating the physical characteristics of the ground under survey; chart and plan drawing; and an intimate knowledge of the use of the instruments used in forestal topography. The course in chemistry is very wide, including every detail of the applied science appertaining to botany, mineralogy, and sylviculture. In the school is a very fine collection of chemical apparatus and instruments, including those of Bunsen, Dupasquier, Gay-Lussac, Donovan, &c. Every kind of instrument required in applied mechanics is also here. There is a very good library of books attached to the school, comprising about 3000 volumes on mathematics and the physical sciences, natural history, language, literature, and history, arts and manufactures, &c. During the first year the studies are topography, differential and integral calculus, descriptive geometry, applied mathematics, and chemistry. In the second year the subjects are mechanics, geodesy, meteorology, climatology, construction, and drawing; in the third year, mineralogy and applied zoology, applied geology, botany, and sylviculture; in the fourth year, kilometry, scientific classification of forests, forest industries, law, and political economy. On the completion of this four years' course, the successful candidates are appointed to the staff of forest engineers. This corps consists of 3 general inspectors, 15 district inspectors, 40 chief engineers of the first class, 50 chief engineers of the second class, 60 second engineers of the first class, and 70 of the second class. There are also 25 assistants of the first class, 350 of the second class, and 420 foremen planters. The salaries of the six grades of engineers are respectively £500, £400, £300, £260, £200, £160, besides an active service allowance of £1 a day to inspectors, 16*s.* a day to chief engineers, and 12*s.* a day to the others. The country is divided into 46 forestal departments, the forest in each case being under the care of a chief engineer, but the inspecting officers reside in Madrid.

SCIENTIFIC SERIALS.

American Journal of Science, August.—History of the changes in the Mount Loa craters; Part 2, on Mokuaweoweo, or the summit crater (continued), by James D. Dana. The subjects here considered are (1) the times and time-intervals of eruptions and of summit illuminations or activity, with reference to periodicity, relations to seasons, variations in activity since 1843, and lastly the changes in the depth of the crater; (2) the ordinary activity within the summit crater; (3) causes of the ordinary movements within the crater. Among the general conclusions are the rejection of any law of periodicity, and the apparently established fact that the inland waters supplied by precipitation are the chief source of the vapours concerned in Hawaiian volcanic action. Then follows Part 3, dealing with the characteristics and causes of eruptions; metamorphism under volcanic action; the form of Mount Loa as a result of its eruptions; the relations of Kilauea to Mount Loa; lastly, general volcanic phenomena.—The Fayette County (Texas) meteorite, by J. E. Whitfield and G. P. Merrill. The specimen was found about ten years ago on the Colorado River near La Grange, Fayette County. It weighs about 146 kilogrammes, and analysis shows that the rocky portion consists essentially of olivine and enstatite with some pyrrhotite. It belongs to the class to which G. Rose has given the name of "chondrites," and its most striking feature is its fine and compact texture, exceeding that of any similar meteorite known to the authors.—Evidence of the fossil plants as to the age of the Potomac formation, by Lester F. Ward. From these researches it appears that no Jurassic species, but many strongly Jurassic types, occur. The Wealden furnishes the largest number of identical species, after which follow the Cenomanian and Urgonian. All these formations also yield many allied species, which, however, are most abundant in the Oolitic. Altogether the flora would appear to be decidedly Cretaceous, but probably not higher than the Wealden and Neocomian.—E. H. Hall describes some experiments carried on for over three years at Harvard College on the effect of magnetic force on the equipotential lines of an electric current; and Thomas M. Chathard gives the analyses of the waters of some Californian and other North American alkali lakes.

Mémoires de la Société d'Anthropologie, tome troisième (Paris, 1888).—This volume contains an exhaustive treatise by Dr. Nicolas on automatism in voluntary acts and movements. The author, who is an ardent opponent of the materialistic and atheistic views common to many of his scientific brethren, is especially anxious to call attention to questions such as those of which he here treats, and which have hitherto been little considered in France. The main conclusion that he draws from the accumulated mass of facts, which he has borrowed principally from the labours of British and German biologists, is that the superiority of an animal in the scale of being is determined by the degree of liberty which it enjoys in controlling reflex actions, and directing automatic reactions.—Contribution to the study of anomalies of the muscles, by M. Ledouble. The principal subjects here treated of are the variations in the iliac, costal, and spinal processes of the latissimus dorsi muscle.—Philosophy, considered from an anthropological point of view, by Dr. Fauvelle. Although the writer passes in review the various schools of philosophy which have sprung up in various periods of time, his purpose is rather to follow the gradual evolution of philosophic thought from the first appearance of man, than to recount its history. Pointing out that comparative anatomy and physiology teach us that intelligence depends directly on the number and degree of differentiation of the cerebral cellules, he asks whether we must assume that these have reached their utmost limits of development, or whether new manifestations of cerebral perfection may not be reserved for man? According to his views, religions of all forms, and speculative philosophy, have equally had the effect of impeding every kind of independent intellectual labour, and have thus in different parts of the universe and in different ages applied successive checks to cerebral evolution, which Dr. Fauvelle regards as identical with human progress.—On the hand and figure of native East Indians, by Dr. Mugnier. In this exhaustive article the author gives elaborate measurements based on his own observations of the maxima and minima and the means of every part of the hand specially, and of the body generally, in the six principal Asiatic races, with tables of comparative measurements of Europeans. From these it is seen that the absolute size of the hand among Asiatics is less than in Europeans, the Japanese approximating most closely to the estimates given for the latter, while the Malays exhibit the lowest maximum. In regard to stature, and relative proportions of figure, all Asiatics are inferior to Europeans, the Japanese presenting the greatest divergence, while the Arabs of Yemen approximate most nearly to the general means of European races.—An anthropological and ethnographic study of the kingdom of Cambodia, by Dr. E. Maurel. Shaded maps of the territorial divisions of Indo-China from the seventh century to the present time curiously illustrate the varying supremacy of Siamese, Laos, and Cambodian tribes in that portion of the Far East which lies between the China Sea and the Indian Ocean. The rapidity with which alluvial deposits are formed would seem to justify the author's assertion that the territories now known as Cochin-China and South Cambodia are of recent geological origin, and were possibly submerged till near the dawn of actual historical ages. Interesting information is supplied as to the effect on the land, and the habits and pursuits of the people, of the regular inundations to which the country is exposed by the overflow of the Mekong, the great river which, rising in East Tibet, flows southward till it divides into three branches in the heart of Cambodia, and ultimately forms the important inland sea of Toulé Sap, whose area exceeds 3000 kilometres before the return of the current temporarily diminishes its volume. The orography and the climatology of the district are carefully treated, but the materials seem still wanting for supplying us with any exact data as to the numbers and ethnic character of the population. —Platycnemia in man and the Anthropoda, by M. Manouvrier. After describing the actual anatomical characters of this peculiar lateral flattening of the tibial bone, the writer considers the grounds on which this condition has been regarded as a character of inferiority by which certain prehistoric and other ancient races would seem to show their affinity to the anthropomorpha. This opinion he absolutely rejects, and finally asserts, as the result of his comparative anatomical investigations of fossil and recent tibiæ, that platycnemia has existed and still exists among the most different human races, although it is of very rare occurrence among certain savage peoples, as the Negroes of Africa, and the Indians of California. He denies that it is a special simian characteristic, since, notwithstanding its frequent occurrence in the chimpanzee and gorilla, it does not present the same features in them as in man, and finally he believes that, even if it were originally transmitted from some arboreal anthropoid, it has been maintained simply by the activity of an essentially human function, its survival being most frequent among nations and tribes addicted to hunting and fishing, or compelled by sudden and great differences of elevation, or extreme inequalities of the surface, to make exertions in ascending and descending steep heights, by which the tibial bones are continuously and often violently exercised. Finally, platycnemia manifests itself only in the human and anthropoid adult, the young being free from it.

SOCIETIES AND ACADEMIES.

SYDNEY.

Royal Society of New South Wales, May 2.—Annual Meeting.—C. S. Wilkinson, Government Geologist, President, in the chair.—The report stated that twenty-four new members had been elected during the year, and the total number on the roll on April 30 was 482.—Dr. Michael Foster, F.R.S., Professor of Physiology, University of Cambridge, had been elected an honorary member.—During the year the Society held nine meetings, at which the following papers were read:—Presidential Address, by Christopher Rolleston, C.M.G.—Recent work on flying machines, by L. Hargrave.—Some N.S.W. tan-substances, Parts 1, 2, 3, and 4, by J. H. Maiden.—Notes on the experience of other countries in the administration of their water supply, by H. G. McKinney.—Notes on some inclusions observed in a specimen of the Queensland opal, by D. A. Porter.—The influence of bush fires in the distribution of species, by Rev. R. Collie.—Origin and mode of occurrence of gold-bearing veins and of the associated minerals, by Jonathan Seaver.—Results of observations of comets vi. and vii., 1886, at Windsor, N.S.W., by John Tebbutt.—Port Jackson silt beds, by F. B. Gipps.—On the presence of fusel oil in beer, by W. M. Hamlet.—Autographic instruments used in the development of flying machines, by Lawrence Hargrave.—The Medical Section held seven meetings, fourteen papers being read; the Sanitary Section four meetings, five papers read; and the Microscopical Section held eight meetings.—The Clarke Medal for the year 1888 had been awarded to the Rev. J. E. Tenison-Woods; the Society's bronze medal and money prize of £25 had been awarded to Mr. Jonathan Seaver for his paper on the origin and mode of occurrence of gold-bearing veins and of the associated minerals; and the Council has since issued the following list of subjects, with the offer of the medal and a prize of £25, for each of the best researches, if of sufficient merit; (to be sent in not later than May 1, 1888) anatomy and life-history of the Echidna and Platypus; anatomy and life-history of Mollusca peculiar to Australia; the chemical composition of the products from the so-called kerosene shale of New South Wales; (to be sent in not later than May 1, 1889) on the chemistry of the Australian gums and resins; on the aborigines of Australia; on the iron ore deposits of New South Wales; list of the marine fauna of Port Jackson, with descriptive notes as to habits, distribution, &c.; (to be sent in not later than May 1, 1890) influence of the Australian climate, general and local, in the development and modification of disease; on the silver ore deposits of New South Wales; on the occurrence of precious stones in New South Wales, with a description of the deposits in which they are found.—The Chairman read the Presidential Address, and the officers and Council were elected for the ensuing year.—A compressed air-engine for driving a flying machine was exhibited by Mr. L. Hargrave. The engine weighed only 2 lbs. 7 oz.; cylinder, $1\frac{5}{8}$ inch diameter; stroke, 2 inches. The receiver for the compressed air was 0·21 cubic feet capacity, made of $\frac{1}{18}$-inch steel, single riveted and brazed. The bursting pressure was 900 lbs., working pressure 500 lbs., and reduced pressure 900 lbs., per square inch. There would be 9200 foot-pounds available for work; this power would have to be expended in from half to three-quarters of a minute. The charged receiver weighed 6 lbs. 12 oz., wood and paper work about 2 lbs. A small Richards's indicator had been made for adjusting the piston-valve. The machine was intended for a flight of 200 yards.

June 6.—Sir Alfred Roberts, President, in the chair.—The Chairman announced that the Council had awarded the Society's medal and prize of £25 to the Rev. J. E. Tenison-Woods for his paper upon the anatomy and life-history of Mollusca peculiar to Australia.—The following papers were read:—Notes on some minerals and mineral localities in the northern districts of New South Wales, by D. A. Porter.—Forest destruction in New South Wales, and its effect on the flow of water in watercourses, and on the rainfall, by W. E. Abbott.—The increasing magnitude of η Argûs, by H. C. Russell, F.R.S.—On a simple plan of easing railway curves, by W. Shellshear.—Indigenous Australian forage plants (exclusive of grasses), including plants injurious to stock, by J. H. Maiden.

July 4.—Sir Alfred Roberts, President, in the chair.—A discussion took place upon Mr. W. E. Abbott's paper on forest destruction in New South Wales, and its effect on the flow of water in watercourses and on the rainfall, read at the preceding meeting. The general result of the discussion was to the effect that the destruction of forests had no appreciable effect on the rainfall.—The following papers were read:—On an improvement in anemometers, by H. C. Russell, F.R.S.—On the anatomy and life-history of Mollusca peculiar to Australia, by the Rev. J. E. Tenison-Woods, in which the author gave evidence as to the existence of eyes in the skulls of many Australian Mollusca.

PARIS.

Academy of Sciences, August 27.—M. Janssen, President, in the chair.—Observations relative to a previous communication on a general property of elastic solid bodies, by M. Maurice Lévy. The author's attention has been called by M. Boussinesq to the fact that the final formula of his note inserted in the *Comptes rendus* of August 13 is found in Prof. Betti's lectures on the theory of electricity. He consequently points out that the theorem, which forms the chief object of that note, must also be accredited to the same illustrious geometrician.—Observations of

Brooks's comet made at the Observatory of Algiers with the 0·50 m. telescope, by MM. Trépied, Sy, and Renaux. The observations are for the period from August 11 to August 15 inclusive. On the former date the brilliancy of the nucleus was about equal to that of a star of the tenth magnitude; diameter of nebulosity about 1′, with faint tail in the direction of the diurnal movement.—Observations of Faye's comet made at the Observatory of Nice, by M. Perrotin. These observations were made on August 11, 14, and 17.—On some experiments with the marine telephone, by M. A. Banaré. These experiments were carried out by order of the Minister of Marine, at Brest, by means of the apparatus to which the author has given the name of "hydrophone." Sounds emitted by various sonorous instruments, such as bells, whistles, and trumpets, were distinctly heard, that of a bell weighing 150 kilogrammes at a distance of 5200 metres. The experiment, with a ship under way also gave favourable results, and here also the ringing of a bell was clearly detected at a distance of 1400 metres simultaneously with the noise of the engine and screw of the tug.—On the remains and zoological affinities of *Testudo perpiniana*, a gigantic fossil turtle of the Perpignan Pliocene epoch, by M. P. Fischer. This magnificent specimen, discovered by M. A. Donnezan, and described by M. Ch. Depéret, has recently been acquired by the Palæontological Department of the Paris Museum. A comparative study of the remains (various parts of the carapace) leads to the conclusion that it must have been a gigantic species of a living African group (*Testudo pardalis*, *sulcata*). Its affinities with the gigantic turtles at present confined to the Aldabra Islands in the Indian Ocean, and the Galapagos in the Pacific, do not appear to have been established. Its relations with the Chersites of South Europe are also doubtful, so that it may be considered as a Pliocene survival in the south of France of an older land fauna of an African type. Its ancestors may perhaps be found amongst the large turtles discovered by M. Gaudry in the Mount Léberon beds, but which are known only by some fragments of the carapace.—The Secretary announced the death of Herr Rudolf Clausius, Corresponding Member of the Section for Mathematics, who died at Bonn on August 24.

BERLIN.

Physiological Society, August 3.—Prof. du Bois Reymond, President, in the chair.—Dr. A. König gave an account of researches which he had carried out, in conjunction with Dr. Brodhun, for the experimental testing of Fechner's psychophysical law in its relationship to the sense of sight. In the case of lights whose brightness varied between the limits $\frac{1}{200}$ and 200000 of the unit used, it was necessary to measure at six different points of the spectrum—that is to say, for six different kinds of monochromatic light—the minimum change of intensity which could be appreciated as a change at all. The experiments were carried out on the trichromatic eye of the speaker and the dichromatic eye of Dr. Brodhun. The observer sat in a dark chamber, into which the eye end of the observing telescope projected, and was able, by the rotation of a handle, to vary the relative brightness of the upper and lower half of the field of vision until the difference was just perceptible. The field of vision was illuminated by a double slit, through which the pure spectral red, orange, yellow, green, blue, or violet light could be admitted. The upper half of the slit was fixed, while the lower half could be widened or narrowed by the observer, and the amount of the alteration in width of the slit observed and recorded by an assistant. The source of light used was a gas-burner with zirconium light. Several thousand separate observations were made, from which it was found that the several colour-systems have no influence on the sensitiveness to differences in brightness of lights; the values obtained in the case of Dr. König's eye were identical with those obtained for Dr. Brodhun's. The shape of the curve which expressed the percentage relationship of the least possible perceptible change in intensity (expressed as an ordinate) to the intensity of the light itself (expressed as an abscissa) was the same for all the above six colours, differing only in the case of lights of minimal intensity. The curve was not a straight line for all intensities of light which were investigated, as it should be according to Fechner's law. In the case of the greatest and least intensities of light it was found that the smallest increase of intensity which was just perceptible was greater than in the case of medium intensities of light. With weak illumination the curve for lights of greater wave-length, such as red, orange, and yellow, was steeper than for lights of shorter wave-length. From this the speaker pointed out that the divergence in the curves of sensitiveness to varying intensities commences with that intensity at which, according to Purkinje, the subjective sensitiveness to lights of different kinds changes as their intensity is diminished, and in the same way as does the sensitiveness to varying intensities. The speaker concluded with some interesting considerations respecting the zero-point of the curve and the negative parts of the abscissæ.—Dr. Uhthoff gave an account of experiments made with a view to determining the amount of change in wave-length of spectral lights which are necessary to produce the least perceptible difference in their colour. The object of the experiments was to subject the results obtained by Drs. König and Dieterici to a renewed testing, in answer to objections which had been raised against them. Using the same apparatus, but a different method, he had confirmed their results. He also found, as Pearce had done in 1883, that the sensitiveness to change of colours is greatest for yellow and blue, and least for red and green.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Beginner's Guide to Photography. 2nd edition (Perken).—A Bibliography of the Foraminifera from 1565 to 1888: C. D. Sherborn (Dulau).—Handbuch der Palæontologie, i. Abtheilung, Palæozoologie, iii. Band, 2 Liefg. (München).—Dr. H. G. Bronn's Klassen und Ordnungen des Thier-Reichs, Erster Band. Protozoa, 47, 48, u. 49, Liefg.: Dr. O. Bütschli (Leipzig).—A Text-book of Euclid's Elements. Parts 1 and 2, containing Books i.-vi.: H. S. Hall and F. H. Stevens (Macmillan).—Catalogue of the Fossil Reptilia and Amphibia in the British Museum (Natural History), Part 1: R. Lydekker (London).—Forschungsreise S.M.S. *Gazelle*, iv. Theil, Botanik *Algen*: Prof. Dr. E. Askenasy (Berlin).—Journal of the Chemical Society, September (Gurney and Jackson.

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THURSDAY, SEPTEMBER 13, 1888.

EXPERIMENTS ON THE GROWTH OF WHEAT.

The Rothamsted Experiments on the Growth of Wheat, Barley, and the Mixed Herbage of Grass Land. By William Fream, B.Sc. Lond., LL.D., Professor of Natural History in the College of Agriculture, Downton. (London: Horace Cox, *Field* Office, 1888.)

THE long series of reports which have emanated from Rothamsted since 1847, and which lie buried to most readers in the Journals of the Royal Agricultural Society, as well as in those of our more purely learned Societies, have long needed an editor. Back numbers of serials are not particularly attractive to the modern reader. The laborious papers by Sir John Lawes and his indefatigable colleague Dr. Gilbert would have run some little danger of being buried alive had not an able editor and exponent been found. Happily, Dr. Fream possessed the necessary knowledge and discrimination for this task, and, with the entire concurrence of the original investigators, the upshot is a valuable digest of a certain section of the results obtained—namely, those relating to the cereals and the grasses. The volume is adapted for reference rather than for rapid reading, although the sections upon the influence of climate on the cultivation of wheat, and upon the home produce, imports, and consumption of wheat, are less close in fibre, and may be scanned with greater ease. The book is, in fact, rather for students than for the omnivorous reader, but nevertheless appeals to a very large constituency. All landlords, land agents, and farmers, as well as agricultural students (now a numerous class), will welcome it as giving, in a compendious form, and in digested condition, matter which is scattered through many periodicals.

The results of continuous wheat and barley growing year after year upon the same land—without manure of any kind, with annual dressings of dung, with annual dressings of nitrogenous manures, with annual dressings of mineral manures, and with annual dressings of mixed nitrogenous and mineral manures—are all given. The fact that wheat and barley have been grown for forty years in succession without manure upon the same land, while the entire straw and grain have been removed, is in itself striking, and still more singular is it that the average produce during all these years is equal to the average yield of Australia, and exceeds that of many of the United States of America. It is also noteworthy that the yield of the last crop comprised in these reports—that of 1883—is 13¾ bushels per acre, or within one-fourth bushel of the average during the entire period of forty years. With regard to manures, minerals alone have added very slightly to the unmanured produce; whereas, manures containing nitric acid alone, or some easily nitrifiable compound of nitrogen, have considerably increased the crop. Manures consisting of potash, phosphoric acid, and nitrogen in the form of ammonia salts or nitrates, are able to grow heavy crops of wheat continuously. It is clearly shown that such compounded fertilizers, containing both the mineral and nitrogenous constituents of plant food, can grow crops superior to what are produced by annual dressings of fourteen tons per acre of farmyard manure. Also the proportion of the nitrogen applied which is made use of by the growing crops is much higher in the case of the artificial fertilizers than in the case of the farmyard manure. A larger proportion, in fact, of the nitrogen applied is recovered by the crop in the case of the artificial dressings. On the other hand, the residuary effect of nitrogen applied in combination with carbon (as in farmyard manure) is much greater than in the case of applications of prepared salts of ammonia or of nitric acid.

The ease with which fertility can be kept up by artificial applications forms, in the opinion of many agriculturists, a reason for discarding the more cumbrous method of keeping up the fertility of land by means of live stock and the dung-cart. But it must be remembered that no artificial manure accumulates fertility in a soil like farmyard manure, and its nitrogen, being liberated gradually, is available over a long series of years, and especially so at those seasons of the year in which vegetation is most in need of it.

The grass experiments are of great interest. First, we have the different quantities of hay produced by various dressings of manurial substances; but more remarkable are the changes brought about in the species of grasses predominating on various plots by the influence of fertilizers applied during a long series of years. On the plot, for example, to which ammonia salts have been continuously applied for thirty years, the total number of the species originally extant has been much reduced, three-quarters of the produce being composed of *Festuca ovina* and *Agrostis vulgaris*. The leguminous herbage has disappeared. On the plot manured continuously with superphosphate, the number and relative predominance of the plant species is much the same as without manure, with a prevalence of *Lathyrus pratensis* among the Leguminosæ, and an increase of *Ranunculus repens*, *R. bulbosus*, *Achillea Millefolium*, and *Rumex Acetosa*. Again, when ammonia salts and mixed mineral manures are applied, *Poa pratensis* becomes the prevailing grass. These examples must suffice to show the great changes wrought by continuous applications, and the principle of the survival of the fittest under regulated alterations of the environment.

Complicated and multifarious as are these experiments, the general conclusions for the guidance of agriculturists are reducible to a few simple deductions. Thus the superior excellence of nitrate of soda as a fertilizer for cereals and for grasses is distinctly shown. The necessity of nitrogenous manures, such as nitrate of soda and ammonia salts, as means of bringing out or developing the effect of the so-called mineral manures, such as potash and phosphates, is constantly proved. The comparatively small value of many constituents of plants (owing to their already existing in sufficient quantities in most soils), such as soda, magnesia, and silica, is also placed beyond doubt. The residual effect of farmyard manure, and its consequent power of not only keeping up but indefinitely increasing the fertility of a soil, are points greatly in its favour; while the slowness of its action, and the very small proportion of its nitrogen which appears to be recoverable at any particular time, are considerations which weigh against it. The residual effect of mineral dressings applied many years ago as affected and brought out by continuous dressings of nitrogenous manures is another significant fact; while the evanescent effect of nitrates applied as salts contrasts unfavourably with the continued effects of nitrogenous matter in organic combination with carbon. Prof. Fream's book is a substantial addition to agricultural literature, and it is satisfactory to find that the editing of such important results has been carried out, with the "kind and ready" assistance of Sir John Lawes and Dr. Gilbert, by one who brings sound scientific attainments to bear upon a stupendous number of observations made during a series of forty years. There is room for a second, if not a third volume, as the experiments upon the cultivation of the root crops, the leguminous crops, and the elaborate researches made at Rothamsted upon the fattening of animals, are not touched in this first instalment.

THE JAPANESE VOLCANIC ERUPTION.

THE *Times* of Tuesday contains a long letter from its Japan Correspondent describing the scene of the recent volcanic explosion in the Bandai-san region in Northern Japan. This is the first account by a foreign eye-witness that has reached the outside world. The writer appears to have started immediately from Tokio for the scene of the disaster, where he spent four days going carefully over the ground, examining the phenomena connected with the outburst, and hearing the stories of the survivors. The communication which is the result of these investigations, and which was evidently written while the powerful impression left by the scene of awful desolation was still fresh in the writer's mind, is probably one of the most graphic and detailed accounts of the immediate results of a stupendous volcanic explosion that has ever been published. Bandai-san is a mountain about 5800 feet high, and has shown no sign of activity for about eleven hundred years. On its north-eastern flank was a subordinate peak known as Little Bandai-san, which rose directly above a group of three solfataras.

At about 8 o'clock on the morning of July 15 (here, as throughout almost the whole of this article, we quote the *Times* Correspondent), almost in the twinkling of an eye, Little Bandai-san was blown into the air and wiped out of the map of Japan. A few minutes later its *débris* had buried or devastated an area about half the size of London. A dozen or more of upland hamlets had been overwhelmed in the earthen deluge, or wrecked by other phenomena attending the outburst. Several hundreds of people had met with sudden and terrible death. Scores of others had been injured; and the long roll of disaster included the destruction of horses and cattle, damming up of rivers, and laying waste of large tracts of rice-land and mulberry-groves. A small party was organized in Tokio to visit the scene. As the travellers approached the mountain, they were told that twenty miles in a straight line from Bandai-san no noise or earthquake was experienced on the 15th, but mist and gloom prevailed for about seven hours, the result of a shower of impalpable blue-gray ash, which fell to a depth of half an inch, and sorely puzzled the inhabitants. An ascent of about 3000 feet was made to the back of the newly-formed crater, so as to obtain a clear view of it and of the country which had been overwhelmed. Only on nearing the end of the ascent were they again brought face to face with signs of the explosion. Here, besides the rain of fine gray ashen mud which had fallen on and still covered the ground and all vegetation, they came upon a number of freshly-opened pits, evidently in some way the work of the volcano. Ascending the last steep rise to the ridge behind Little Bandai-san, signs of the great disaster grew in number and intensity. "Fœtid vapours swept over us, emanating from evil-looking pools. Great trees torn up by their roots lay all around; and the whole face of the mountain wore the look of having been withered by some fierce and baleful blast. A few minutes further and we had gained the crest of the narrow ridge, and now, for the first time, looked forth upon the sight we had come to see. I hardly know which to pronounce the more astonishing, the prospect that now opened before our eyes or the suddenness with which it burst upon us. To the former, perhaps, no more fitting phrase can be applied than that of absolute, unredeemed desolation—so intense, so sad, and so bewildering, that I despair of describing it adequately in detail. On our right, a little above us, rose the in-curved rear wall of what, eight days before, had been Sho-Bandai-san, a ragged, almost sheer, cliff, falling with scarce a break, to a depth of fully 600 feet. In front of this cliff everything had been blown away and scattered over the face of the country before it in a roughly fan-shaped deposit of for the most part unknown depth—deep enough, however, to erase every landmark and conceal every feature of the deluged area. At the foot of the cliff, clouds of suffocating steam rose ceaselessly and angrily, and with loud roaring, from two great fissures in the crater bed, and now and then assailed us with their hellish odour. To our eyes, the base denuded by the explosion seemed to cover a space of between three and four square miles. This, however, can only be rough conjecture. Equally vague must be all present attempts to determine the volume of the disrupted matter. Yet, if we assume, as a very moderate calculation, that the mean depth of the *débris* covering the buried area of thirty square miles is not less than 15 feet, we find that the work achieved by this last great mine of Nature's firing was the upheaval and wide distribution of no fewer than 700,000,000 tons of earth, rocks, and other ponderous material. The real figure is probably very much greater."

The desolation beyond the crater, and the mighty mass thrown out by the volcano which covered the earth were almost incredible. "Down the slopes of Bandai-san, across the valley of the Nakasegawa, choking up the river, and stretching beyond it to the foot-hills five or six miles away, spread a vast billowy sheet of ash-covered earth or mud, obliterating every foot of the erstwhile smiling landscape. Here and there its surface was dotted or streaked with water. Elsewhere the eye rested on huge disordered heaps of rocky *débris*, in the distance resembling nothing so much as the giant concrete block substructure of some modern breakwater. It was curious to see on the farther side the sharp line of demarcation between the brown sea of mud and the green forests on which it had encroached; or, again, the lakes formed in every tributary glen of the Nakasegawa by the massive dams so suddenly raised against the passage of their stream waters. One lake was conspicuous among the rest. It was there that the Nakasegawa itself had been arrested at its issue from a narrow pass by a monster barrier of disrupted matter thrown right across its course. Neither living thing nor any sign of life could be descried over the whole expanse. All was dismally silent and solitary. Beneath it, however, lay half a score of hamlets, and hundreds of corpses of men, women, and children, who had been overtaken by swift and painful deaths."

Near by two houses, built for the accommodation of visitors to the hot springs were overwhelmed, and a little lower down two spa-hamlets were absolutely buried in mud. From various indications, especially a comparison of the places destroyed with those saved, it appears that the disruptive force must, in the main, have been directed outwards from the hill-face at a considerable inclination to the vertical. On no other hypothesis is it possible to account for some of the most startling phenomena, for the great distances reached by the waves of *ejectamenta*, and for the incredibly brief intervals that elapsed between the short-lived explosion and the submersion of large tracts many miles away from the crater. It must not, however, be supposed that the havoc wrought by the volcano's fury was limited to the fall of disrupted matter, or to the area covered by it. Besides the rain of scalding earth and mud, heated rocks and stones, sand, and hot softly-falling ashes, there were the awful shocks of the explosion, accompanied by winds or whirlwinds, which every survivor describes as of intense and extraordinary vehemence. Nowhere, of course, were the effects of these concomitants more fierce than on the heights of Bandai-san. The forests on the unburied slopes above and near the crater presented a weird spectacle. In these hardly a stick was left standing. As if some giant reaper had mown down whole acres with a sweep of his sickle, the trees lay literally in hundreds on the ground, all felled in a direction away from the crater, stripped of branches, leaves, and even of their bark, and twisted into the most grostesque contortions.

One day was given to exploring the buried area at its lower levels in the valley of the Nakasegawa, and also the outskirts of the volcanic deluge. At one place, a secondary earth-wave, issuing from the crater by a lateral gap, had rushed swiftly down the mountain-side, burying a large party of grass-cutters and horses, and reaching, but only half destroying, the little hamlet of Mine. Its energy seems to have exactly spent at this point. It was strange to see the great wall of earth and stones, with its vertical face some 7 or 8 feet high, brought up all-standing, as it were, by a frail farm outbuilding. A yet stranger sight was it to see the enormous masses of rock that were strewn about on the surface of the neighbouring field of mud. One of them, which was measured, weighed at least 200 tons. Higher up, on the far side of the river, a couple of large villages, in which, though not reached by any mud-stream, not a house was whole, many had been levelled to the ground; others were tottering on the verge of destruction; and of the rest, all were cracked, mutilated, unroofed, twisted, tilted up, or otherwise injured or partially wrecked. A scene of more ruthless and utter desolation could hardly be conceived. Beyond this, the route entered upon the great earth-field visible from the heights of Bandai-san. Nothing could convey a more vivid idea of the destructive forces that were let loose upon that doomed region than a sight of the wild chaos of earth, rock, and mud which now reigns over its surface. The whole effect in some places is much as if a raging sea of those materials, on a gigantic scale, had been suddenly congealed and made to stand still. At one spot there is a long mud precipice, said by some observers to be fully 200 feet high.

Although the little village of Nagasaka was comparatively uninjured, nearly all its able-bodied inhabitants lost their lives in a manner which shows the extraordinary speed with which the mud-stream flowed. When Little Bandai-san blew up, and hot ashes and sand began to fall, the young and strong fled panic-stricken across the fields, making for the opposite hills by paths well known to all. A minute later came a thick darkness, as of midnight. Blinded by this, and dazed by the falling *débris* and other horrors of the scene, their steps, probably also their senses, failed them. And before the light returned every soul was caught by a swift bore of soft mud, which, rushing down the valley bed, overwhelmed them in a fate more horrible and not less sudden than that of Pharaoh and his host. None escaped save those who stayed at home—mostly the old and very young.

From the stories told by the survivors, as well as from his own observations, the writer sketches the following sequence of events connected with the outburst:—

It seems clear from every account that one of the most terrible features of the catastrophe must have been its appalling suddenness. Though there had been, it is said, slight shocks of earthquake for a couple of days before, and, according to some witnesses, strange subterranean rumblings and suspicious variations in the temperature and volume of the hot springs, these caused no grave alarm. Nothing worthy to be called a serious warning occurred until about 7.30 a.m. on the 15th. Then came a violent earthquake, followed a quarter of an hour later by a second, yet more intense. Ten minutes after there ensued throes of such terrible severity that the ground heaved and fell, people were thrown down, and houses demolished or wrecked. To all it seemed that their last hour had come. Instantly upon this arose a fearful noise, described by some as like that of a hundred thunders, by others as the most unearthly sound that ever startled the ears of men. Little Bandai-san was seen to be lifted bodily into the air and spread abroad, and after it leaped forth tongues of flame and dense dark clouds of vapour of *ejectamenta*. Of the ensuing phenomena it is hard to gain any clear idea from the tales of the distracted survivors. Apparently, however, a quick succession of reports, accompanied by violent earth-throes and winds of hurricane force, lasted for about a minute. Then began the shower of ashes, dust, hot water, and leaves. The light quickly faded as the exploded matter spread over the firmament, so that day was soon changed into night, and did not return for a space of several minutes. Meanwhile, the avalanches of earth and mud must have already done much of their deadly work. The interval between the explosion and the arrival of the mud-torrent which swept past that hamlet cannot have been more than from ten to fifteen minutes. Before the light was restored, all the flower of the village had been swallowed up. How that long journey of some ten miles from the crater had been performed by the mud at such an astonishing speed it is impossible to say. There is evidence that in places the earth-flow lasted for about an hour. But in the above we have the clearest proof that some at least of the destroying matter was hurled over the country at railroad speed, even after being deflected through wide angles from its original line of motion.

We may, perhaps, hope to learn something hereafter that will throw a clear light on the immediate cause of the explosion (the agent, it cannot be doubted, was steam), on the approximate volume of the projected matter, on the partiality of the effects, and on the many and most bewildering mysteries connected with the propagation and distribution of the earth-waves, rocks, &c. Meanwhile we have before us the fact that a massive mountain peak has been blown to bits by an explosion within its bowels powerful enough to toss many hundred millions of tons of material high into the air, and to change the face of nature over an area of some thirty square miles. While whole forests were levelled by the shock, the disrupted matter dammed up rivers, deluged and drowned the land and crops, and buried a dozen hamlets. Earthquakes and *coups de vent* added their quota to the work of destruction. Nearly 600 people perished by horrible deaths in their mountain homes and valleys. Four times that number have been reduced to destitution or dire poverty. With one possible exception, it is the gravest disaster of its class that has happened, even in that land of volcanoes, since the famous eruption of Asamayama in 1783, and it cannot but be ranked among the most startling volcanic explosions of which history has any record.

It is interesting to know that experts are already at work investigating some of the problems here sketched out by the *Times* Correspondent, and happily Japan is well provided with experts in the science of seismology, at their head being Prof. Milne, the leading seismologist of the day. Seeing also the countenance given to the study of these phenomena by the Japanese Government, it may be anticipated that no volcanic eruption of modern times will have been so carefully and scientifically investigated as this of Bandai-san, as none has been so graphically and eloquently described.

CALCULATION OF RANGES, ETC., OF ELONGATED PROJECTILES.

FROM time to time it has been suggested to me that some reduction in the coefficients of resistance deduced from my experiments made in 1867–68, is required to adapt them for use in connection with the improved guns of more recent times. I do not agree with those suggestions. My coefficients were most carefully deduced from experiments made with ogival-headed shot fired at very low elevations so as to secure ranges of about 500 or 600 yards, and the observations were made near the gun. The 5-inch gun was a remarkably good gun, and from the numerous records it gave had a preponderating effect on the final result; while an unsteady shot cut only a few screens, and had a very trifling influence. It seems, therefore, that the coefficients were derived from shot moving very nearly in the direction of their axes. I have applied these coefficients to calculate ranges for comparison with Commander May's (R.N.) range-table for the 12-inch muzzle-loading gun (based on practice 1885); muzzle velocity, 1892 f.s.; "jump," 6 minutes.

Elevation		1°	2°	3°	4°	
Exp. range	...	1200	2267	3200	4057	yards.
Calc. range	...	1206	2249	3192	4039	,,
Difference	...	+6	−18	−8	−18	,,

I will now do the same for the 4-inch breech-loading gun, which was the gun chosen by the authorities to be used in testing my coefficients on a long range; muzzle velocity, 1900 f.s.; range-table founded on experiments made in 1884; "jump," 6 minutes.

Elevation		1°	2°	3°	4°	
Exp. range	...	1086	1811	2400	2917	yards.
Calc. range	...	1049	1817	2410	2895	,,
Difference	...	−37	+6	+10	−22	,,

Thus it appears that my coefficients give very satisfactory results when applied under the conditions of the original experiments. Commander May's table stops at a range of 4000 yards. As the elevation of the 4-inch gun was gradually increased, the calculated ranges fell shorter and shorter of the experimental ranges. At an elevation of 15° the calculated range was 6364 yards, and the experimental range 6608 yards, giving a difference of 244 yards. The explanation of this seems to me to be as follows:—

When an elongated shot is fired from a rifled gun at high elevations, the shot endeavours to preserve the parallelism of its axis. This causes the axis of the shot to become sensibly inclined to the direction of the motion of its centre of gravity. Thus the pressure of the air acts from below and raises the shot bodily, so as to give its trajectory an increased elevation. This would naturally increase the range of the shot. After a short time the shot inclines sideways, as explained by Magnus, and the shot continues to move with its axis inclined to the direction of its motion, which is the cause of the lateral "drift" of the shot. This shot having had its axis so much inclined to the direction of its motion, would encounter a greater resistance from the air than another shot fired at a lower elevation, because this latter would move with its axis more nearly in the direction of its motion.

Hence it is clear that, in order to apply any rational correction to the calculated ranges for high elevations, it would be necessary slightly to *increase* both (1) the elevation, and (2) the values of the coefficients of resistance.

Major Mackinlay, R.A., warns us that the published range-tables are not to be "blindly followed," a very necessary caution, when it is considered that we cannot be quite certain about the muzzle velocity, the "jump," the elevation, and the precise form of the head. The height of the barometer is seldom mentioned. My only surprise is that such good agreement between calculation and experiment should be found as above. The only question seems to be whether it is worth while to trouble about the correction of calculated ranges for high velocities and high elevations, when the reason for some little discrepancy is so evident. But to *reduce* coefficients would be to make matters worse.

Having been requested to calculate the range of a 9·2-inch shot weighing 380 pounds, fired at an elevation of 40° with a muzzle velocity of 2360 f.s., I could not feel satisfied till I had completed the calculation of a range-table for elevations 0° to 45° on a horizontal plane 27 feet below the muzzle. I give the result. Gravity and the temperature of the air were considered constant. The air was supposed to be at rest, and the shot was assumed to move in the direction of its axis; head ogival, struck with a radius of 1½ diameter. When the results of experiment are published I shall be ready to discuss the matter, but there are so many things uncertain at heights of 10,000, 15,000 feet, &c., that I doubt whether any theoretical advantages will result. It will, however, be interesting to know what can be done in an extremity.

It will be seen that the ranges go on increasing up to an elevation of 45°, and would probably go on beyond an elevation of 50° before reaching a maximum.

Elevation.	Range,	Height of Vertex.	Time of Flight.	Angle of Descent.	Striking Velocity.	Horizontal Striking Velocity.
°	Yards.	Feet.	Seconds.	° ′	f.s.	y.s.
0	969	0	1·3	1 4	2,154	718
1	2,115	25	3·0	1 35	1,931	643
2	3,416	94	5·1	2 47	1,708	569
3	4,611	237	7·1	4 20	1,528	508
4	5,600	343	9·4	5 52	1,399	464
5	6,475	517	11·4	7 38	1,291	426
6	7,271	716	13·4	9 30	1,200	395
7	7,999	937	15·3	11 28	1,128	368
8	8,669	1,180	17·1	13 28	1,075	349
9	9,291	1,445	18·9	15 28	1,040	334
10	9,876	1,731	20·6	17 23	1,022	325
11	10,430	2,036	22·3	19 9	1,015	320
12	10,952	2,360	23·9	20 54	1,009	314
13	11,448	2,703	25·5	22 38	1,003	309
14	11,922	3,065	27·0	24 21	998	303
15	12,379	3,443	28·5	26 2	993	297
16	12,804	3,835	30·0	27 40	990	292
17	13,217	4,242	31·5	29 15	987	287
18	13,618	4,663	33·0	30 48	985	282
19	14,007	5,099	34·4	32 19	984	277
20	14,385	5,550	35·9	33 48	984	273
21	14,750	6,015	37·3	35 15	985	268
22	15,103	6,489	38·8	36 40	987	264
23	15,445	6,970	40·2	38 3	990	260
24	15,775	7,459	41·6	39 24	993	256
25	16,092	7,956	43·0	40 41	996	252
26	16,398	8,461	44·4	41 54	1,000	248
27	16,691	8,974	45·7	43 2	1,004	245
28	16,973	9,494	47·1	44 6	1,009	242
29	17,242	10,022	48·4	45 7	1,014	239
30	17,501	10,558	49·7	46 5	1,019	236
31	17,747	11,102	51·0	47 1	1,025	233
32	17,981	11,654	52·2	47 56	1,031	230
33	18,203	12,214	53·5	48 50	1,037	228
34	18,413	12,782	54·7	49 43	1,044	225
35	18,612	13,357	56·0	50 35	1,051	222
36	18,799	13,941	57·2	51 27	1,058	220
37	18,973	14,534	58·5	52 18	1,065	217
38	19,136	15,136	59·7	53 8	1,072	214
39	19,287	15,747	61·0	53 58	1,079	212
40	19,426	16,368	62·2	54 47	1,086	209
41	19,553	17,001	63·4	55 36	1,092	206
42	19,668	17,646	64·7	56 24	1,099	203
43	19,772	18,302	65·9	57 11	1,105	200
44	19,864	18,969	67·1	57 57	1,111	197
45	19,944	19,648	68·3	58 43	1,117	193

F. BASHFORTH.

THE BRITISH ASSOCIATION.

BATH, *Tuesday Evening*.

So far as numbers are concerned, the Bath meeting has been below the average. The number of tickets sold has been about 50 less than 2000. This is a marked contrast to last year's meeting, which beat the record; and is even less by some hundreds than the former Bath meeting. But then it should be remembered that that meeting presented attractions of an unusual kind: the lion-hunters who form so large a section of these annual gatherings had such prey presented to them as Livingstone, Burton, and Speke. As will be seen, the diminished attendance has told to some extent on the grants, several of which have had unfortunately to be reduced below the sums originally proposed and approved of. All sorts of reasons have been put forward to account for the comparatively small attendance, and probably there is a little truth in each. Probably the excursions have had as much to do with it as anything else; those of Saturday presented few attractions, except that to the Severn Tunnel and the Barry Docks. Curiously enough, however, scarcely anyone entered for that excursion, and had the enterprising secretaries of Section G not taken it in hand, it would have fallen through. As it was, it turned out one of the most successful of Saturday's excursions. Small as the attendance has been, the accommodation of the town has been strained, and several of the guests of the Local Committee speak somewhat disrespectfully of their quarters. But the Local Committee have done their best, and they have no reason to be dissatisfied with their success. The reception-room accommodation has certainly been limited, and members have missed the smoking-room, refreshment-rooms, and other amenities with which they were indulged at Manchester last year. Fortunately the weather has been, on the whole, good, so that people have not greatly felt the want of indoor accommodation. Notwithstanding the small attendance, the crush at the two *soirées* was excessive, mainly arising from the smallness of the Assembly Rooms. The Drill Hall has proved satisfactory for all the public lectures. Sir Frederick Bramwell's address was, as might have been expected, received with universal appreciation; while the public lectures were all well attended. Prof. Ayrton's address on the transmission of power was so highly appreciated that he has been asked to repeat it for the benefit of the working classes. Tickets for Sir John Lubbock's lecture to the "working classes" were so greatly in demand, that many of those for free distribution were being sold throughout the town at 2*s*. 6*d*. and 5*s*.

One of the great attractions at the present meeting has been the recently unearthed Roman baths. They are in almost complete preservation; the lead lining and lead piping nearly perfect, the steps, the columns, the carvings, in wonderful preservation, the whole probably forming a more complete specimen of this class of Roman work than exists anywhere else. Even greater, however, has been the excitement over the phonograph and graphophone. Crowds have been besieging Section G in order to see and hear the wonderful little cylinders; and daily receptions have been given both by Colonel Gouraud and Mr. Edmunds of the rival instruments. Each has its strong body of partisans, but the general result seems to be that law and not science will be the final arbiter of the merits of the two.

In the ordinary work of the Sections there have been various exciting episodes. The discussion between Sections B and D, on the chemistry of certain physiological processes, was one of great importance, and it is hoped it will be well reported. The discussion on stays and waist-bands was probably more entertaining than instructive; while that on coral-reefs, though valuable, suffered from the absence of some of the leading authorities on the subject. The discussion in Section H, on the few remarks by Mr. Park Harrison on the question "What is a Nation?" had somewhat of a political flavour about it. It was taken part in by General Pitt-Rivers, Sir John Lubbock, Prof. Sayce, and Dr. John Evans. Another discussion which, like the papers on the phonograph and graphophone, nearly emptied the other Sections, was that on lightning-conductors, on Tuesday, in Section G. These various discussions, combined with the fact that so many foreign geologists were present in Section C, have contributed to keep the second Bath meeting up to a good average.

It seems to be generally admitted that the Presidential Address in Section D, by Mr. Thiselton Dyer, was the weightiest from a scientific point of view. It was the longest, all the addresses this year being marked by brevity. Some little amusement has been caused by the very modified admission made by Sir William Thomson, in his paper in Section A, on "A Simple Hypothesis for Electro-magnetic Induction of Incomplete Circuits," that, after all, Clerk Maxwell may have been to some extent not altogether wrong.

The meeting next year will be presided over by Prof. Flower. Leeds will receive the Association in 1890, while Edinburgh and Cardiff compete for the honour of a visit in 1891; there can be little doubt of the result if the Corporation and the University of Edinburgh give snbstantial evidence of their zeal.

The following is the list of grants which have been allotted by the General Council:—

	£
A.—*Mathematics and Physics.*	
Ben Nevis Observatory	50
Electrical Standards	100
Electrolysis	20
Solar Radiation	10
Differential Gravity Meter	10
Uniform Nomenclature in Mechanics	10
Calculating Tables of Certain Mathematical Functions	10
Seasonal Variations in the Temperature of Lakes, Rivers, and Estuaries	30
B.—*Chemistry.*	
The Influence of the Silent Discharge of Electricity on Oxygen and other Gases	10
Methods of Teaching Chemistry	10
Oxidation of Hydracids in Sunlight	10
C.—*Geology.*	
Geological Record	80
Erratic Blocks	10
Volcanic Phenomena of Japan	25
Volcanic Phenomena of Vesuvius	20
Fossil Phyllopoda of the Palæozoic Rocks	20
Higher Eocene Beds of the Isle of Wight	15
Fossil Plants of the Tertiary and Secondary Beds of the United Kingdom	15
D.—*Biology.*	
Zoology and Botany of the West India Islan ls	100
Marine Biological Association	200
Flora of China	25
Naples Zoological Station	100
Physiology of the Lymphatic System	25
To Improve and Experiment with a Deep-sea Tow-net for opening and closing under Water	10
Natural History of the Friendly Islands	100
E.—*Geography.*	
Geography and Geology of the Atlas Ranges	100
F.—*Economic Science and Statistics.*	
Precious Metals in Circulation	20
Variations in the Value of the Monetary Standard	10
G.—*Mechanical Science.*	
Investigation of Estuaries by means of Models	100
Development of Graphic Methods in Mechanical Science	25

H.—*Anthropology.*	£
Effect of Occupations on Physical Development	20
North-Western Tribes of Canada	150
Editing a New Edition of Anthropological Notes and Queries	50
Calculating the Anthropological Measurements taken at Bath	5
Exploration of Roman Baths at Bath	100
Characteristics of Nomad Tribes of Asia Minor	30
For carrying on the Work of the Corresponding Societies Committee	20
Total	£1645

SECTION B.

CHEMICAL SCIENCE.

OPENING ADDRESS BY PROF. WILLIAM A. TILDEN, D.Sc. LOND., F.R.S., F.C.S., PRESIDENT OF THE SECTION.

A PART of the duty which devolves upon the President of a Section of the British Association consists in delivering an address, and the knowledge that a pretty full liberty of choice is permitted in regard to the selection of a subject is the only source of comfort which serves to alleviate the onerous nature of the task.

It seemed to me that the time is gone by when an attempt to review progress over the whole field of chemical science is likely to be useful or even possible, and an account of what is being done within the narrow limits of those parts of the science to which I have been able to give special attention would be ill-adapted to the character of a speech addressed to the members of the Section collectively. The fact that at the last meeting of the Association a Committee was appointed to inquire into the methods at present adopted for teaching chemistry suggested that, as I had not been able to accept an invitation to join this Committee, I might make use of this opportunity for contributing to the discussion. The first report of the Committee will be received with much interest by the Section. As might be expected, it embodies the expression of many varieties of opinion.

The existence of chemistry as a department of science not merely requiring the observation of facts that are to be made useful, but seeking in the accumulated stores of observation to discover law, is a thing of comparatively recent growth. How chemistry arose out of alchemy I need not remind you, but the connection between the study of chemistry and that of medicine, and the maintenance of this connection down to even the present generation, is illustrated by the fact that a large number of men who have become eminent as chemists began their career in the surgery or the pharmacy. Black, Davy, Berzelius, Wollaston, Wöhler, Wurtz, Andrews, and W. A. Miller began by the study of medicine, whilst Scheele, H. Rose, and the great names of Liebig and Dumas are to be found in the long roll of those who received their earliest notions of chemistry in the pharmaceutical laboratory. Chemistry has been gradually emancipated from these associations with enormous advantage to both sides. So long as technical purposes alone were held in view a scientific chemistry could not exist, but no sooner did the study take an independent form and direction than multitudes of useful applications of the facts discovered became apparent.

It is only within a comparatively few years, however, that universities, in this country at least, have ceased to deal with chemistry as a kind of poor relation or humble follower of medicine, and have permitted her to emerge from the cellars of a museum or school of anatomy and have given her a commodious dwelling in the fair light of day.

In the old time such instruction in chemistry as was given in the universities and mining or technical schools seems to have taken the form of lectures read by the Professor, and access to a laboratory for practical manipulation seems to have been a high privilege accorded only under exceptional circumstances to the few. We are told, for example, that when Liebig went to Paris in 1823 he applied to Gay-Lussac for practical instruction at first without success, and that admission to the laboratory of the Ecole Polytechnique was ultimately granted him only through the intervention of Von Humboldt.

In a great many cases the student of chemistry must have been almost entirely dependent upon private study, though books were scarce and materials more costly than now. Davy, for example, seems to have had no instruction whatever previous to his appointment as assistant to Dr. Beddoes at the Pneumatic Institute at Bristol.

Doubtless, therefore, the recollection of his own early difficulties when seeking instruction contributed largely to influence Liebig in the establishment of the laboratory in the University of Giessen, and in the adoption of the principles which guided his teaching there. For the first time in the history of chemistry students met not merely to listen to the discourse of a professor concerning his own experiments and conclusions, but to examine for themselves the basis of the theories taught, to learn the processes of analysis, and by independent investigation to extend the boundaries of existing knowledge.

The fame of the new school spread fast and far, and soon men from every part of the civilized world assembled to share in the advantages offered. The influence of the new method can be estimated when we reflect that nearly all the now passing generation of chemists in England and America obtained the greater part of their training in Liebig's laboratory; and as a large number of them have been teachers, it may be assumed that they transplanted into their own countries the methods they had learnt from the great German master.

It was not till 1846, long after the school at Giessen had risen into fame, that in England a sense of our deficiencies in respect to provision for teaching chemistry was felt strongly enough to lead to the establishment of a College of Chemistry. At that time the Professor of Chemistry at Oxford was also Professor of Botany. At Cambridge it was thought praise and boast enough that the occupant of the chair of chemistry had, during more than thirty years, frequently resided at the University and every year gave a course of lectures. The Jacksonian professorship was not then, as now, in the possession of a chemist. University College, London, had at this period a very distinguished man in the chair of chemistry, but it was only in 1848 that a commodious laboratory was provided by public subscription, raised in commemoration of the services of Dr. Birkbeck in promoting popular education. In that year Fownes was appointed to co-operate with Graham in the work of teaching, though his premature death soon after left but little time for the fulfilment of the rich promise of his earlier years. At Manchester, John Owens had died in 1846, leaving the bulk of his estate for the purpose of establishing a university in Manchester, but as yet the Owens College was not.

The foundation of the College of Chemistry in 1846 was therefore an event of supreme importance in the history of chemical teaching in this country; and though at the time some dissatisfaction was expressed at the choice of the professor selected to direct the work, who, though a distinguished pupil of Liebig, was not an Englishman, all British chemists now concur in believing the choice to have been a most fortunate one. The great majority of my contemporaries having begun, continued, or ended their studies in Oxford Street, they and all who have come under Dr. Hofmann's teaching know how vast was his capacity for work and how marvellous was the power he possessed of communicating his own enthusiasm to his pupils.

Since the time of which I have been speaking the means of instruction in science in England have multiplied enormously. In University College, London, founded in 1828, and in Owens College, Manchester, founded in 1851, not only have chairs of chemistry existed from the first, but they have been occupied by a succession of chemists of the highest eminence. But long after 1846 the whole of the serious teaching of scientific chemistry was accomplished at the College of Chemistry, and it was nigh upon twenty years before the Manchester school began to attract considerable notice.

In 1872-73 the movement set in which has resulted in the erection of colleges for higher instruction at a number of important English and Welsh towns. These, together with the pre-existent Queen's Colleges in Ireland and the Universities of more ancient foundation in the three kingdoms, are for the most part provided with pretty good laboratories and a competent staff. We have also the Normal School of Science and the Institute raised by the City and Guilds of London at South Kensington, and its Associate College at Finsbury. England is therefore at the present time as well provided with *places* of instruction for the study of chemistry as any country in the world.

And a very large proportion of the professors or heads of the chemical schools in the colleges and universities of the United Kingdom have shown by their own activity in research that they are qualified to give instruction of the highest kind, and are ready to train young chemists in the art as well as in the theory of their subject.

It is therefore no longer true that a student desiring to become a scientific chemist must needs choose between a single institution in London and another in Manchester, or must seek the instruction which he cannot get at home in the laboratory of a foreign university. As an element in a liberal education the position of chemistry is also considerably in advance of what it was twenty years ago.

It is nevertheless true that increased opportunities for study, a considerable supply of capable teachers, and an enormous body of students, have not produced such an amount of original investigation, or even of accurate analytical work, as might reasonably be expected. A full and complete explanation of all the influences which contribute to this result would be difficult; but I think the apparent inactivity of the chemical schools in this country is not generally the fault of the professors, but is chargeable in the main to the ignorance, and partly to the indifference, of the public. There exists as yet no intelligent feeling in favour of learning, nor indeed in favour of any sort of education, unless there is expectation of direct returns in the form of obvious practical results. It is this which animates the present popular movement in favour of so-called "technical" education. That part of the attention of the nation which can be spared from the contemplation of Irish affairs is concentrated upon the problem of how to make every little boy learn the rudiments of chemistry, whether he likes it or not, whilst there are comparatively few people interested in the question of how to provide means and instruction for those who are capable and desirous of attaining to a mastery of the subject. Moreover, the public have not yet grasped this truth, that, so far as chemistry is concerned, it is of very little consequence to the great metallurgical and chemical industries whether the workpeople do or do not know a little chemistry, though it is important that they should be intelligent enough to obey orders. What is wanted is that every manufacturer and manager should himself be an accomplished engineer and chemist, trained to observe, to reason, and to solve problems for himself.

In the case of chemistry this absence of sentiment in favour of concentration and thoroughness, and the demand for superficiality, if only it can be had wholesale, tells in a variety of ways. The governing bodies who control the various colleges and universities, and the public generally, cannot understand that good and useful work is being done unless it can be shown in the form of passes at examinations. Though I most firmly believe in the necessity for examinations, serious mischief begins when they are regarded as the end itself, and not as mere incidents in the student's career towards the end, which should be knowledge.

In respect to chemistry this is the disadvantage which attends the operation of such a system as that of the Science and Art Department, or of any system under which certificates in connection with individual subjects are granted on easy terms. Especial objection I also feel to such expressions as "advanced," used in reference to a particular stage, so commonly misunderstood as they are by the student and his friends, and operating against his further progress.

Reflect also upon the fact that there are only two or three colleges in this country which can boast of more than one professor of chemistry. In nearly all cases one man is called upon to discharge the duty of teaching classes both elementary and advanced, in pure and applied chemistry, inorganic and organic, theoretical and practical. This is a kind of thing which kills specialism, and without specialists we can have not only no advance, but no efficient teaching of more than rudiments.

That teachers ought to engage in research at all is by no means clear to the public and to those representatives of the public who are charged with the administration of these new institutions. This was illustrated very painfully a few years ago by the conditions under which professors were engaged at a certain college founded, according to the declaration of its promoters, "by the people for the people," wherein it was announced in round terms that original research was not wanted, as the college was "for the good of the many and not for the advantage of the few." This example of ignorance is only remarkable by reason of its audacity. Probably many people hold a similar view, though few are bold enough to declare it.

Without going far into the discussion of the general question, which is a large one, I may perhaps be allowed to offer a few remarks for the consideration of any of my audience who may perchance incline towards that opinion.

It is only when a teacher occupies himself with research that the most complete guarantee is given that he is interested in his subject and that he is a learner. A popular mistake consists in regarding a professor as a living embodiment of science—complete. infallible, mysterious; whereas in truth he is, or ought to be, only a senior student who devotes the greater part of his time to extending and consolidating his own knowledge for the benefit of those who come to learn of him, not only what lies within the boundaries of the known, but how to penetrate into the far greater region of the unknown. Moreover, the man who has no intellectual independence, and simply accepts other people's views without challenge, is pretty certain to make the stock of knowledge with which he sets out in life do service to the end. That one may be fitted to form a sound judgment concerning new theories he must be familiar with the methods by which progress is accomplished. The work of investigation then reacts beneficially upon the work of teaching; that is why teachers should be encouraged, nay even required, to investigate, and not because their discoveries may haply prove to be practically useful.

Of course it may be said that there have been distinguished investigators who could not teach, but the converse is not true; every teacher who has attained to eminence as a teacher, who has drawn men after him, who has founded a school of thought, and has left his mark upon his generation, has been an industrious worker in research of some kind. All teachers cannot be expected to reach the same high standard, but this is the ideal after which all must strive, or fail utterly.

The fact that there is as yet little demand among schoolmasters for high attainments in chemistry is another reason why so little is accomplished in the chemical schools. Here, again, the public is really to blame. It is disgraceful that in all classes of schools, even where chemistry is supposed to be taught, there are but few places where serious employment is found for the well-trained chemist. I could point to several schools which claim the position of first-rate, where chemistry is taught by masters who have never studied the subject at all, but who are, I suppose, allowed the traditional "ten minutes' start" with the book. Would the head masters of such places dare to employ a person to teach mathematics who did not know the four first rules of arithmetic, or another to teach Latin who had not even got through the accidence? I fancy not. This, however, is without exaggeration the exact parallel of the position in which chemistry is placed in the majority of schools. I have heard the excuse that there is a lack of competent teachers. Of course the demand and the supply will react upon each other. When you offer a reasonable stipend, reasonable accommodation for teaching effectively, reasonable leisure for the master's own studies, and a position on the staff not inferior to that of the classical and mathematical masters, I believe that then, but not till then, there will be as many good school teachers of chemistry as there are of other subjects.

I could point to other prominent schools where the chemistry and other branches of science are taught by a peripatetic South Kensington teacher, who arrives weekly with his box of tricks. Not long ago I was invited to distribute the prizes given in connection with the evening classes in a town not far from Birmingham, and I took the opportunity of advising the teachers present on the occasion to read. One of them said to me afterwards, "When do you suppose I can read? I am engaged in going round to my schools from nine in the morning till ten at night." People of this kind do the greater part of the so-called science teaching sustained by the Science and Art Department, and the worthy town councillors and committees who employ them think that these are the people who are going to help the British manufacturer in his struggle against foreign competition under the guidance of the highly-trained chemists from the German universities. This would be ludicrous if it were not so very serious.

There is an opportunity at the present time of correcting some of these mistakes, but no advantage is being taken of it. I refer now to the "technical schools" which are springing up everywhere. There may be a few competent teachers of chemistry employed in some of them, but I find it difficult to think of many examples. The sort of person who is put in charge of these places is usually a schoolmaster, who is allowed, sometimes even after his appointment, to get a short course of qualitative analysis in order to enable him to obtain a certificate which will entitle him to earn grants from the Science and Art Department.

And manufacturers are much to blame. Instead of employing trained chemists, the greater number of those who want chemical assistance are satisfied to engage the services of boys who have been to an evening class for a winter or two.

The difficulty of finding a satisfactory career in connection with the subject also accounts for the fact, which I fear must be admitted, that chemistry does not attract its due share of the intellect of the nation. Clever young men can usually do better at the law, in medicine, or in commerce, than in teaching chemistry or in manufactures in which chemical skill is applicable. So badly educated are many of the young men who commence the study with professional objects in view, that it is quite impossible to teach them anything beyond routine analysis, if so much.

I heard lately from a friend of mine a story of a young groom in his employ who cannot read or write; and who declines to be taught to read on the ground that, considering himself pretty smart, he is afraid that "learning might dull him." This idea seems to be rather prevalent among certain classes of people, but I can assure those who wish to be chemists that some familiarity with the rule of three, and such a command of English as will enable them to understand words of more than one syllable, will be no obstacle to the acquisition of chemical knowledge.

Three years has hitherto been regarded as the normal period of study. The question arises, can a young man, previously well educated, expect to become an accomplished chemist, competent to apply his knowledge usefully, by giving the whole of his time to study during *three years?* I believe not.

By reason of the enormous development of science the position of the student of chemistry is nowadays very different from what it was thirty years ago. Since that time we have not only got a few new elements, a matter of small importance in itself, but new views of the nature of the elements and of their mutual relations. This could hardly have come about but for the recognition of the law of Avogadro as a fundamental principle, upon which we rely as the ultimate criterion by which the true distinction between so-called equivalent weights and molecular ratios has been established. By the gradual evolution of ideas having reference successively to electro-chemical relations of elements and compounds, the theory of types, and atomicity or valency, we have arrived at notions of chemical constitution based upon the hypothesis of the orderly linking together of atoms. Thirty years ago isomerism had scarcely attracted notice, and carbon compounds were only just beginning to be arranged in homologous series. The general use at the present day of the language of the molecular kinetic theory shows how deeply this theory influences our ideas of the internal constitution of matter. Within the period referred to, dissociation has been studied and a vast body of thermo-chemical data have been accumulated. And although the larger portion of the results of this work still await interpretation, dynamical ideas of chemical action are now generally accepted. We have also new methods of investigation, including spectroscopic analysis with all its vast train of results.

When I began chemistry many of these subjects and others had not been heard of. Of course we had our difficulties, and I well remember the puzzles met with in the endeavour to refer compounds to their appropriate types, also the consternation caused in the student's mind and the confusion in his note-book by the successive changes in the atomic weights of carbon, oxygen, sulphur, and the metals. But on the whole there was much less to learn.

It has always been thought essential that a student of chemistry should have some knowledge of physics. It is now more than ever necessary that this knowledge should be extensive, sound, and based upon a good foundation of mathematics. Thirty years ago a hundred pages of Fownes contained all that was thought necessary, but no one nowadays could be satisfied with that. It is now asserted that a young chemist who expects to find a career in industrial chemistry should also have learnt drawing, and more important still that he should have a good general knowledge of mechanics, steam, and building construction. I suppose everyone will agree in adding French and especially German. You see how the requirements expand.

The inference from all this is that it now takes longer to make a chemist than formerly. This is a point of considerable practical importance.

My estimate that a well-educated and intelligent young man will now require five years for the study of chemistry and accessory subjects before he is likely to be of much use will not appear extravagant.

Here one may remark that in order to become a chemist it is before all things necessary to study chemistry. If the greater part of a student's time is to be taken up with other things, it is not very clear how this is to be done.

A reform all round is wanted. The mathematics, modern languages, and drawing properly belong to the antecedent school period, and I believe the Institute of Chemistry would greatly promote the interests of the profession if it would impose upon candidates for the Associateship not only a three years' course of training with an examination in practical chemistry at the end, but a severe examination in mathematics, in the English, French, and German languages, and perhaps drawing, before matriculation or registration.

A consideration of the present position of the student of chemistry leads naturally to a review of the methods of teaching the subject. Speaking broadly, I suppose nearly all professional chemists who have had the advantage of systematic training have, up to the present time, passed through very much the same kind of course. This consists, as everybody knows, very largely of analytical work, qualitative and quantitative, preceded or followed by the preparation of a number of definite chemical compounds, besides practice in certain very necessary physical determinations, *e.g.* relative density of solids, liquids, and gases, melting-points, boiling-points, and so forth. There seems now to be a disposition in some quarters to depart from this time-honoured curriculum in favour of a course in which the student is early engaged in some semblance of investigation, and in which he is encouraged to attack difficult problems, which from their fundamental importance offer considerable temptation. I venture to express a hope that this will not be carried too far. Already we are in danger of losing the art of accurate analysis. One constantly meets with young chemists who are ready enough to discuss the constitution of benzene, but who cannot make a reliable combustion. And, according to my own experience, attempts at research among inexperienced chemists become abortive more frequently in consequence of deficient analytical skill than from any other cause.

One modification I should gladly see generally adopted. I think an unnecessary amount of time is often spent upon qualitative mineral analysis, and an acquaintance with the properties of common and important carbon compounds ought to be acquired at an early stage. Quantitative work might with advantage be taken up sooner than usual. By that, however, I mean serious work, in which good methods are used and every effort made to secure accuracy. I do not believe in the use of rough methods because they are easy; the use of such leads the student to be satisfied with approximations, which, after all, he will learn soon enough are all that is possible to man. I am very glad to know that I have the support of one of my predecessors in this chair (Sir Henry Roscoe), whose opinion will carry far greater weight than mine, in deprecating premature efforts to engage students in research.[1]

But though it does not appear to me to be wise to encourage beginners, without sufficient experience or manipulative skill, to attempt original work, one of the best possible exercises preparatory to original work is to select suitable memoirs, and not only to read them but to work conscientiously through the whole of the preparations and analyses described, following the instructions given. Many of Dr. Hofmann's papers afford excellent examples. So also do the writings of Dr. Perkin and Dr. Frankland, besides those of many other chemists which could easily be selected by the teacher.

An intelligent student, possessing the requisite preliminary knowledge, would obtain much instruction by repeating the work contained in such papers as the following, for example:—Emerson Reynolds on the missing sulphur urea (J. Chem. Soc. 1869, i.); Fittig and Tollens on the synthesis of hydrocarbons of the benzol series (*Liebig's Annalen*, 1864, cxxxi. 303); L. Claisen and Pupils on the introduction of acid radicles into

[1] See Address to Section B, Montreal meeting.

ketones, &c. (*Berichte*, xx.); Lawson and Collie on the action of heat on salts of tetramethyl-ammonium (J. Chem. Soc., June 1888); Thorpe and Hambly on manganic trioxide (J. Chem. Soc., March 1888); besides many others, including papers on analytical processes. To such as these there might subsequently be added the determination of an atomic weight on the model of one of the best masters, as a discipline which could not fail to be impressive, and full of instruction.

When chemistry is taught, not with professional or technical objects in view, but for the sake of educational effects, as an ingredient in a liberal education, the primary object is to make the pupil observe and think. But with young students it is very important to proceed slowly, for chemistry is really a very difficult subject at first, owing to the variety of strange materials with uncouth names. To reason from particulars to generals is for the unpractised always a difficult process, and in chemistry this is specially the case. With young students it is, in my experience, preferable to adopt a somewhat dogmatic style, which should of course be exchanged for a more cautious one as the pupil proceeds.

Thus the law of Avogadro can only be given at first as a recognized physical law, without much explanation, since the full apprehension of the evidence upon which it rests can only be secured at a late stage of the learner's progress. There is of course great advantage in the use of an inductive method if only it is employed judiciously. Otherwise the result is only confusion.

A number of papers, pamphlets, and text-books have lately appeared, professing to teach the principles of the science practically and by new methods. Most of these turn out, upon inspection, to be very old methods indeed, but there is a small residue of distinctly original character which are sure to attract, as they deserve, considerable attention. The systems I refer to provide a series of problems which the pupils are called upon to solve. According to this plan the student is not allowed peaceably to examine the properties of oxygen or sulphur which he now sees for the first time. He must weigh, and measure, and observe, and then infer. All this coming at once upon the head of a beginner seems to me to be well fitted to drive him to despair.

I well remember the first experiment in chemistry I ever made. It consisted in dissolving zinc in diluted sulphuric acid in an evaporating dish, lighting with a match the bubbles of hydrogen as they rose, and afterwards leaving the solution to crystallize. I was about sixteen, and the bubbles of gas, as well as the crystals I afterwards got, interested me very much. If at that time I had been made to weigh the zinc and acid, and measure the hydrogen with the object of answering some question about the composition of zinc and hydrogen sulphates, I should have been pretty much in the position of a boy ignorant of geometry shut up with the propositions of Euclid and ordered to give the demonstrations.

I think when we recall such a fact as that Priestley, who discovered oxygen in 1774, failed to the end of his days to understand the process of combustion, and actually wrote, in 1800, a pamphlet in defence of "phlogiston," we ought not to be surprised when young people, though born a century later, fail to perceive at once the full significance of facts to which they are introduced for the first time. At the outset you cannot reasonably expect a young student both to observe accurately and infer justly. These two things must be kept separate at first, and for this reason among others I believe that attempts to make young students verify for themselves the fundamental propositions of chemistry will not be successful. One has only to trace the origin of one's own convictions in reference to any important fact or principle to perceive that they very seldom spring into existence suddenly, but almost always commence in vagueness and hesitation, acquiring consistency and solidity only as the result of accumulated experience.

I will not pretend to determine what may be included within the wide circle of the functions of the British Association; but I think I cannot be mistaken in assuming that the advancement of science is dependent in no small degree upon the provision for the efficient teaching of science. I have traced an outline of what has been done in the past, and have endeavoured to show in what respects I think we are deficient at the present time. No matter how ardent may be the aspirations, how earnest the endeavours of the few, progress will be slow unless they are sustained by the sympathy of the many. On one principle the public must surely insist, that only those shall be allowed to teach who know.

SECTION D.

BIOLOGY.

Opening Address by W. T. Thiselton-Dyer, C.M.G., M.A., B.Sc., F.R.S., F.L.S., President of the Section.

Before we commence the formal business of the Section, I propose to invite your attention to several points which have suggested themselves to me from a consideration of the present position and progress of the study of botany in this country.

It is not so very long ago that at English Universities, at least, the pursuit of botany was regarded rather as an elegant accomplishment than as a serious occupation. This is the more remarkable because at every critical point in the history of botanical science the names of our countrymen will be found to occupy an honourable place in the field of progress and discovery. In the seventeenth century, Hooke and Grew laid the foundation of the cell-theory, while Millington, by discovering the function of stamens, completed the theory of the flower. In the following century, Morison first raised ferns from spores, Lindsay detected the fern prothallus, Ray laid the foundations of a natural classification, Hales discovered root-pressure, and Priestley the absorption of carbon dioxide and the evolution of oxygen by plants. In the early part of the present one we have Knight's discovery of the true cause of geotropism, Daubeny's of the effect upon the processes of plant-life of rays of light of different refrangibility, and, finally, the first description of the cell-nucleus by R. Brown. I need not attempt to carry the list through the last half-century. I have singled out these discoveries as striking landmarks, the starting-points of important developments of the subject. It is enough for my purpose to show that we have always had an important school of botany in England, which has contributed at least its share to the general development of the science.

I think at the moment, however, we have little cause for anxiety. The academic chairs throughout the three kingdoms are filled, for the most part, with young, enthusiastic, and well-trained men. Botany is everywhere conceded its due position as the twin branch with zoology of biological science. We owe to the enlightened administration of the Oxford University Press the possession of a journal which allows of the prompt and adequate publication of the results of laboratory research. The excellent work which is being done in every part of the botanical field has received the warm sympathy of our colleagues abroad. I need only recall to your recollection, as a striking evidence of this, the remarkable gathering of foreign botanists which will ever make the meeting of this Association at Manchester a memorable event to all of us. The reflection rises sadly to the mind that it can never be repeated. Not many months, as you know, had passed before the two most prominent figures in that happy assemblage had been removed from us by the inexorable hand of death. In Asa Gray we miss a figure which we could never admit belonged wholly to the other side of the Atlantic. In technical botany we recognized him as altogether in harmony with the methods of work and standard of excellence of our own most distinguished taxonomists. But, apart from this, he had that power of grasping large and far-reaching ideas, which, I do not doubt, would have brought him distinction in any branch of science. We owe to him the classical discussion of the facts of plant distribution in the northern hemisphere which is one of the corner-stones of modern geographical botany. He was one of the earliest of distinguished naturalists who gave his adhesion to the theory of Mr. Darwin. A man of simple and sincere piety, the doctrine of descent never presented any difficulty to him. He will remain in our memories as a figure endowed with a sweetness and elevation of character which may be compared even with that of Mr. Darwin himself.

In De Bary we seem to have suffered no less a personal loss than in the case of Gray. Though, before last year, I do not know that he had ever been in England, so many of our botanists had worked under him that his influence was widely felt amongst us. And it may be said that this was almost equally so in every part of the civilized world. His position as a teacher was in this respect probably unique, and the traditions of his methods of work must permanently affect the progress of botany, and, indeed, have an even wider effect. This is not the occasion to dwell on each of his scientific achievements. It is sufficient to say that we owe to him the foundations of a rational vegetable pathology. He first grasped the true conditions of parasitism in plants, and not content with working out the complex phases of the life-history of the invading organism, he never lost sight of the conditions which permitted or inhibited its invasion. He treated the problem, whether on the side of the host or of the parasite, as a whole—as a biological problem, in fact, in the widest sense. It is this thorough grasp of the conditions of the problem that gives such a peculiar value to his last published book, the "Lectures on Bacteria," an admirable translation of which we owe to Prof. Balfour. To this I shall have again to refer. I must content myself with saying now, that in this and all his work there is that note of highest excellence which consists in lifting detail to the level of the widest generality. To a weak man this is a pitfall, in which a firm grasp of fact is lost in rash speculation. But when, as in De Bary's case, a true scientific insight is inspired by something akin to genius, the most fruitful conceptions are the result. Yet De Bary never sacrificed exactness to brilliancy, and to the inflexible love of truth which pervaded both his work and his personal intercourse we may trace the secret of the extraordinary influence which he exerted over his pupils.

As the head of one of the great national establishments of the country devoted to the cultivation of systematic botany, I need hardly apologize for devoting a few words to the present position of that branch of the science. Of its fundamental importance I have myself no manner of doubt. But as my judgment may seem in such a matter not wholly free from bias, I may fortify myself with an opinion which can hardly be minimized in that way. The distinguished chemist, Prof. Lothar Meyer, perhaps the most brilliant worker in the field of theoretical chemistry, finds himself, like the systematic botanist, obliged to defend the position of descriptive science. And he draws his strongest argument from biology. "The physiology of plants and animals," he tells us, "requires systematic botany and zoology, together with the anatomy of the two kingdoms: each speculative science requires a rich and well-ordered material, if it is not to lose itself in empty and fruitless fantasies." No one, of course, supposes that the accumulation of plant specimens in harbaria is the mere outcome of a passion for accumulating. But to do good systematic work requires high qualities of exactitude, patience, and judgment. As I had occasion to show at the Linnean centenary, the world is hardly sensible of the influence which the study of the subject has had on its affairs. The school of Jeremy Bentham has left an indelible mark on the social and legislative progress of our own time. Mill tells us that "the proper arrangement of a code of laws depends on the same scientific conditions as the classifications in natural history; nor could there," he adds, "be a better preparatory discipline for that important function than the principles of a natural arrangement, not only in the abstract, but in their actual application to the class of phenomena for which they were first elaborated, and which are still the best school for learning their use." He further tells us that of this Jeremy Bentham was perfectly aware, and that his "Fragment on Government" contains clear and just views on the meaning of a natural arrangement which reflect directly the influence of Linnæus and Jussieu. Mill himself possessed a competent knowledge of systematic botany, and therefore was well able to judge of its intellectual value. For my part, I do not doubt that precisely the same qualifications of mind which made Jeremy Bentham a great jurist, enabled his nephew to attain the eminence he reached as a botanist. As a mere matter of mental gymnastic, taxonomic science will hold its own with any pursuit. And, of course, what I say of botany is no less true of other branches of natural history. Mr. Darwin devoted eight or nine years to the systematic study of the *Cirripedia*. "No one," he himself tells us, "has a right to examine the question of species who has not minutely described many." And Mr. Huxley has pointed out, in the admirable memoir of Mr. Darwin which he has prepared for the Royal Society, that "the acquirement of an intimate and practical knowledge of the process of species-making . . ." was "of no less importance to the author of the 'Origin of Species' than was the bearing of the Cirripede work upon the principles of a natural classification."

At present the outlook for systematic botany is somewhat discouraging. France, Germany, and Austria no longer possess anything like a school in the subject, though they still supply able and distinguished workers. That these are, however, few, may be judged from the fact that it is difficult to fill the place of the lamented Eichler in the direction of the Botanic Garden and Herbarium at Berlin. Outside our own country, Switzerland is the most important seat of general systematic study, to which three generations of De Candolles have devoted themselves. The most active centres of work at the moment are, however, to be found in our own country, in the United States, and in Russia. And the reason is, in each case, no doubt the same. The enormous area of the earth's surface over which each country holds sway brings to them a vast amount of material which peremptorily demands discussion.

No country, however, affords such admirable facilities for work in systematic botany as are now to be found in London. The Linnean Society possesses the Herbarium of Linnæus; the Botanical Department of the British Museum is rich in the collections of the older botanists; while at Kew we have a constantly increasing assemblage of material, either the results of travel and expeditions, or the contributions of correspondents in different parts of the Empire. A very large proportion of this has been worked up. But I am painfully impressed with the fact that the total of our available workers bears but a small proportion to the labour ready to their hands.

This is the more a matter of concern, because for the few official posts which are open to botanists at home or abroad a practical knowledge of systematic botany is really indispensable. For suitable candidates for these one naturally looks to the Universities. And so far, I am sorry to say, in great measure one looks in vain. It would be, no doubt, a great impulse to what is undoubtedly an important branch of national scientific work if Fellowships could occasionally be given to men who showed some aptitude for it. But these should not be mere prizes for undergraduate study, but should exact some guarantee that during the tenure of the Fellowship the holder would seriously devote himself to some definite piece of work. At present, undoubtedly, the younger generation of botanists show a disposition to turn aside to those fields in which more brilliant and more immediate results can be attained. Their neglect of systematic botany brings to some extent its own Nemesis. A first principle of systematic botany is that a name should denote a definite and ascertainable species of plant. But in physiological literature you will find that the importance of this is entirely overlooked. Names are employed which are either not to be found in the books, or they are altogether misapplied. I call to mind the case of an English physiologist who wrote a highly ingenious paper on the movement of water in plants. He was content to refer to the plant upon which he experimented as the "bay-laurel." I ascertained that the plant he really used was the cherry-laurel. Now the bay is truly a laurel, while the cherry-laurel is a plum. Anyone repeating his experiments would therefore be led wholly astray. But if proper precautions are taken to ascertain the accurate botanical name of a plant, no botanist throughout the civilized world is at a loss to identify it.

But precision in nomenclature is only the necessary apparatus of the subject. The data of systematic botany, when properly discussed, lend themselves to very important generalizations. Perhaps those which are yielded by the study of geographical distribution are of the most general interest. The mantle of vegetation which covers the surface of the earth, if only we could rightly unravel its texture, would tell us a good deal about geological history. The study of geographical distribution, rightly handled, affords an independent line of attack upon the problem of the past distribution of land and sea. It would probably never afford sufficient data for a complete independent solution of the problem; but it must always be extremely useful as a check upon other methods. Here, however, we are embarrassed by the enormous amount of work which has yet to be accomplished. And unfortunately this is not of a kind which can be indefinitely postponed. The old terrestrial order is fast passing away before our eyes. Everywhere the primitive vegetation is disappearing as more and more of the earth's surface is brought into cultivation, or, at any rate, denuded of its forests.

A good deal, however, has been done. We owe to the indomitable industry of Mr. Bentham and of Sir Ferdinand Mueller a comprehensive flora of Australia, the first large area of the earth's surface of which the vegetation has been completely worked out. Sir Joseph Hooker, in his retirement, has pushed on within sight of completion the enormous work of describing so much of the vast Indo-Malayan flora as is comprised within the British possessions. To the Dutch botanists we owe a tolerably complete account of the Malayan flora proper. But New Guinea still remains botanically a *terra incognita*, and till within the last year or two the flora of China has been an absolute blank to us. A Committee of the British Association (whose report will be presented to you) has, with the aid of a small grant of money, taken in hand the task of gathering up the scanty data which are available in herbaria and elsewhere. This has stimulated European residents in China to collect more material, and the fine collections which are now being rapidly poured in upon us will, if they do not overwhelm us by their very magnitude, go a long way in supplying data for a tentative discussion of the relations of the Chinese flora to that of the rest of Asia. I do not doubt that this will in turn explain a good deal that is anomalous in the distribution of plants in India. The work of the Committee has been practically limited to Central and Eastern China. From the west, in Yunnan, the French botanists have received even more surprising collections, and these supplement our own work in the most fortunate manner. I have only to add, for Asia, Boissier's "Flora Orientalis," which practically includes the Mediterranean basin. But I must not omit the invaluable report of Brigade-Surgeon Aitchison on the collections made by him during the Afghan Delimitation Expedition. This has given an important insight into the vegetation of a region which had never previously been adequately examined. Nor must I forget the recent publication of the masterly report by Prof. Bayley Balfour on the plants collected by himself and Schweinfurth in Socotra, an island with which the ancient Egyptians traded, but the singularly anomalous flora of which was almost wholly unknown up to our time.

The flora of Africa has been at present but imperfectly worked up, but the materials have been so far discussed as to afford a tolerably correct theory of its relations. The harvest from Mr. Johnston's expedition to Kilimanjaro was not as rich as might have been hoped. Still, it was sufficient to confirm the conclusions at which Sir Joseph Hooker had arrived, on very slender data, as to the relations of the high-level vegetation of Africa generally. The flora of Madagascar is perhaps, at the moment, the most interesting problem which Africa presents to the botanist. As the rich collections, for which we are indebted to Mr. Baron and others, are gradually worked out, it can hardly be doubted that it will be necessary to modify in some respects the views which are generally received as to the relation of the island to the African continent. My colleague, Mr. Baker, communicated to the York meeting of the Association the results which, up to that time, he had arrived at, and these subsequent material has not led him to modify. The flora as a whole presents a large proportion of endemic genera and species, pointing to isolation from a very ancient date. The tropical element is, however, closely allied to that of Tropical Africa and of the Mascarene Islands, and there is a small infusion of Asiatic types which do not extend to Africa. The high-level flora, on the other hand, exhibits an even closer affinity with that temperate flora the ruins of which are scattered over the mountainous regions of Central Africa, and which survives in its greatest concentration at the Cape.

The American botanists at Harvard are still systematically carrying on the work of Torrey and Gray in the elaboration of the flora of Northern America. The Russians are, on their part, continually adding to our knowledge of the flora of Northern and Central Asia. The whole flora of the North Temperate Zone can only be regarded substantially as one. The identity diminishes southwards and increases in the case of the Arctic and Alpine regions. A collection of plants brought us from high levels in Corea by Mr. James might, as regards a large proportion of the species, have been gathered on one of our own Scotch hills.

We owe to the munificence of two English men of science the organization of an extensive examination of the flora and fauna of Central America and the publication of the results. The work, when completed, can hardly be less expensive that of the results of the *Challenger* voyage, which has severely taxed the liberality of the English Government. The problems which geographical distribution in this region presents will doubtless be found to be of a singularly complicated nature, and it is impossible to over-estimate the debt of gratitude which biologists of all countries must owe to Messrs. Godman and Salvin when their arduous undertaking is completed. I am happy to say that the botanical portion, which has been elaborated at Kew, is all but finished.

In South America, I must content myself with referring to the great "Flora Brasiliensis," commenced by Martius half a century ago, and still slowly progressing under the editorship of Prof. Urban, at Berlin. Little discussion has yet been attempted of the mass of material which is enshrined in the mighty array of volumes already published. But the travels of Mr. Ball in South America have led him to the detection of some very interesting problems. The enormous pluvial denudation of the ancient portions of the continent has led to the gradual blending of the flora of different levels with sufficient slowness to permit of adaptive changes in the process. The tropical flora of Brazil, therefore, presents an admixture of modified temperate types which gives to the whole a peculiar character not met with to the same degree in the tropics of the Old World. On the other hand, the comparatively recent elevation of the southern portion of the continent accounts, in Mr. Ball's eyes, for the singular poverty of its flora, which we may regard indeed as still in progress of development.

The botany of the *Challenger* Expedition, which was also elaborated at Kew, brought for the first time into one view all the available facts as to the floras of the older oceanic islands. To this was added a discussion of the origin of the more recent floras of the islands of the Western Pacific, based upon material carefully collected by Prof. Moseley, and supplemented by the notes and specimens accumulated with much judgment by Dr. Guppy. For the first time we were enabled to get some idea how a tropical island was furnished with plants, and to discriminate the littoral element due to the action of oceanic currents from the interior forest almost wholly due to frugivorous birds. The recent examination of Christmas Island by the English Admiralty has shown the process of island flora-making in another stage. The plants collected by Mr. Lister prove, as might be expected, to be closely allied to those of Java. But the effect of isolation has begun to tell; and I learn from my colleague, Prof. Oliver, that the plants from Christmas Island cannot be for the most part exactly matched with their congeners from Java, but yet do not differ sufficiently to be specifically distinguished. We have here, therefore, it appears to me, a manifest case of nascent species.

The central problem of systematic botany I have not as yet touched upon: this is to perfect a natural classification. Such a classification, to be perfect, must be the ultimate generalization of every scrap of knowledge which we can bring to bear upon the study of plant affinity. In the higher plants experience has shown that we can obtain results which are sufficiently accurate for the present without carrying our structural analysis very far. Yet even here, the correct relations of the Gymnosperms would never have been ascertained without patient and minute microscopic study of the reproductive processes. Upon these, indeed, the correct classification of the Vascular Cryptogams wholly depends, and generally, as we descend in the scale, external morphology becomes more and more insecure as a guide, and a thorough knowledge of the minute structure and life-history of each organism becomes indispensable to anything like a correct determination of its taxonomic position. The marvellous theory of the true nature of lichens would never have been ascertained by the ordinary methods of examination which were held to be sufficient by lichenologists.

The final form of every natural classification—for I have no doubt that the general principles I have laid down are equally true in the field of zoology—must be to approximate to the order of descent. For the theory of descent became an irresistible induction as soon as the idea of a natural classification had been firmly grasped.

In regard to flowering plants we owe, as I have said, the first step in a natural classification to our own great naturalist, John Ray, who divided them into Monocotyledons and Dicotyledons. The celebrated classification of Linnæus was avowedly purely artificial. It was a temporary expedient, the provisional character of which no one realized more thoroughly than himself. He, in fact, himself gave us one of the earliest outlines of a truly natural system. Such a system is based on affinity, and we know of no other explanation of affinity than that which is implied in the word—namely, common parentage. No one finds any difficulty in admitting that, where a number of individual organisms closely resemble one another, they must have been derived from the same stock. I allow that, in cases where external form is widely different, the conclusion to one who is not a naturalist is by no means so obvious. But in such cases it rests on the profound and constant resemblance of internal points of structure. Anyone who studies the matter with a perfectly open mind finds it impossible to draw a line. If genetic relationship or heredity is admitted to be the explanation of affinity in the most obvious case, the stages are imperceptible by which the same conclusion is seen to be inevitable when the evidence is fairly examined, even in cases where at the first glance it seems least likely.

This leads me to touch on the great theory which we owe to Mr. Darwin. That theory, I need hardly say, was not merely a theory of descent. This had suggested itself to naturalists in the way I have indicated long before. What Mr. Darwin did was to show how by perfectly natural causes the separation of living organisms into races which at once resemble and yet differ from one another so profoundly came about. Heredity explains the resemblance; Mr. Darwin's great discovery was that variation worked upon by natural selection explained the difference. That explanation seems to me to gather strength every day, and to continually reveal itself as a more and more efficient solvent of the problems which present themselves to the student of natural history. At the same time, I am far from claiming for it the authority of a scientific creed or even the degree of certainty which is possessed by some of the laws of astronomy. I only affirm that as a theory it has proved itself a potent and invaluable instrument of research. It is an immensely valuable induction; but it has not yet reached such a position of certitude as has been attained by the law of gravitation; and I have myself, in the field of botany, felt bound to protest against conclusions being drawn deductively from it without being subjected to the test of experimental verification. This attitude of mine, which I believe I share with most naturalists, must not, however, be mistaken for one of doubt. Of doubt as to the validity of Mr. Darwin's views I have none: I shall continue to have none till I come across facts which suggest doubt. But that is a different position from one of absolute certitude.

It is therefore without any dissatisfaction that I observe that many competent persons have, while accepting Mr. Darwin's theory, set themselves to criticize various parts of it. But I must confess that I am disposed to share the opinion expressed by Mr. Huxley, that these criticisms really rest on a want of a thorough comprehension.

Mr. Romanes has put forward a view which deserves the attention due to the speculations of a man of singular subtlety and dialectic skill. He has startled us with the paradox that Mr. Darwin did not, after all, put forth, as I conceive it was his own impression he did, a theory of the origin of species, but only of adaptations. And inasmuch as Mr. Romanes is of opinion that specific differences are not adaptive, while those of genera are, it follows that Mr. Darwin only really accounted for the origin of the latter, while for an explanation of the former we must look to Mr. Romanes himself. For my part, however, I am altogether unable to accept the premises, and therefore fail to reach the conclusion. Specific differences, as we find them in plants, are for the most part indubitably adaptive, while the distinctive characters of genera and of higher groups are rarely so. Let anyone take the numerous species of some well-characterized English genus—for example, *Ranunculus*; he will find that one species is distinguished by having creeping stems, one by a tuberous root, one by floating leaves, another by drawn-out submerged ones, and so on. But each possesses those common characters which enables the botanist almost at a glance, notwithstanding the adaptive disguise, to refer them to the common genus *Ranunculus*. It seems to me quite easy to see, in fact, why specific characters should be usually adaptive, and generic not so. Species of any large genus must, from the nature of things, find themselves exposed to anything rather than uniform conditions. They must acquire, therefore, as the very condition of their existence, those adaptive characters which the necessities of their life demand. But this rarely affects those marks of affinity which still indicate their original common origin. No doubt these were themselves once adaptive, but they have long been overlaid by newer and more urgent modifications. Still, Nature is ever conservative, and these reminiscences of a bygone history persist; significant to the systematic botanist as telling an unmistakable family story, but far removed from the stress of a struggle in which they no longer are called upon to bear their part.

Another episode in the Darwinian theory is, however, likely to occupy our attention for some time to come. The biological world now looks to Prof. Weismann as occupying the most prominent position in the field of speculation. His theory of the continuity of the germ-plasm has been put before English readers with extreme lucidity by Prof. Moseley. That theory, I am free to confess, I do not find it easy to grasp clearly in all its concrete details. At any rate, my own studies do not furnish me with sufficient data for criticizing them in any adequate way. It is, however, bound up with another theory—the non-inheritance of acquired characters—which is more open to general discussion. If with Weismann we accept this principle, it cannot be doubted that the burden thrown on natural selection is enormously increased. But I do not see that the theory of natural selection itself is in any way impaired in consequence.

The question, however, is, Are we to accept the principle? It appears to me that it is entirely a matter of evidence. It is proverbially difficult to prove a negative. In the analogous case of the inheritance of accidental mutilations, Mr. Darwin contents himself with observing that we should be "cautious in denying it." Still, I believe that, though a great deal of pains has been devoted to the matter, there is no case in which it has been satisfactorily proved that a character acquired by an organism has been transmitted to its descendants; and there is, of course, an enormous bulk of evidence the other way.

The consideration of this point has given rise to what has been called the new Lamarckism. Now, Lamarck accounted for the evolution of organic Nature by two principles—the tendency to progressive advancement and the force of external circumstances. The first of these principles appears to me, like Nägeli's internal modifying force, to be simply substituting a a name for a thing. Lamarck, like many other people before him, thought that the higher organisms were derived from others lower in the scale, and he explained this by saying that they had a tendency to be so derived. This appears to me much as if we explained the movement of a train from London to Bath by attributing it to a tendency to locomotion. Mr. Darwin lifted the whole matter out of the field of mere transcendental speculation by the theory of natural selection, a perfectly intelligible mechanism by which the result might be brought about. Science will always prefer a material *modus operandi* to anything so vague as the action of a tendency.

Lamarck's second principle deserves much more serious consideration. To be perfectly fair, we must strip it of the crude illustrations with which he hampered it. To suggest that a bird became web-footed by persistently stretching the skin between its toes, or that the neck of a giraffe was elongated in the perpetual attempt to reach the foliage of trees, seems almost repugnant to common-sense. But the idea that changes in climate and food—*i.e.* in the conditions of nutrition generally—may have some slow but direct influence on the organism seems, on a superficial view, so plausible, that the mind is very prone to accept it. Mr. Darwin has himself frankly admitted that he thought he had not attached sufficient weight to the direct action of the environment. Yet it is extremely difficult to obtain satisfactory evidence of effects produced in this way. Hoffmann experimented with much pains on plants, and the results were negative. And Mr. Darwin confessed that Hoffmann's paper had "staggered" him.

Organic evolution still, therefore, seems to me to be explained in the simplest way as the result of variation controlled by natural selection. Now, both these factors are perfectly intelligible things. Variation is a mere matter of every-day observation, and the struggle for existence, which is the cause of which natural selection is the effect, is equally so. If we state in a parallel form the Lamarckian theory, it amounts to a tendency controlled by external forces. It appears to me that there is no satisfactory basis of fact for either factor. The practical superiority of the Darwinian over the Lamarckian theory is, as a working hypothesis, immeasurable.

The new Lamarckian school, if I understand their views correctly, seek to re-introduce Lamarck's "tendency." The fact has been admitted by Mr. Darwin himself that variation is not illimitable. No one, in fact, has ever contended that any type can be reached from any point. For example, as Weismann puts it, "Under the most favourable circumstances, a bird can never become transformed into a mammal." It is deduced from this that variation takes place in a fixed direction only, and this is assumed to be due to an innate law of development, or, as Weismann has termed it, a "phyletic vital force." But the introduction of any such directive agency is superfluous, because the limitation of variability is a necessary consequence of the physical constitution of the varying organism.

It is supposed, however, by many people that a necessary part of Mr. Darwin's theory is the explanation of the phenomenon of variation itself. But really this is not more reasonable than to demand that it should explain gravitation or the source of solar energy. The investigation of any one of these phenomena is a matter of first-rate importance. But the cause of variation is perfectly independent of the results that flow from it when subordinated to natural selection.

Though it is difficult to establish the fact that external causes promote variation directly, it is worth considering whether they may not do so indirectly. Weismann, like Lamarck before him, has pointed out, as others have also done, the remarkable persistence of the plants and animals of Egypt; and the evidence of this is now even stronger. We, at Kew, owe to the kindness of Dr. Schweinfurth, a collection of specimens of plants from Egyptian tombs, which are said to be as much as 4000 years old. They are still perfectly identifiable, and, as one of my predecessors in this chair has pointed out, they differ in no respect from their living representatives in Egypt at this day. The explanation which Lamarck gave of this fact "may well," says Sir Charles Lyell, "lay claim to our admiration." He attributed it, in effect, to the persistence of the physical geography, temperature, and other natural conditions. The explanation seems to me adequate. The plants and animals, we may fairly assume, were, 4000 years ago, as accurately adjusted to the conditions in which they then existed as the fact of their persistence in the country shows that they must be now. Any deviation from the type that existed then would either, therefore, be disadvantageous or indifferent. In the former case it would be speedily eliminated, in the latter it would be swamped by cross-breeding. But we know that if seeds of these plants were introduced into our gardens we should soon detect varieties amongst their progeny. Long observation upon plants under cultivation has always disposed me to think that a change of external conditions actually stimulated variation, and so gave natural selection wider play and a better chance of re-establishing the adaptation of the organism to them. Weismann explains the remarkable fact that organisms may for thousands of years reproduce themselves unchanged by the principle of the persistence of the germ-plasm. Yet it seems hard to believe that the germ-plasm, while enshrined in the individual whose race it is to perpetuate, and nourished at its expense, can be wholly indifferent to all its fortunes. It may be so, but in that case it would be very unlike other living elements of organized beings.

I am bound, however, to confess that I am not wholly satisfied with the data for the discussion of this question which practical horticulture supplies. That the contents of our gardens do exhibit the re-ults of variation in a most astonishing degree no one will dispute. But for scientific purposes any exact account of the treatment under which these variations have occurred is unfortunately usually wanting. A great deal of the most striking variation is undoubtedly due to wide crossing, and these cases must, of course, be eliminated when the object is to test the independent variation of the germ-plasm. Hoffmann, whose experiments I have already referred to, doubts whether plants do as a matter of fact vary more under cultivation than in their native home and under natural conditions. It would be very interesting if this could be tested by the concerted efforts of two cultivators, say, for example, in Egypt and in England. Let some annual plant be selected, native of the former country, and let its seed be transmitted to the latter. Then let each cultivator select any variations that arise in regard to some given character; set to work, in fact, exactly as any gardener would who wanted to "improve" the plant, but on a preconcerted plan. A comparison of the success which each obtained would be a measure of the effect of the change of the environment on variability. If it proved that, as Hoffmann supposed, the change of conditions did not affect what we may call the rate of variation, then, as Mr. Darwin remarks in writing to Prof. Semper, "the astonishing variations of almost all cultivated plants must be due to selection and breeding from the varying individuals. This idea," he continues, "crossed my mind many years ago, but I was afraid to publish it, as I thought that people would say, 'How he does exaggerate the importance of selection.'" From an independent consideration of the subject I also find my mind somewhat shaken about it. Yet I feel disposed to say with Mr. Darwin, "I still *must* believe that changed conditions give the impulse to variability, but that they act *in most cases* in a very indirect manner."

Whatever conclusions we arrive at on these points, everyone will agree that one result of the Darwinian theory has been to give a great impulse to the study of organisms, if I may say so, as "going concerns." Interesting as are the problems which the structure, the functions, the affinity, or the geographical distribution of a plant may afford, the living plant in itself is even more interesting still.

Every organ will bear interrogation to trace the meaning and origin of its form and the part it plays in the plant's economy. That there is here an immense field for investigation there can be no doubt. Mr. Darwin himself set us the example in a series of masterly investigations. But the field is well-nigh inexhaustible. The extraordinary variety of form which plants exhibit has led to the notion that much of it may have arisen from indifferent variation. No doubt, as Mr. Darwin has pointed out, when one of a group of structures held together by some morphological or physiological *nexus* varies, the rest will vary correlatively. One variation then may, if advantageous, become adaptive, while the rest will be indifferent. But it appears to me that such a principle should be applied with the greatest caution; and from what I have myself heard fall from Mr. Darwin, I am led to believe that in the later years of his life he was disposed to think that every detail of plant structure had some adaptive significance, if only the clue could be found to it. As regards the forms of flowers an enormous body of information has been collected, but the vegetative organs have not yielded their secret to anything like the same extent. My own impression is that they will be found to be adaptive in innumerable ways which at present are not even suspected. At Kew we have probably a larger number of species assembled together than are to be found anywhere on the earth's surface. Here, then, is ample material for observation and comparison. But the adaptive significance will doubtless often be found by no means to lie on the surface. Who, for example, could possibly have guessed by inspection the purpose of the glandular bodies on the leaves of *Acacia sphærocephala* and on the pulvinus of *Cecropia peltata* which Belt in the one case, and Fritz Müller in the other, have shown to serve as food for ants? So far from this explanation being far-fetched, Belt found that the former "tree is actually unable to exist without its guard," which it could not secure without some attraction in the shape of food. One fact which strongly impresses me with a belief in the adaptive significance of vegetative characters is the fact that they are constantly adopted in almost identical forms by plants of widely different affinity. If such forms were without significance one would expect them to be infinitely varied. If, however, they are really adaptive, it is intelligible that different plants should independently avail themselves of identical appliances and expedients.

Although this country is splendidly equipped with appliances for the study of systematic botany, our Universities and Colleges fall far behind a standard which would be considered even tolerable on the Continent in the means of studying morphological and physiological botany or of making researches in these subjects. There is not at the moment anywhere in London an adequate botanical laboratory, and though at most of the Universities matters are not quite so bad, still I am not aware of any one where it is possible to do more than give the routine instruction, or to allow the students, when they have passed through this, to work for themselves. It is not easy to see why this should be, because on the animal side the accommodation and appliances for teaching comparative anatomy and physiology are always adequate and often palatial. Still less explicable to me is the tendency on the part of those who have charge of medical education to eliminate botanical study from the medical curriculum, since historically the animal histologists owe everything to botanists. In the seventeenth century, as I have already mentioned, Hooke first brought the microscope to the investigation of organic structure, and the tissue he examined was cork. Somewhat later, Grew, in his "Anatomy of Plants," gave the first germ of the cell-theory. During the eighteenth century the anatomists were not merely on a hopelessly wrong tack themselves, but they were bent on dragging botanists into it also. It was not till 1837, a little more than fifty years ago, that Henle saw that the structure of epithelium was practically the same as that of the *parenchyma plantarum* which Grew had described 150 years before. Two years later Schwann published his immortal theory, which comprised the ultimate facts of plant and animal anatomy under one view. But it was to a botanist, Von Mohl, that, in 1846, the biological world owed the first clear description of protoplasm, and to another botanist, Cohn (1851), the identification of this with the sarcode of zoologists.

Now the historic order in discovery is not without its significance. The path which the first investigators found most accessible is doubtless that which beginners will also find easiest to tread. I do not myself believe that any better access can be obtained to the structure and functions of living tissues than by the study of plants. However, I am not without hopes that the serious study of botany in the laboratory will be in time better cared for. I do not hesitate to claim for it a position of the greatest importance in ordinary scientific education. All the essential phenomena of living organisms can be readily demonstrated upon plants. The necessary appliances are not so costly, and the work of the class room is free from many difficulties with which the student of the animal side of biology has to contend.

Those, however, who have seriously devoted themselves to the pursuit of either morphological or physiological botany need not now be wholly at a loss. The splendid laboratory on Plymouth Sound, the erection of which we owe to the energy and enthusiasm of Prof. Ray Lankester, is open to botanists as well as to zoologists, and affords every opportunity for the investigation of marine plants, in which little of late years has been done in this country. At Kew we owe to private munificence a commodious laboratory in which much excellent work has already been done. And this Association has made a small grant in aid of the establishment of a laboratory in the Royal Botanic Garden at Peradeniya, in Ceylon. It may be hoped that this will afford facilities for work of the same kind as has yielded Dr. Treub such a rich harvest of results in the Buitenzorg Botanic Garden in Java.

Physiological botany, as I have already pointed out, is a field in which this country in the past has accomplished great things. It has not of late, however, obtained an amount of attention in any way proportionate to that devoted to animal physiology. In the interests of physiological science generally, this is much to be deplored; and I believe that no one was more firmly convinced of this than Mr. Darwin. Only a short time before his death, in writing to Mr. Romanes on a book that he had recently been reading, he said that the author had made "a gigantic oversight in never considering plants: these would simplify the problem for him." This goes to the root of the matter. There is, in my judgment, no fundamental biological problem which is not exhibited in a simpler form by plants than animals. It is possible, however, that the distaste which seems to exist amongst our biologists for physiological botany may be due in some measure to the extremely physical point of view from which it has been customary to treat it on the Continent. It is owing in great measure to the method of Mr. Darwin's own admirable researches that in this country we have been led to a more excellent way. The work which has been lately done in England seems to me full of the highest promise. Mr. Francis Darwin and Mr. Gardiner have each in different directions shown the entirely new point of view which may be obtained by treating plant phenomena as the outcome of the functional activity of protoplasm. I have not the least doubt that by pursuing this path English research will not merely place vegetable physiology, which has hitherto been too much under the influence of Lamarckism, on a more rational basis, but that it will also sensibly react, as it has done often before, on animal physiology.

There is no part of the field of physiological botany which has yielded results of more interest and importance than that which relates to the action of ferments and fermentation; and I could hardly give you a better illustration of the purely biological method of treating it. I believe that these results, wonderful and fascinating as they are, afford but a faint indication of the range of those that are still to be accomplished. The subject is one of extreme intricacy, and it is not easy to speak about it briefly. To begin with, it embodies two distinct groups of phenomena which have in reality very little which is essential in common.

What are usually called ferments are perhaps the most remarkable of all chemical bodies, for they have the power of effecting very profound changes in the chemical constitution of other substances, although they may be present in very minute quantity; but, and this is their most singular and characteristic property, they themselves remain unchanged in the process. It may be said without hesitation that the whole nutrition of both animals and plants depends on the action of ferments. Organisms are incapable of using solid nutrient matter for the repair and extension of their tissues; this must be first brought into a soluble form before it can be made available, and this change is generally brought about by the action of a ferment. Animal physiology has long been familiar with the part played by ferments, and it may be said that no small part of the animal economy is made up of organs required either for the manufacture of ferments or for the exposure of ingested food to their action. It may seem strange at first sight to speak of analogous processes taking place in plants. But it must be remembered that plant nutrition includes two very distinct stages. Certain parts of plants build up, as everyone knows, from external inorganic materials substances which are available for the construction of new tissues. It might be supposed that these are used up as fast as they are formed. But it is not so; the life of the plant is not a continuous balance of income and expenditure. On the contrary, besides the general maintenance of its structure, the plant has to provide from time to time for enormous resources to meet such exhausting demands as the renewal of foliage, the production of flowers, and the subsequent maturing of fruit.

In such cases the plant has to draw on an accumulated store of solid food which has rapidly to be converted into the soluble form in which alone it is capable of passing through the tissues to the seat of consumption. And I do not doubt for my part that in such cases ferments are brought into play of the same kind and in the same way as in the animal economy. Take such a simple case as a potato-tuber. This is a mass of cellular tissue, the cells of which are loaded with starch. We may either dig up the tuber and eat the starch ourselves, or we may leave it in the ground, in which case it will be consumed in providing material for the growth of a potato-plant next year. But the processes by which the insoluble starch is made available for nutrition are, I cannot doubt, closely similar in either case.

When we inquire further about these mysterious and all-important bodies, the answer we can give is extremely inadequate. It is very difficult to obtain them in amount sufficient for analysis, or in a state of purity. We know, however, that they are closely allied to albuminoids, and contain nitrogen in varying proportion. Papain, which is a vegetable ferment derived from the fruit of the papaw, and capable of digesting most animal albuminoids, is said to have the same ultimate composition as the pancreatic ferment and as peptones, bodies closely allied to proteids; the properties of all three bodies are, however, very different. It seems clear, nevertheless, that ferments must be closely allied to proteids, and, like these bodies, they are, no doubt, directly derived from protoplasm.

I need not remind you that, unlike other constituents of plant tissues, protoplasm, as a condition of its vitality, is in a constant state of molecular activity. The maintenance of this activity involves the supply of energy, and this is partly derived from the waste of its own substance. This "self-decomposition" of the protoplasm liberates energy, and in doing so gives rise to a number of more stable bodies than protoplasm. Some of these are used up again in nutrition; others are thrown aside, and are never drawn again into the inner circle of vital processes. In the animal organism, where the strictest economy of bulk is a paramount necessity, they are promptly got rid of by the process of excretion. In the vegetable economy these residual products usually remain. And it is for this reason, I may point out, that the study of the chemistry of plant nutrition appears to me of such immense importance. The record of chemical change is so much more carefully preserved; and the probability of our being able to trace the course it has followed is consequently far more likely to be attended with success.

This preservation in the plant of the residual by-products of protoplasmic activity no doubt accounts for the circumstance which otherwise is extremely perplexing—the profusion of substances which we meet with in the vegetable kingdom to which it is hard to attribute any useful purpose. It seems probable that ferments, in a great many cases, belong to the same category. I imagine that it is in some degree accidental that some of them have been made use of, and thus the plant has been able to temporarily lock up accumulations of food to be drawn upon in future phases of its life with the certainty that they would be available. Without the ferments the key of the storehouse would be lost irretrievably.

Plants, moreover, are now known to possess ferments, and the number will doubtless increase, to which it is difficult to attribute any useful function. Papain, to which I have already alluded, abounds in the papaw, but it is not easy to assign to it any definite function; still less is it easy, on teleological grounds, to account for the rennet ferment contained in the fruits of an Indian plant, *Withania coagulans*, and admirably investigated by Mr. Sheridan Lea.

Having dwelt so far on the action of ferments, we may now turn to fermentation, and that other kind of change in organic matter called "putrefaction," which is known to be closely allied to fermentation. Ferments and fermentation, as I have already remarked, have very little to do with one another; and it would save confusion and emphasize the fact if we ceased to speak of ferments but used some of the alternative names which have been proposed for them, such as *zymases* or *enzymes*.

The classical case of fermentation, which is the root of our whole knowledge of the subject, is that of the conversion of sugar into alcohol. Its discovery has everywhere accompanied the first stages of civilization in the human race. Its details are now taught in our text-books; and I should hardly hope to be excused for referring to it in any detail if it were not necessary for my purpose to draw your attention more particularly to one or two points connected with it.

Let us trace what happens in a fermenting liquid. It becomes turbid, it froths and effervesces, the temperature sensibly increases; this is the first stage. After this it begins to clear, the turbidity subsides as a sediment; the sugar which the fluid at first contained has in great part disappeared, and a new ingredient, alcohol, is found in its place.

It is just fifty years ago that the great Dutch biologist Schwann made a series of investigations which incontrovertibly demonstrated that both fermentation and putrefaction were due to the presence of minute organisms which live and propagate at the expense of the liquids in which they produce as a result these extraordinary changes. The labours of Pasteur have confirmed Schwann's results, and—what could not have been foreseen—have extended the possibilities of this field of investigation to those disturbances in the vital phenomena of living organisms themselves which we include under the name of "disease," and which, no one will dispute, are matters of the deepest concern to every one of us.

Now, at first sight, the conversion of starch into sugar by means of diastase seems strikingly analogous to the conversion of sugar into alcohol. It is for this reason that the phenomena have been so long associated. But it is easy to show that they are strikingly different. Diastase is a chemical substance of obscure composition it is true, but inert and destitute of any vital properties, nor is it affected by the changes it induces. Yeast, on the other hand, which is the active agent in alcoholic fermentation, is a definite organism; it enormously increases during the process, and it appears to me impossible to resist the conclusion that fermentation is a necessary concomitant of the peculiar conditions of its life. Let me give you a few facts which go to prove this. In the first place, you cannot ferment a perfectly pure solution of sugar. The fermentable fluid must contain saline and nitrogenous matters necessary for the nutrition of the yeast protoplasm. In pure sugar the yeast starves. Next, Schwann found that known protoplasmic poisons, by killing the yeast-cells, would prohibit fermentation. He found the same result to hold good of putrefaction, and this is the basis of the whole theory of antiseptics. Nor can the action of yeast be attributed to any ferment which the yeast secretes. It is true that pure cane-sugar cannot be fermented, and that yeast effects the inversion of this, as it is called, into glucose and lævulose. It does this by a ferment which can be extracted from it, and which is often present in plants. But you can extract nothing from yeast which will do its peculiar work apart from itself. Helmholtz made the crucial experiment of suspending a bladder full of boiled grape-juice in a vat of fermenting must; it underwent no change; and even a film of blotting-paper has been found a sufficient obstacle to its action. We are driven, then, necessarily to the conclusion that in the action of "ferments" or zymases we have to do with a chemical—*i.e.* a purely physical process; while in the case of yeast we encounter a purely physiological one.

How, then, is this action to be explained? Pasteur has laid stress on a fact which had some time been known, that the production of alcohol from sugar is a result of which yeast has not the monopoly. If ripening fruits, such as plums, are kept in an atmosphere free from oxygen, Bérard found that they, too, exhibit this remarkable transformation; their sugar is converted appreciably into alcohol. On the other hand, Pasteur has shown that, if yeast is abundantly supplied with oxygen, it feeds on the sugar of a fermentable fluid without producing alcohol. But, under the ordinary circumstances of fermentation, its access to oxygen is practically cut off; the yeast, then, is in exactly the same predicament as the fruit in Bérard's experiment. Sugar is broken up into carbon dioxide and alcohol in an amount far in excess of the needs of mere nutrition. In this dissociation it can be shown that an amount of energy is set free in the form of heat equal to about one-tenth of what would be produced by the total combustion of an equivalent amount of grape-sugar. If the protoplasm of the yeast could, with the aid of atmospheric oxygen, completely decompose a unit of grape-sugar, it would get ten times as much energy in the shape of heat as it could get by breaking it up into alcohol and carbon dioxide. It follows, then, that to do the same amount of growth in either case, it must break up ten times as much sugar without a supply of oxygen as with it. And this throws light on what has always been one of the most remarkable facts about fermentation—the enormous amount of change which the yeast manages to effect in proportion to its own development.

There are still two points about yeast which deserve attention before we dismiss it. When a fermenting liquid comes to contain about 14 per cent. of alcohol, the activity of the yeast ceases, quite independently of whether the sugar is used up or not. In other cases of fermentation the same inhibiting effect of the products of fermentation is met with. Thus, lactic fermentation soon comes to an end unless calcium carbonate or some similar substance be added, which removes the lactic acid from the solution as fast as it is formed.

The other point is that in all fermentations, besides what may be termed the primary products of the process, other bodies are produced. In the case of alcoholic fermentation the primary bodies are alcohol and carbon dioxide; the secondary, succinic acid and glycerine. Delpino has suggested that these last are residual products derived from that portion of the fermentable matter which is directly applied to the nutrition of the protoplasm.

Yeast, itself the organism which effects the remarkable changes on which I have dwelt, is somewhat of a problem. It is clear that it is a fungus, the germs of which must be ubiquitous in the atmosphere. It is difficult to believe that the simple facts, which are all we know about it, constitute its entire life-history. It is probably a transitory stage of some more complicated organism.

I can only briefly refer to putrefaction. This is a far more complex process than that which I have traced in the case of alcoholic fermentation. In that, nitrogen is absent, while it is an essential ingredient in albuminoids, which are the substances which undergo putrefactive changes. But the general principles are the same. Here, too, we owe to Schwann the demonstration of the fact that the effective agents in the process are living organisms. If we put into a flask a putrescible liquid such as broth, boil it for some time, and during the process of boiling plug the mouth with some cotton-wool, we know that the broth will remain long unchanged, while if we remove the wool putrescence soon begins. Tyndall has shown that, if we conduct the experiment on one of the high glaciers of the Alps, the cotton-wool may be dispensed with. We may infer, then, that the germs of the organisms which produce putrefaction are abundant in the lower levels of the atmosphere and are absent from the higher. They are wafted about by currents of air; but they are not imponderable, and in still air they gradually subside. Dr. Lodge has shown that air is rapidly cleared of suspended dust by an electric discharge, and this, no doubt, affords a simple explanation of the popular belief that thunderous weather is favourable to putrefactive changes.

Cohn believes that putrefaction is due to due to an organism called *Bacterium termo*, which plays in it the same part that yeast does in fermentation. This is probably too simple a statement; but the general phenomena are nevertheless similar. There is the same breaking down of complex into simpler molecules; the same evolution of gas, especially carbon dioxide; the same rise of temperature. The more or less stable products of the process are infinitely more varied, and it is difficult, if not impossible, to say, in the present state of our knowledge, whether in most cases they are the direct outcome of the putrefactive process, or residual products of the protoplasmic activity of the organisms which induce it. Perhaps, on the analogy of the higher plants, in which some of them also occur, we may attribute to the latter category certain bodies closely resembling vegetable alkaloids; these are called ptomaines, and are extremely poisonous. Besides such bodies, Bacteria undoubtedly generate true ferments and peculiar colouring-matters. But there are in most cases of putrefaction a profusion of other substances, which represent the various stages of the breaking up of the complex proteid molecule, and are often themselves the outcome of subsidiary fermentations.

These results are of great interest from a scientific point of view. But their importance at the present moment in the study of certain kinds of disease can hardly be exaggerated. I have already mentioned Henle as having first found the true clue to animal histology in the structure of plants. As early as 1840 the same observer indicated the grounds for regarding contagious diseases as due to living organisms. I will state his argument in the words of De Bary, whose "Lectures on Bacteria," the last work which we owe to his gifted hand, I can confidently recommend to you as a luminous but critical discussion of a vast mass of difficult and conflicting literature.

It was, of course, clear that contagion must be due to the communication of infectious particles or contagia. These contagia, although at the time no one had seen them, Henle pointed out, "have the power, possessed, as far as we know, by living creatures only, of growing under favourable conditions, and of multiplying at the expense of some other substance than their own, and therefore of assimilating that substance." Henle enforced his view by comparison with the theory of fermentation, which had then been promulgated by Schwann. But for many years his views found no favour. Botanists, however, as in so many other cases, struck on the right path, and from about the year 1850 steady progress, in which De Bary himself took a leading part, was made in showing that most of the diseases of plants are due to parasitic infection. The reason of this success was obvious: the structure of plants makes them more accessible to research, and the invading parasites are larger than animal contagia. On the animal side all real progress dates from about 1860, when Pasteur, having established Schwann's theory of fermentation on an impregnable basis, took up Henle's theory of living contagia.

The only risk now is that we may get on too fast. To put the true theory of any one contagious disease on as firm a basis as that of alcoholic fermentation is no easy matter to accomplish. But I believe that this is, notwithstanding a flood of facile speculation and imperfect research, slowly being done.

There are two tracts in the body which are obviously accessible to such minute organisms as Bacteria, and favourable for their development. These are the alimentary canal and the blood. In the case of the former there is evidence that every one of us possesses quite a little flora of varied forms and species. They seem for the most part, in health, to be comparatively innocuous; indeed, it is believed that they are ancillary to and aid digestion. But it is easy to see that other kinds may be introduced, or those already present may be called into abnormal activity, and fermentative processes may be set up of a very inconvenient kind. These may result in mere digestive disorder, or in the production of some of those poisonous derivatives of proteids of which I have spoken, the effect of which upon the organism may be most disastrous.

The access of Bacteria to the blood is a far more serious matter. They produce phenomena the obvious analogy of which to fermentative processes has led to the resulting diseases being called zymotic. Take, for example, the disease known as "relapsing fever." This is contagious. After a period of incubation, violent fever sets in, which lasts for something less than a week, is then followed by a period of absence, to be again followed in succession by one or more similar attacks, which ultimately cease. Now you will observe that the analogy to a fermentative process is very close. The period of incubation is the necessary interval between the introduction of the germ and its vegetative multiplication in sufficient numbers to appreciably affect the total volume of the blood. The rise in temperature and the limited duration of the attack are equally, as we have seen, characteristic of fermentative processes, while the bodily exhaustion which always follows fever is the obvious result of the dissipation by the ferment organisms of nutritive matter destined for the repair of tissue waste. During the presence of this fever there is present in the blood an organism, *Spirochæte obermeieri*, so named after its discoverer. This disappears when the fever subsides. It is found that if other individuals are inoculated with blood taken from patients during the fever attack, the disease is communicated, but that this is not the case if the inoculation is made during the period of freedom. The evidence, then, seems clear that this disease is due to a definite organism. The interesting point, however, arises, why does the fever recur, and why eventually cease? The analogy of fermentation leads to the hypothesis that, as in the case of yeast, the products of its action inhibit after a time the further activity of the *Spirochæte*. The inhibiting substance is, no doubt, eventually removed partially from the blood by its normal processes of depuration, and the surviving individuals of *Spirochæte* can then continue their activity, as in lactic fermentation. With regard to the final cessation of the disease, there are facts which may lead one to suppose that in this as in other cases sufficient of the inhibiting substance ultimately remains in the organism to protect it against any further outbreak of activity on the part of the *Spirochæte*.

Here we have an example of a disease which, though having a well-marked zymotic character, is comparatively harmless. In anthrax, which is known to be due to *Bacillus anthracis*, we have one which is, on the contrary, extremely fatal. I need not enter into the details. It is sufficient to say that there is reason to believe that the *Bacillus* produces, as one of those by-products of protoplasmic destruction to which I have already alluded, a most virulent poison. But the remarkable thing is that this *Bacillus*, which can be cultivated externally to the body, if kept at a heightened temperature, can be attenuated in its virulence. It drops, in fact, the excretion of the poison. It is then found that, if injected into the blood, it does no mischief, and, what is more extraordinary, if the *Bacillus* in its most lethal form is subsequently introduced, it too has lost its power. The explanation of the immunity in this case is entirely different from that which was suggested by a consideration of the facts of relapsing fever. The researches of Metschnikoff have led to the hypothesis that in the present case the white blood-corpuscles destroy the *Bacillus*. When they first come into contact with these in their virulent form, they are unable to touch them. But if they have been educated by first having presented to them the attenuated form, they find no difficulty in grappling with the malignant. This is a very remarkable view. I should not have put it before you had there not been solid reasons for regarding the idea of the education of protoplasm with scientific respect. The plasmodia of the Myxomycetes, which consist of naked protoplasm, are known to become habituated to food which they at first reject, and the researches of Beyerinck on the disease known as "gumming" in plants have apparently shown that healthy cells may be taught, as it were, to produce a ferment which otherwise they would not excrete.

If Metschnikoff's theory be true, we have a rational explanation of vaccination and of preventive inoculation generally. It is probably, however, not the only explanation. And the theory of the inhibitive action upon itself of the products of the ferment-organism's own activity is still being made the basis of experiment. In fact, the most recent results point to the possibility of obtaining protection by injecting into the blood substances artificially obtained entirely independent of the organisms whose development they inhibit.

It is impossible for me to touch on these important matters at any greater length, but I doubt if the theory of fermentation, as applied to the diseases of organisms, has as yet more than opened its first page. It seems to me possible that, besides the rational explanation of zymotic diseases, it may throw light on others where, owing to abnormal conditions, the organism, as in the case of Bérard's plums, is itself the agent in its own fermentative processes.

And now I must conclude. I have led you, I am afraid, a too lengthy and varied a journey in the field of botanical study. But to sum up my argument. I believe I have shown you that at the bottom of every great branch of biological inquiry it has never been possible to neglect the study of plants; nay, more, that the study of plant-life has generally given the key to the true course of investigation. Whether you take the problems of geographical distribution, the most obscure points in the theory of organic evolution, or the innermost secrets of vital phenomena, whether in health or disease, not to consider plants is still, in the words of Mr. Darwin, "a gigantic oversight, for these would simplify the problem."

SECTION E.

GEOGRAPHY.

OPENING ADDRESS BY COLONEL SIR C. W. WILSON, R.E., K.C.B., K.C.M.G., D.C.L., LL.D., F.R.S., F.R.G.S., DIRECTOR-GENERAL OF THE ORDNANCE SURVEY, PRESIDENT OF THE SECTION.

ON opening the present session of the Geographical Section of the British Association I cannot refrain from alluding to the last occasion, now nearly a quarter of a century ago, upon which it met in this city. The chair was then filled by one to whom I, in common with others of the younger generation of that day, must ever owe a deep debt of gratitude for many kindly words of advice and encouragement. Then, as now, popular interest centred in Africa, and Sir Roderick Murchison, on taking the chair, was accompanied by a group of distinguished African explorers. Some amongst us may remember the enthusiastic greeting accorded to Livingstone, and the heart-felt sorrow caused by the announcement that the gallant, chivalrous officer, whose name will ever live in history as the discoverer of the sources of the Nile, had been cut off in the fullness of his strength and vigour.

The African travellers who have honoured us with their presence to-day, have shown the same pluck, the same perseverance, the same disregard of personal risk and comfort as their predecessors. One African traveller, a distinguished officer of the German army, who hoped to have been with us, has this year been awarded the highest honour which the Royal Geographical Society can confer—its gold medal. Lieut. Wissman, who possesses all Livingstone's indomitable courage, his constancy of purpose, and his kindly feeling towards the natives, has twice crossed Africa, in its widest extent, without firing a shot in anger. He returned recently to Europe, filled, like the great English traveller, with indignation at the atrocities perpetrated by the Arabs on the blacks; and eager to find means, if such there be, of putting an end to, or at least mitigating, the unspeakable horrors of the slave trade. He is now organizing an expedition which has the double object of opening up the territory in Eastern Africa that falls within the sphere of German influence, and of bearing relief to Emin Pasha. In both enterprises we may heartily wish him "God speed!"

The light thrown upon the interior of the Dark Continent is the most striking feature of geographical exploration during the last twenty-five years; and it is really the work of the last eleven years, for it was only in 1877 that Mr. Stanley, by his remarkable journey, gave a new continent to the world. If Sir Roderick Murchison were now alive he would feel more than gratified at results which have been so largely due to his initiative. I propose, presently, to return to the interesting subject of Africa; but I would first draw attention to the influence which the natural features of the earth's surface have had, and are still having, in conjunction with other causes, on the trade routes and commercial relations between the West and the East, and more especially with India.

The great civilizations of high antiquity appear to have risen and expanded in four riverain districts: Chinese in the basins of the Hoang-ho and the Yang-tse-kiang; Hindu in those of the Indus and the Ganges; Chaldæan and Assyro-Babylonian in those of the Tigris and Euphrates; and Egyptian in that of the Nile. India is separated from China, on the one hand, by rugged, lofty mountain ranges, and the high-lying plateau of Tibet; and from Mesopotamia, on the other, by the Suleiman Mountains and the Perso-Afghan plateau. Intercommunication between these early seats of man's activity must, therefore, have been of slow growth. From Mesopotamia, on the contrary, there is easy access to the Nile basin by way of Syria and Palestine, and there are indications of traffic between these districts at a very remote period. Inquiry into the causes which first led to intercommunication and into the means by which it was effected is needlesss. Desire of gain, lust of power, were as much a part of human nature in the earliest ages as they are now. The former induced the pioneers of commerce to feel their way across trackless deserts, and to brave the hidden dangers of the sea; and for nearly three hundred years it led gallant men to seek a way to the wealth of India through the ice-laden seas of the Arctic region. The latter brought the great empires of Assyria and Egypt into hostile conflict, and carried Alexander to the banks of the Oxus and the Indus; and it is largely answerable for the land-hunger of European States in our own generation.

Nations rise, fall, and disappear, but commerce extends in ever-widening circles, and knows no limits. Efforts are constantly being made to discover and open up new fields of commercial activity and to connect the great centres of commerce by quicker and shorter trade routes. The earliest traffic was conducted by land: men travelled together in caravans for mutual protection, and rested where food and water were to be obtained; at the most important of these halting-places cities were founded. As trade extended, it became necessary to carry goods through independent tribes or countries which often insisted on retaining the transit trade in their own hands, and this led to the rise of cities at points convenient for the transfer of loads and the exchange of commodities of one country for those of another. Generally speaking this early overland trade was co-extensive with the geographical limit of the camel. Next in order to land traffic came that by water, first on rivers, then on the sea; and cities naturally sprang up at places on the coast where the merchandise brought down the rivers in boats could, conveniently and safely, be transferred to galleys or ships suitable for coasting. After a knowledge of the monsoons had been acquired, men began to trust themselves to the open sea; the ships were improved, and a system was established under which voyages were made, with great regularity, at certain seasons of the year, so that advantage might be taken of the periodic winds. Increased knowledge of the globe, improvements in the art of shipbuilding, and the invention of the steam-engine, have gradually led to the ocean traffic of the present day, conducted by large steamers which, regardless of wind and tide, follow the most direct course from one point to another. The trade routes of the world are subject to two great modifying influences, one physical, the other political. The inland trade of India, for instance, can only reach Central Asia and the West by way of Herat or Bamian; caravan roads across the deserts of Asia and Africa must follow lines of springs or wells; climatic conditions render all Polar routes impracticable; and the removal of a physical obstacle, by the construction of the Suez Canal, is now causing a remarkable redistribution of the channels of commerce. So, too, disturbance of traffic by war, or its designed destruction by conquerors; and great political changes, such as the establishment of the Persian Empire, the rise of Rome, the disruption of the Roman Empire, and the advent of the Arabs to power in Wsstern Asia, divert trade from its accustomed routes and force it into new channels, to the ruin of some cities and States and the enrichment of others. The general tendency of trade so diverted is to seek, where possible, a maritime route, for water transport is not only less costly but less liable to interruption than land transport.

India, partly from its geographical position, partly from the character of its people, has always played a passive *rôle* in commerce, and allowed the initiative in commercial enterprise to rest with the West. The greatest advantages have always been derived from the possession of the trade between the East and the West, and from a remote period the nations of the world have contended for this rich prize. One State after another has obtained and lost the prize; England now holds it, but if she is to keep what she has obtained there must be a far closer study than there has hitherto been of geography and terrestrial phenomena in their relation to commerce. Trade between the East and the West may be divided into three periods: the *first*, during which the limits of Oriental commerce were the eastern and south-eastern shores of the Mediterranean, closed with the foundation of Carthage about 800 B.C.; the *second*, or Mediterranean period, ended in the fifteenth century; the *third*, or Oceanic period, has lasted to the present day. In the first period there were two principal lines of traffic: the southern sea route, following the coast line, and the northern land route, traversing Asia in its whole extent from east to west. There are indications of communication between China and the West so early as 2698 B.C.; and in 2353 B.C. an embassy arrived in China from a country which is supposed to have been Chaldæa. There is also an early notice of caravan traffic in the company of Ishmaelites, bearing spicery, and balm, and myrrh to Egypt, to whom Joseph was sold (Genesis xxxvii. 25–28). The earliest maritime people to appreciate the value of trade between the East and West were, apparently, those living along the south coast of Arabia. Happily situated between the Persian Gulf and the Red Sea, and separated by vast deserts from the great nations of Asia, the Sabæans were free from those alternations of industry and war which are so unfavourable to commercial pursuits; for centuries they possessed the commerce of India, and they became famous for their opulence and luxury. Sabæan ships visited Ceylon and the Malabar coast, and Sabæan merchants supplied Indian goods to Mesopotamia and Syria, as well as to Egypt and Ethiopia. The ships trading to the Persian Gulf discharged their cargoes near the mouth of the Euphrates; whence the traffic passed partly by river, partly by land, to the coast towns of Syria and Palestine, and through the Syrian and Cilician gates to Mazaca (*Kaisariyeh*), and Pterium (*Boghazkeui*); from the last place Indian goods found their way to Sardis and Sinope. The ships visiting the Red Sea landed goods at Elath, at the head of the gulf of Akabah, for carriage by land to Tyre and Sidon, and on the western shores of the

Red Sea for transmission to Meroë, Thebes, and Memphis. At the same time silks from China, and gems from India, were carried overland to Chaldæa and Assyria; and Bactra (*Balkh*), "the mother of cities," rose and flourished at the central point of the transit trade. Egypt, with no timber for shipbuilding, a distrust of all foreigners, especially when they came by sea, and a settled dislike of maritime pursuits amongst her people, long neglected the opportunities afforded by her favourable geographical position. Tyre, Sidon, and other Phœnician towns, reached by easy roads from the Euphrates and the Red Sea, and from their situation commanding the Mediterranean, became centres of distribution for Indian goods; and the Phœnicians, gradually extending their operations to the Red Sea, traded with the ports of Southern Arabia, and even ventured to the shores of India. It was in this first period that the Jewish kingdom reached its widest extent. During the long wars of David's reign the Jews obtained possession of the land routes over which the rich products of India were carried to Tyre and Sidon; and Solomon did all in his power, by building Tadmor in the Wilderness (Palmyra), by improving the port of Elath, and by carrying out other great works, to protect and facilitate the transit trade from which such large profits were derived. The Jews do not appear to have been the actual carriers, but many of them no doubt, following the example of their merchant-king, engaged in commercial pursuits, and wealth poured into the kingdom so that silver was made to be as stones in Jerusalem.

In the early portion of the second period the commercial prosperity of the Phœnicians reached its culminating point. Their colonies dotted the shores of the Mediterranean, and their ships passed the "Pillars of Hercules" to Great Britain and the western shores of Africa, and floated on the waters of the Red Sea, the Persian Gulf, and the Indian Ocean. The seaborne trade of the known world was in their hands; wealth flowed into their cities, and in the markets of Tyre tin from Cornwall and amber from the Baltic were exposed for sale with the silks, gems, and spices of the far-distant East. The decline of Phœnicia dates from the establishment of the Persian Empire in the sixth century B.C., and after the capture of Tyre by Alexander its commerce gradually passed into the hands of the Greeks. The Persian policy of closing the Persian Gulf to commerce forced the Indian traffic along the land routes. Babylon, which had become the emporium of Eastern trade, declined, whilst Susa and Ecbatana were enriched by the transit trade which passed through them and crossed the whole extent of the empire to the Mediterranean ports. The policy of Alexander was to secure the carrying and distribution trade of the world to the Greeks; and with this object he founded Alexandria, and intended, had he lived, to restore Babylon to her former splendour. Ptolemy, his successor in Egypt, used every means in his power to draw trade to Alexandria, and the new city soon rose to opulence and splendour. The Greek merchants obtained their Indian goods from the Arab traders whom they met in the ports of Southern Arabia; they landed them at Myos Hormos and Berenice on the western shore of the Red Sea, carried them by camel across the desert, and floated them down the Nile and by canal to Alexandria, whence they were distributed to the neighbouring parts of Africa and the coasts of the Mediterranean. This trade route remained unaltered until Egypt became a Roman province. Another stream of commerce passed by way of the Persian Gulf to Seleucia on the Tigris, and thence, partly by water and partly by land, through Aleppo to Antioch and Seleucia at the mouth of the Orontes; and a third followed the ancient highway from Central Asia to the ports of the Euxine and Ægean Seas.

After the rise of Rome all trade routes were directed upon the Imperial City, which became a centre of distribution for the merchandise of the East. The Greeks still monopolized the seaborne trade; and those of Egypt, recognizing the advantage of their geographical position, took the direct trade to India into their hands, and extended their voyages to Kattigara, the port of the Sinæ, in the Gulf of Tongking. Alexandria became the commercial capital of the Roman Empire, the distributing centre of the world for Indian and Asiatic goods, and a place of such wealth that one of the merchants is said to have been able to maintain an army. At the same time the old ports of Tyre, Beirût, Antioch, Ephesus, Byzantium, and Trebizonde maintained their position as *termini* of the land traffic. The extent of the intercourse between the East and the West during the Roman Empire is shown by the embassy of the Seres (Chinese) to Rome in the reign of Augustus, and by the several embassies to China, which followed that sent by Marcus Aurelius in 166 A.D., until the Arab Empire interposed; as well as by the fact that in the time of Pliny the Roman imports from Asia each year were valued at 100,000,000 sesterces (about £800,000). Trade followed well-established routes which remained in use, with but slight modification, till the fifteenth century. There were three principal lines of communication through Central Asia, all leading from China across the Desert of Gobi. The northern ran to the north of the Thien-Shan by Lake Balkash to the Jaxartes (*Syr Darya*); the central passed along the southern slopes of the Thien-Shan and crossed the mountains by the Terek Pass to Samarcand and the Oxus (*Amu Darya*); and the southern passed over the Pamir and through Badakhshan to Balkh. The northern route apparently went on from the Jaxartes, through Khiva, to the Caspian, which it crossed, and then ran on to the Black Sea. Even at this early period trade filtered round the northern shores of the Caspian, and later, during the Middle Ages, there was a well-established trade route in this direction through Khiva to Novgorod and the Baltic, by which the northern countries received Indian goods. From the Oxus region reached by the central and southern lines there were two routes to the West. One passed through Merv, crossed the Caspian, ascended the Araxes to reach Artaxates and Trebizonde, or to descend the Phasis (*Rion*) to Poti, and then coasted the shores of the Black Sea to Byzantium. The other also passed through Merv, and, running along the northern frontier of Persia, reached the shores of the Black Sea through Artaxates, or continued on through Mesopotamia, Syria, and Asia Minor to Byzantium. The land trade from India passed through the Bamian Pass to Balkh, and through Kandahar and Herat to Merv or Sarrakhs to join the great stream of Central Asian traffic. The greater portion of the carrying trade on these long lines was in the hands of the people dwelling between the Jaxartes and the Oxus, who had their centre at Samarcand; and these Sogdians, or Asi as they are called in the Chinese annals, fearing lest they should lose the profit on the transit trade, threw every obstacle in the way of direct communication between China and the Roman Empire. The difficulties which thus interrupted the land traffic gave an impetus to the trade by sea, and so benefited Alexandria and the cities in the Persian Gulf. The sea trade at this time was carried by way of the Persian Gulf and the Red Sea. In the first case the cargoes were landed at some port on the Euphrates or Tigris, whence the goods were carried by river and caravan up the valleys of those rivers and then through Syria to Beirût and Antioch, and through Asia Minor to Ephesus, Smyrna, Constantinople, and Samsûn. In the second case the merchandise was landed either near Suez, whence it was conveyed by caravan, canal, and river to Alexandria, and at a later date to Pelusium, or at the head of the Gulf of Akabah for transport to Syria and Palestine. The sea trade was to a great extent a coasting trade, and it appears to have been shared by the Greeks and the Arabs, and perhaps by the Chinese, whose junks were to be seen at Hira, on the Euphrates, in the fifth century.

On the disruption of the Roman Empire the Byzantines, with their capital situated on the confines of Europe and Asia, naturally became the intermediaries between the East and the West, and they retained this position until the maritime towns of Italy, France, and Spain became sufficiently strong to engage in direct trade with the Mediterranean ports to which the produce of the East found its way. Until the seventh century the Sassanians held the lines of communication by land, and they did all they could to prevent Eastern produce from being carried over any other roads than those passing through their territory or by any other hands than theirs. In the sixth century they allowed an exchange of produce between the East and the West to take place at only three points: Artaxates for goods arriving from Central Asia; Nisibis for those from Central Asia and by the Tigris route; and Callinicum (*Rakka*) for those coming by way of the Persian Gulf and the Euphrates. Justinian attempted to free Oriental commerce from its dependence on the Sassanians by opening up new trade routes. The Sogdian silk merchants passed, outside of Persian territory, round the north end of the Caspian to meet those of Byzantium on the shores of the Sea of Azov and the Black Sea; the products of India were obtained from Ethiopian traders at Adulis, on the Red Sea; and Greek navigators, taking advantage of the monsoons, sailed direct from the southern end of the Red Sea to the Malabar coast and Ceylon.

In the seventh and eighth centuries the Arabs overran the whole of Central Asia, and the carrying trade by sea and by land passed into their hands. Profound modifications were thus introduced into the commercial intercourse between the East and the West. All land traffic from the East was directed upon Baghdad, which became the distributing centre whence goods were despatched by the ancient trade routes to the West, and which almost rose to the splendour of Babylon. On the sea the Arabs regained their old reputation; they sailed direct from the Red Sea to Cape Comorin, and from Ceylon to the Malay Peninsula, and extended their voyages to Kanpu, on a delta arm of the Yang-tse-Kiang; they established factories in the Indian Ocean, and, in the eighth century, were so numerous in Canton as to be able to attack and pillage that city. Their only rivals were the Chinese, whose junks visited the Euphrates and Aden, and brought silks and spices to the Malabar coast to be there exchanged for the raw material and manufactures of the West.

The Eastern produce brought by the Arabs to the ports of the Mediterranean was conveyed to Europe by the merchants of Venice, Genoa, Pisa, and other towns, who also traded to Constantinople and the Black Sea. Venice from its geographical position was well adapted to be the intermediary between the East and Central Europe, and even before the rise of Islam a large share of the carrying trade of the Mediterranean had fallen into its hands through the apathy and luxurious indolence of the Byzantines. It is unnecessary to trace the rise of Venice or discuss the impetus given by the Crusades to commercial intercourse between the East and Western Europe; it will be sufficient to note that in the first quarter of the fifteenth century the carrying trade of the Mediterranean was wholly in the hands of the Venetians, and Venice had become the distributing centre for all Europe. Venetian fleets, well guarded by war galleys, sailed at stated times for Constantinople and the Black Sea; for Syria and Egypt; for France; for Spain and Portugal, and for Holland. From the ports in those countries, as well as from Venice herself, the products of the East were carried inland over well-defined trade routes, and cities such as Pavia, Nürnberg, and Bruges, the emporium of the Hanseatic League, rose to importance as *entrepots* of Eastern commerce.

The victorious advance of the Turks, the fall of Constantinople, the piracy in the Mediterranean, and the termination of all intercourse with China on the decline of the Mongol dynasty in the fourteenth century, combined with other circumstances to turn men's minds towards the discovery of a more convenient way to the East. India was the dream of the fifteenth-century merchant, and how to reach it by a direct sea voyage was the problem of the day. The problem was solved when Vasco de Gama reached the shores of India on May 20, 1498; and its solution was due to the wise policy of a great grandson of Edward III., Prince Henry of Portugal, "the Navigator," who unfortunately died before success was attained. The discovery of the Cape route was no mere accident, but the result of scientific training, deep study, careful preparation, and indomitable perseverance. Prince Henry having determined to find a direct sea route to India, invited the most eminent men of science to instruct a number of young men who were educated under his own eye, and in a few years he made the Portuguese the most scientific navigators in Europe. The successful voyage of Vasco de Gama soon produced important results; the saving in freight by the direct sea route was enormous, and when it became generally known that the products of the East could be obtained much cheaper in Lisbon than anywhere else, that city became the resort of traders from every part of Europe. From Lisbon, Indian commodities were carried to Antwerp, which soon became the emporium of Northern Europe. By these changes the trade of Venice was almost annihilated, and Lisbon became the richest commercial city in Europe. The Venetians had endeavoured to confine commerce within its existing limits, and to keep to the trade routes then in use. They had never made any attempt to enlarge the sphere of nautical and commercial enterprise, and the consequence was that their ablest seamen, imbued with the spirit of adventure, took service in the Western States. When the Cape route was discovered, instead of attempting to secure a share in the direct sea trade, they entered into an alliance with the Sultan of Egypt to crush the Portuguese, and built a fleet for him at Suez which was defeated by Almeida in 1508. After this defeat the trade of Venice soon passed away.

Since the discovery of the Cape route there has been one long struggle for the possession of the commerce of India; who should be the carriers and distributors of Indian commodities was for more than two and a half centuries a much contested point amongst the maritime nations of the West. At first there seems to have been a general acquiescence in the claim of the Spaniards and Portuguese to a monopoly of the southern sea-routes, and this led to those heroic efforts to find a north-east or north-west passage to India which have so greatly added to our geographical knowledge. Failure in this direction was followed by attempts to reach India by the Cape in the face of the hostile attitude of Spain and Portugal. The mighty events which in turn transferred wealth and commerce from Lisbon to Antwerp, Amsterdam, and the banks of the Thames are matter of history, and it is scarcely necessary to say that at the close of the Napoleonic wars England remained undisputed mistress of the sea, and had become not only the carrier of all ocean-borne traffic, but the distributing centre of Indian goods to the whole world. A period of keen competition for a share in the commerce of India has again commenced amongst the States of Europe, and symptoms of a coming change in the carrying and distributing trade have been increasingly apparent since Africa was separated from Asia, nearly twenty years ago, by the genius of M. de Lesseps.

The opening of the Suez Canal, by diverting trade from the Cape route to the Mediterranean, has produced and is still producing changes in the intercourse between the East and the West which affect this country more nearly, perhaps, than any other European State. The changes have been in three directions.

First. An increasing proportion of the raw material and products of the East is carried direct to Mediterranean ports, by ships passing through the Canal, instead of coming, as they once did, to England for distribution. Thus Odessa, Trieste, Venice, and Marseilles are becoming centres of distribution for Southern and Central Europe, as Antwerp and Hamburg are for the North; and our merchants are thus losing the profits they derived from transmitting and forwarding Eastern goods to Europe. It is true that the carrying trade is still, to a very great extent, in English hands; but should this country be involved in a European war, the carrying trade, unless we can efficiently protect it, will pass to others, and it will not readily return. Continental manufacturers have always been heavily handicapped by the position England has held since the commencement of the century, and the distributing trade would doubtless have passed from us in process of time. The opening of the Canal has accelerated the change, to the detriment of English manufactures, and consequently of the national wealth; and it must tend to make England less and less each year the emporium of the world. We are experiencing the results of a natural law that a redistribution of the centres of trade must follow a rearrangement of the channels of commerce.

Second. The diversion of traffic from the Cape route has led to the construction of steamers for special trade to India and the East through the Canal. On this line coaling-stations are frequent, and the seas, excepting in the Bay of Biscay, are more tranquil than on most long voyages. The result is that an inferior type of vessel, both as regards coal-stowage, speed, endurance, and seaworthiness, has been built. These "Canal wallahs," as they are sometimes called, are quite unfitted for the voyage round the Cape, and should the Canal be blocked by war or accident they would be practically useless in carrying on our Eastern trade. Since the Canal has deepened they have improved, for it has been found cheaper to have more coal-stowage, but they are still far from being available for the long voyage round the Cape. Had the Canal not been made, a large number of fine steamers would gradually have been built for the Cape route, and though the sailing-ships which formerly carried the India and China trade would have held their own longer, we should by this time have had more of the class of steamer that would be invaluable to us in war time, and our trade would not have been liable, as it is now, to paralysis by the closing of the Canal.

Third. Sir William Hunter has pointed out that, since the opening of the Canal, India has entered the market as a competitor with the British workman; and that the development of that part of the Empire as a manufacturing and food-exporting country will involve changes in English production which must for a time be attended by suffering and loss. Indian trade has advanced by rapid strides, the exports of merchandise have risen from an average of 57 millions for the five years preceding 1874 to 88 millions in 1884, and there has been an immense expansion in the export of bulky commodities. Wheat, which occupied an insignificant place in the list of exports, is now a great staple of Indian commerce, and the export has risen since

1873 from $1\frac{3}{4}$ to 21 million hundredweights. It is almost impossible to estimate the ultimate dimensions of the wheat trade, and it is only the forerunner of other trades in which India is destined to compete keenly with the English and European producers.

The position in which England has been placed by the opening of the Canal is in some respects similar to that of Venice after the discovery of the Cape route; but there is a wide difference in the spirit with which the change in the commercial routes was accepted. Venice made no attempt to use the Cape route, and did all she could to prevent others from taking advantage of it: England, though by a natural instinct she opposed the construction of the Canal, was one of the first to take advantage of it when opened, and so far as the carrying trade is concerned she has hitherto successfully competed with other countries.

It is only natural to ask what the result of the opening of the Panama Canal will be. To this it may be replied that the Canal, when completed as a maritime canal, without locks, will promote commercial intercourse between the eastern and western coasts of America; will benefit merchants by diminishing distances, and reducing insurance charges; and possibly divert the course of some of the trade between the East and West; but it will produce no such changes as those which have followed the construction of the Suez Canal.

The increasing practice of the present day is for each maritime country to import and carry the Indian and other commodities it requires, and we must be prepared for a time when England will no longer be the emporium of Eastern commerce for Europe, or possess so large a proportion as she now does of the carrying trade. So great, however, is the genius of the English people for commercial enterprise, and so imbued are they with the spirit of adventure, that we may reasonably hope loss of trade in one direction will be compensated by the discovery of new fields of commercial activity. The problem of sea-carriage has virtually been solved by the construction of the large ocean steamers which run direct from port to port without regard to winds or currents; and the only likely improvement in this direction is an increase of speed which may possibly rise to as much as thirty knots an hour. The tendency at present is to shorten sea-routes by maritime canals; to construct canals for bringing ocean-going ships to inland centres of industry; and to utilize water carriage, wherever it may be practicable, in preference to carriage by land. For a correct determination of the lines which these shortened trade routes and great maritime canals should follow, a sound knowledge of geography and of the physical condition of the earth is necessary; and instruction in this direction should form an important feature in any educational course of commercial geography. The great problem of the future is the inland carrying trade, and one of the immediate commercial questions of the day is, Who is to supply the interiors of the great continents of Asia and Africa, and other large areas not open to direct sea traffic? Whether future generations will see

> "The heavens fill with commerce, argosies of magic sails,
> Pilots of the purple twilight, dropping down with costly bales,"

or some form of electric carriage on land, may be matter for speculation; but it is not altogether impossible to foresee the lines which inland trade must follow, and the places which must become centres of the distributing trade, or to map out the districts which must, under ordinary conditions, be dependent upon such centres for their supply of imported commodities. The question of supplying European goods to one portion of Central Asia has been partially solved by the remarkable voyage of Mr. Wiggins last year, and by the formation of the company of the "Phœnix Merchant Adventurers." Mr. Wiggins started from Newcastle-on-Tyne for Yeniseisk, the first large town on the Yenisei, some 2000 miles from the mouth of that river, and within a few hundred versts of the Chinese frontier. On the 9th of October, 1887, he cast anchor and landed his cargo in the heart of Siberia. The exploit is one of which any man might well be proud, but in Mr. Wiggins's case there is the additional merit that success was the result of conviction arrived at by a strict method of induction, that the Gulf Stream passed through the Straits into the Kara Sea, and that its action, combined with that of the immense volume of water brought down by the Obi and Yenisei, would free the sea from ice and render it navigable for a portion of each year. The attempts of England to open up commercial relations with the interior of Africa have too often been marked by want, if not open contempt, of geographical knowledge, and by a great deficiency of foresight; but the competition with Germany is forcing this country to pay increased attention to African commerce, and the formation of such companies as the British East African Company, the African Lakes Company, and the Royal Niger Company is a happy omen for the future.

Another branch of the subject to which attention may be briefly directed is the fact that it is becoming increasingly evident that manufactures cannot profitably be carried on at a distance from the source of the raw material and the destination of the products. In India, for instance, where the first mill for the manufacture of cotton yarn and cloth was set up in 1854, there are now over 100 cotton and jute mills with 22,000 looms and 2,000,000 spindles; and similar changes are taking place elsewhere.

I am afraid that I have frequently travelled beyond the sphere of geography. My object has been to draw attention to the supreme importance to this country of the science of commercial geography. That science is not confined to a knowledge of the localities in which those products of the earth which have a commercial value are to be found, and of the markets in which they can be sold with the greatest profit. Its higher aims are to divine, by a combination of historical retrospect and scientific foresight, the channels through which commerce will flow in the future, and the points at which new centres of trade must arise in obedience to known laws. A precise knowledge of the form, size, and geological structure of the globe; of its physical features; of the topographical distribution of its mineral and vegetable products, and of the varied forms of animal life, including man, that it sustains; of the influence of geographical environment on man and the lower animals; and of the climatic conditions of the various regions of the earth, is absolutely essential to a successful solution of the many problems before us. If England is to maintain her commanding position in the world of commerce, she must approach these problems in the spirit of Prince Henry the Navigator, and by high scientific training fit her sons to play their part like men in the coming struggle for commercial supremacy. The struggle will be keen, and victory will rest with those who have most fully realized the truth of the maxim that "Knowledge is power."

I may add that if there is one point clearer than another in the history of commerce it is this: that when a State cannot effectually protect its carrying trade in time of war, that trade passes from it and does not return. If England is ever found wanting in the power to defend her carrying trade, her fate will only too surely, and I might almost say justly, be that of Venice, Spain, Portugal, and Holland.

I will now ask you to turn your attention for a few moments to another subject—Africa. In 1864, Sir Roderick Murchison alluded to the great continent in the following terms: "Looking at the most recent maps of Africa, see what enormous *lacunæ* have to be filled in, and what vast portions of it the foot of the white man has never trodden." It was then impossible to give a general sketch even of the geography of Equatorial Africa. Tanganyika and Nyassa had been discovered, and Speke and Grant had touched at a few points on the southern, western, and northern shores of the Victoria Nyanza; but we were still in ignorance of the drainage and form of the immense tract of country between the Tanganyika Lake and the Zambesi; and the heart of Africa, through which the mighty Congo rolls, was as much unknown to us as the centre of America was to our ancestors in the middle of the sixteenth century. There are now few school-boys who could not give a fairly accurate sketch of the geography of Central Africa; and a comparison of the maps published respectively in 1864 and 1888 will show how rapidly the *lacunæ* of which Sir Roderick complained are being filled in. There is still much to be done, and it is precisely in one of the few blank spots left on our maps that the man who may well be called the Columbus of Africa has so mysteriously disappeared. The discovery of the course of the Congo by Stanley has been followed by results not unlike those which attended the discovery of America by Columbus. In the latter part of the nineteenth century Africa has become to Europe what America was in the sixteenth century. Events march more rapidly now than they did then, and the efforts of the maritime nations of Europe to secure themselves some portion of African territory and some channel through which they can pour their products into Central Africa are rapidly changing the condition of the Dark Continent.

The roads over which the land trade of Equatorial Africa now passes from the coast to the interior are mere footpaths, described by Prof. Drummond, in his charming book "Tropical Africa," as being "never over a foot in breadth, beaten as hard as adamant, and rutted beneath the level of the forest bed by centuries of native traffic. As a rule these footpaths are marvellously direct. Like the roads of the old Romans, they run straight on through everything, ridge and mountain and valley, never shying at obstacles, nor anywhere turning aside to breathe. Yet with this general straightforwardness there is a singular eccentricity and indirectness in detail. Although the African footpath is on the whole a bee-line, no fifty yards of it are ever straight. And the reason is not far to seek. If a stone is encountered, no native will ever think of removing it. Why should he? It is easier to walk round it. The next man who comes that way will do the same. . . . Whatever the cause, it is certain that for persistent straightforwardness in the general, and utter vacillation and irresolution in the particular, the African roads are unique in engineering." No country in the world is better supplied with paths; every village is connected with some other village, every tribe with the next tribe, and it is possible for a traveller to cross Africa without once being off a beaten track. The existence nearly everywhere of a wide coast plain with a deadly climate, and the difficulties attending land transport in a country where the usual beasts of burden, such as the camel, the ox, the horse, and the mule, cannot be utilized, will probably for many years retard the development of the land trade. On the other hand, the Congo with its wide-reaching arms, the Niger, the Nile, the Zambesi, the Shiré, and the great lakes Nyassa, Tanganika, and the Victoria and Albert Nyanzas offer great facility for water transport, and afford easy access to the interior without traversing the pestilential plains. Already steamers ply on most of the great waterways—each year sees some improvement in this respect; and a road is in course of construction from Lake Nyassa to Tanganyika which will tend, if Arab raiders can be checked, to divert inland traffic from Zanzibar to Quilimane, and will become an important link in what must be one of the great trade routes in the future. It is possible, I believe, with our present knowledge of Africa, and by a careful study of its geographical features, to foresee the lines along which trade routes will develop themselves, and the points at which centres of trade will arise; but I have already detained you too long, and will only venture to indicate Sawákin, Mombasa, Quilimane, or some point near the mouth of the Zambesi, and Delagoa Bay, as places on the east coast of Africa which, from their geographical position, must eventually become of great importance as outlets for the trade of the interior.

The future of Africa presents many difficult problems, some of which will no doubt be brought to your notice during the discussion which, I trust, will follow the reading of the African papers; and there is one especially—the best means of putting an end to slave hunting and the slave-trade—which is now happily attracting considerable attention. It is surely not too much to hope that the nations which have been so eager to annex African soil will remember the trite saying that "Property has its duties as well as its rights," and that one of the most pressingly important of the duties imposed upon them by their action is to control the fiends in human form who, of set purpose, have laid waste some of the fairest regions of the earth, and imposed a reign of terror throughout Equatorial Africa.

NOTES.

We regret to announce that Dr. Peter Griess died very suddenly at Bournemouth on Thursday last week, apparently from an attack of apoplexy. A very skilful manipulator, enthusiastically devoted to his science, a patient and unwearying worker, his death will deprive chemical science of one of its brightest ornaments. He will be chiefly remembered for his discovery of the diazo-compounds, one of the most remarkable classes of substances known to chemistry.

A TELEGRAM from the city of Mexico states that on the night of the 6th instant there occurred the heaviest shocks of earthquake ever recorded in the city. The houses swayed, the walls cracked, and people rushed into the streets to pray. There was for a few moments much apprehension. The phenomenon was preceded by high winds and dust-storms.

A FRIGHTFUL cyclone, involving great destruction of property and loss of life, took place at Havannah on the 4th instant. It is stated to have been the most severe experienced in the West Indies for many years past.

The inaugural address of St. Thomas's' Hospital will be delivered in the theatre on Monday, October 1, at 3 p.m., by Dr. Cullingworth.

The sixth course of twelve lectures and demonstrations for the instruction of sanitary inspectors will be delivered at the Parkes Museum on Tuesdays and Fridays at 8 p.m., commencing with the 25th instant. The lectures will deal with sanitary subjects generally, and will be delivered by the leading men in the various branches—Sir Douglas Galton, Profs. Corfield and Henry Robinson, Drs. Poore, Louis Parkes, and Charles Kelly, Messrs. Wynter Blyth, Boulnois, Cassal, and Sykes. A nominal fee of five shillings will be charged, and students attending the course will be granted free admission to the Parkes Museum and Library during September, October, and November. The last course was attended by over ninety students, and it is proposed to repeat it twice each year to suit the requirements of persons preparing for the examinations of the Sanitary Institute, as well as of others desirous of obtaining a practical knowledge of sanitary requirements and regulations.

The September issue of the Kew *Bulletin* continues the notes on colonial fruit, including a long and most interesting report on the fruits of the Island of Dominica. There is also a report from the British Political Officer at Bahmo on the india-rubber trade of the Mogaung district of Upper Burma. The rubber forests, though worked by Chinese, are owned by the Kachins, a tribe inhabiting the borderland between Burma and China.

We have received Parts 2 and 3 of the second volume of the Journal of the College of Science of the Imperial University of Japan. The former opens with a paper by Dr. Koto "On the so-called Crystalline Schists of Chichibu," a district lying north-west of Tokio, and, geologically speaking, a region complete in itself, and, according to Dr. Koto, typical of the geological formation of the rest of Japan. The essay, which is accompanied by five plates, occupies the greater part of the number. Prof. Okubo gives a brief account of the botany of Sulphur Island, a volcanic and uninhabited island off the Japanese coast. Dr. Ijima and Mr. Murata describe some new cases of the occurrence of *Bothriocephalus liguloides*, Lt. No. 3 is filled with the account of a magnetic survey of all Japan, carried out by order of the President of the Imperial University, the authors being Profs. Knott and Tanakadate. The paper, which is an elaborate one, is divided into five sections: (1) historical retrospect, and general description of the aim and methods of the survey; (2) particular account of the equipment and modes of operation of the northern party; (3) the same details for the southern party; (4) final reduction of the observations, and general conclusions; (5) comparison of results with those of previous observers. In an appendix, Prof. Knott gives an exceedingly interesting sketch of Ino Tadayoshi, a Japanese surveyor and cartographer of the latter half of the last century.

The current number of the *Westminster Review* contains an article by Mr. Gundry, entitled "China; A New Departure," the "departure" in question being the introduction of mathematics into the curriculum of subjects in the competitive examinations upon which the whole system of Chinese administration is based. Various methods have been proposed from time to time to bring Chinese students into touch with Western learning. Prince Kung, who was Prime Minister in 1866, suggested the erection of a special department presided over by foreign professors for the study of "mathematics," that term being obviously meant to include all branches of physical science. This was done, but public opinion was not ripe for the change, and the result was failure. In 1875 it was proposed, not to instruct Chinese in Western learning, but to teach foreigners the ancient lore of China, and thus enable them to qualify for office. This plan was not tried. Then students were sent abroad to be educated, but they became demoralized, and returned totally out of sympathy with their national traditions. Last year the Censors, who till then were the opponents of all innovation, advocated alterations in the educational system, and the Cabinet, presided over by Prince Chun, the father of the reigning Emperor, thereupon reported in favour of introducing mathematics into the competitive examinations. For the first time, then, provision has been made for spreading through the empire a knowledge of Western science, and there can be no doubt that the ultimate result must be a complete revolution in Chinese thought. The influence of a remote past will be diminished, the necessity for change recognized, and intimacy with "barbarian" learning will do away with the present prejudices against the "barbarians" themselves. But these advantages must not be over-estimated. Though the necessity for studying foreign science is admitted, widespread and intense prejudice has to be conquered, and a new generation will probably have arisen before the full effect of the innovation is felt.

In the last number of the *Essex Naturalist* (vol. ii., Nos. 7 and 8, p. 113), Prof. Meldola announces that he has at length detected the scent emitted by the male moth *Herminia tarsipennalis*. It has long been known that this insect possessed fan-like structures on the front legs, and it had been surmised that these were secondary sexual characters. The detection of the scent now places the function of these organs beyond doubt, and it is of interest to add that the odour has been recognized as similar to that of artificial essence of jargonelle pear—that is, to amyl acetate. Some of the males of South American butterflies, which are provided with elaborate scent organs, according to Fritz Müller, give off a distinct odour of vanilla.

The *Oderzeitung* reports the finding in the Lossow district, near Frankfort-on-the-Oder, of about thirty clay vessels of various sizes and patterns, some urns, some pots, deep saucers, flasks, &c. They were filled with the ashes of burnt corpses mixed with sand. The colour was a brownish-yellow; some were broken, and the fractures showed that coal ashes had been mixed with the clay of which they were made. Some bronze needles were found with them, being finished at the top in a semicircular shape. The vessels seemed to have been formed on a lathe, tolerably smooth, regular in shape, and only slightly baked. The largest were about 30 centimetres in diameter at the widest part, and 26 centimetres high. The ornaments were either triangles or semicircles, scratched on the surface with points impressed on the surface. Possibly the site where they were found was a refuge and a place of sacrifice in old German times.

We have received the Calendar of the University College, Dundee, for the forthcoming session, together with reports on the work of the past year. The progress seems to have been of the usual satisfactory character. A department of dyeing and bleaching has been added since the last session.

An interesting article has been published in the *Cologne Gazette* from the pen of Herr Gerhard Rohlfs, the African explorer, in which the German plans for rescuing Emin Pasha are subjected to an exhaustive criticism. Herr Rohlfs is of opinion that the proposed expedition may attain its ends if the preliminary preparations are properly and not too slowly conducted, and if the necessary sum of money is subscribed; all that Emin Pasha can want being guns, small cannon, and ammunition. The advance of the expedition must take place slowly and methodically, and depots, commanded by Germans, should be established on the road at intervals from one another represented by from six to eight days' march. From Bagamoyo to Mutansige a distance of 1500 kilometres has to be covered without leaving German territory. From Mutansige to Wadelai the distance is 400 kilometres. The expeditionary force need not include more than 100 Germans, but, as it must be sent out at once if it is to do any good, State aid becomes absolutely necessary. A considerable sum is required. Herr Rohlfs estimates that the expedition conducted by Stanley to the relief of Livingstone cost 2,000,000 marks, and the process of obtaining the sum needed by subscription is far too slow. As this expedition, adds Herr Rohlfs in conclusion, is likely to assist in consolidating German colonial enterprise in Africa, no sacrifice should be spared for carrying it into execution.

We have received from the Deutsche Seewarte at Hamburg vol. ix. of *Meteorologische Beobachtungen in Deutschland*, containing the observations, for 1886, made at twenty-five stations of the second order, in accordance with the proposal of the Meteorological Congress at Vienna, 1873, that each country should publish the individual observations for a certain number of places. We observe, however, from the preface that in future the Central Office at Berlin will undertake the publication of some of these observations. The volume also contains hourly observations for four stations, and a summary of the storms experienced on the German coasts. These useful statistics of storms have been regularly published since 1878.

The Meteorological Section of the Report of the Governor of St. Helena on the state of the colony for the past year is interesting, if brief:—"The year under review was dry; the rainfall at Longwood, where Napoleon lived, was 28·74 inches. No lightning has occurred since 1878, and storms are unknown."

We have received the Report and Proceedings of the Bristol Naturalists' Society for the past year. The members number 224, which seems satisfactory all things considered, yet the Council are far from content. They urge that more cordial recognition and extended support might be expected in a city like Bristol, at a time when science holds so commanding a position for a Society which aims at promoting original scientific research, and at the same time presenting its results in a form intelligible to the general public, and accordingly members are urged to make the benefits of the Society as widely known as possible, while a *conversazione* is to be held next month with a view to directing public attention afresh to its objects and claims. *Sic itur ad astra:* it is thus that a strong and successful Natural History Society is founded. The contents of the Proceedings are attractive and varied, chief amongst them being a "geological reverie" on the Mendips, by Prof. Lloyd Morgan. An Engineering Section was last year added to the Society, and its papers are also published. Looking to this number of the Proceedings it appears to us that the Council have much reason to be proud of the Society, although perhaps it would not be quite prudent to say this in the Annual Report, when more members are required, and the balance with the treasurer has fallen very low. We cannot believe that so excellent a Society, which does much good work with such small funds, can lack abundant support in a district such as Bristol and its vicinity.

From the Parliamentary paper which has just been issued on the British Museum, it appears that the total number of persons admitted to view the collections has undergone a very great diminution within the past few years. In the year 1882 there were 767,402 visitors to the general collections, as against 501,256 in 1887. This diminution is more than accounted for by the transfer of the natural history collections to South Kensington, for we find that in the latter year there were 358,178 visitors to the Cromwell Road collections, being an increase of 80,000 over the number admitted in 1882. With regard to the number of visitors to particular departments for the purpose of study or research it has increased from 146,891 in 1882 to 182 778 in 1887 to the reading-room, from 1452 in 1885 (when the room was opened) to 11,802 in 1887 to the newspaper-room, and from from 2709 in 1882 to 14,238 in 1887 to the various departments in the new building in Cromwell Road. The students who frequent the reading-room will agree with the principal librarian's remarks as to the inadequacy of the accommodation of that room, and will hope that his recommendation to provide a separate room for "the throng of readers for general information" may be speedily carried out. Amongst the more important donations to the Museum during the past year were the following: stone implements from Japan and Greenland, ancient Peruvian pottery and masks, presented by the trustees of the late Mr. Christy; a collection of Andamanese objects from the Colonial Exhibition, by M. V. Portman; a valuable collection of ethnological objects from the Nicobar Islands, by E. H. Man; a remarkable collection of objects of the Late Celtic period, found in graves at Aylesford; a large collection of stone implements from Japan, presented by Sir Alexander Cunningham. The arrangement of many of the sections in the ethnographical gallery has been altered in the past year. Thus several sections of Asiatic islands have been revised to make room for the two large series from the Andaman and Nicobar Islands. Amongst the Oriental and ethnographical acquisitions during the year were the following: a collection of Indian antiquities, consisting of relic caskets of various kinds with various Buddhist sculptures, &c., presented by General Sir Alexander Cunningham; a number of antiquities from Siam and Burma, presented by E. M. Satow; seventy-six specimens of Chinese porcelain with armorial devices, presented by the Rev. F. Warre; a number of ethnographical specimens collected in the Pacific Islands by H. J. Veitch; and an extensive collection of specimens from New Guinea, including models of houses, boats, &c., collected by H. H. Romilly, and presented by the Queensland Commissioners of the Colonial and Indian Exhibition.

WITH regard to the natural history collections great progress has been made in the arrangement and description. Two cases have been placed on the floor of the Great Hall, illustrating general laws in natural history. The specimens in one case have been presented by Mr. Henry Seebohm, and show that what are regarded as two distinct species of crows (the *Corvus cornix* and the *Corvus corone*) may unite and produce offspring. The second case illustrates the effect of domestication on pigeons. The great collection of birds, which was formed chiefly by the late Marquess of Tweeddale, has been given to the Museum under certain conditions by Mr. R. G. Wardlaw-Ramsay, together with his large ornithological library. The collection comprises nearly 40,000 bird-skins, and is particularly valuable to the Museum, as it is very rich in birds of the Philippine Islands, Andaman Islands, &c., in which the Museum was very deficient. A collection of butterflies, anthropological objects, skins of birds and mammals, sent from Wadelai by Emin Pasha, has reached the Museum. The Commissioners present at the Indian and Colonial Exhibition gave some fine specimens of the flora of Australia and New Zealand. The zoology department is now overcrowded, 270,000 specimens having been added in the space of four years.

THE King of Italy, acting on the recommendation of the Minister of Public Instruction, has issued a decree regulating the manner in which Italy proposes to celebrate the fourth centennial of the discovery of America by Columbus. This will consist mainly in the publication of the collected works of the great navigator, and of all the documents and charts which will throw any light upon his life and voyages. This will be accompanied by a biography of the works published in Italy upon Columbus and the discovery of America from the earliest period down to the present time. The head of the Royal Commission charged with the preparation of this edition is Cesare Correnti, President of the Italian Historical Institute; and among its members are Signors Amari, Cantu, and Desimoni, and the Marquis Doria. An appropriation of 12,000 lire has been made to cover the expenses of this work, which is now fairly undertaken for the first time. Various editors have published portions of the writings of Columbus, as Navarrete the account of his voyages, and Major his letters; but no one has yet collected all his writings into a single edition, though an index to them was published in 1864.

THE British Consul at Chicago in a recent report refers to an interesting experiment in some of the Western States in afforestation. He says that in the vast prairies of the western half of Dakota, Nebraska, and Kansas, the eastern part of Colorado, and in the plains of Dakota and Wyoming, there is an almost total absence of trees, and hence the moisture is very deficient. In the forest regions and amongst the mountains, lumber and firewood have rapidly decreased from the reckless way in which old and young trees have been cut. This waste has been restrained by various Acts, principally by the Timber Culture Law, which regulates the disposal of lands. In Nebraska, fifteen years ago, a voluntary movement was started for the encouragement of planting and forestry in general, and one day in the year, called "Arbor Day," was set apart for that purpose. On that day trees are planted by prominent persons, and by the local bodies. This example has been followed by almost every other State named above, and "Arbor Day" is now a public holiday in those regions, the date being fixed by the Governor. So great has been the progress that in Kansas alone there are now no less than 250,000 acres of artificial forest. The kind of trees planted varies very much with the district and the taste of the planters. White elm is said to be the best tree, being of rapid growth and yet hardy. Oak, walnut, maple, elm, ash, catalpa, pine, tulip-tree, linden, and others, have all been found to flourish.

THE additions to the Zoological Society's Gardens during the past week include a Squirrel Monkey (*Chrysothrix sciurea*) from Guiana, presented by Mr. George Miles; a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Mr. J. Witham; a Kinkajou (*Cercoleptes caudivolvulus*) from Venezuela, presented by Dr. A. Batchelor, F.R.C.S.; a Black-backed Jackal (*Canis mesomelas* ♀) from South Africa, presented by Lieut. Lionel de Lautour Wells, R.N.; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, presented by Mrs. J. de la Mare; a Sulphur and White-breasted Toucan (*Ramphastos vitellinus*) from Rio Negro, presented by Dr. C. E. Lister; an Alligator (*Alligator mississippiensis*) from Florida, presented by Mr. Michael Millard; two Sharp-nosed Crocodiles (*Crocodilus acutus*) from Nicaragua, presented by Mr. E. A. Williams; a Common Viper (*Vipera berus*), British, presented by Colonel C. S. Sturt; a Grey Lemur (*Hapalemur griseus*) from Madagascar, received in exchange; a Barbary Wild Sheep (*Ovis tragelaphus* ♀) from North Africa, deposited; a Brazilian Cariama (*Cariama cristata*) bred in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *SEPTEMBER* 16–22.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on September 16

Sun rises, 5h. 39m.; souths, 11h. 54m. 34·6s.; sets, 18h. 10m.: right asc. on meridian, 11h. 38·0m.; decl. 2° 23′ N. Sidereal Time at Sunset, 17h. 54m.

Moon (Full on September 20, 5h.) rises, 16h. 57m.; souths, 21h. 25m.; sets, 2h. 0m.*: right asc. on meridian, 21h. 10·2m.; decl. 17° 45′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	7 24	13 1	18 38	12 44·2	5 8 S.
Venus.....	7 20	13 3	18 46	12 46·4	3 58 S.
Mars......	12 23	16 22	20 21	16 6·5	22 29 S.
Jupiter....	11 54	16 11	20 28	15 55·0	19 45 S.
Saturn....	1 57	9 30	17 3	9 13·4	16 52 N.
Uranus...	7 43	13 17	18 51	13 0·5	5 47 S.
Neptune..	20 33*	4 20	12 7	4 2·3	18 58 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Sept.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
16	30 Capricorni	6	21 47	23 1	128 287
19	B.A.C. 8274	6	20 56	near approach	176 —

Sept.	h.	
19	4	Mercury in conjunction with and 1° 40′ south of Venus.
22	15	Sun in equator.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Sept. 21,	4 54	*M*
T Arietis	2 42·1	17 3 N.	,, 21,		*m*
Algol	3 0·9	40 31 N.	,, 17,	20 15	*m*
R Leporis	4 54·5	14 59 S.	,, 18,		*m*
T Monocerotis	6 19·2	7 9 N.	,, 21,	3 0	*M*
ζ Geminorum	6 57·5	20 44 N.	,, 19,	0 0	*m*
S Canis Minoris	7 26·6	8 33 N.	,, 19,		*M*
S Cancri	8 37·5	19 26 N.	,, 22,	1 11	*m*
V Boötis	14 25·3	39 22 N.	,, 22,		*m*
U Coronæ	15 13·6	32 3 N.	,, 16,	1 6	*m*
			,, 22,	22 48	*m*
S Libræ	15 15·0	19 59 S.	,, 22,		*M*
S Scorpii	16 11·0	22 37 S.	,, 16,		*M*
U Ophiuchi	17 10·9	1 20 N.	,, 19,	3 22	*m*
R Scuti	18 41·5	5 50 S.	,, 19,		*M*
β Lyræ	18 46·0	33 14 N.	,, 20,	21 0	m_2
η Aquilæ	19 46·8	0 43 N.	,, 18,	23 0	*m*
T Vulpeculæ	20 46·7	27 50 N.	,, 19,	21 0	*m*
			,, 20,	23 0	*M*
W Cygni	21 31·8	44 53 N.	,, 20,		*M*
δ Cephei	22 25·0	57 51 N.	,, 20,	3 0	*m*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ε Tauri	64	21 N.	Swift; streaks.
,, η Aurigæ	74	41 N.	Sept. 21. Swift; streaks.
,, χ Orionis	89	18 N.	Very swift.
	98	44 N.	Very swift; streaks.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 3.—M. Janssen, President, in the chair.—Microbism and abscess, by M. Verneuil. The ordinary type of abscess is studied in connection with the new light thrown on the subject by microbic researches on suppuration. The almost constant presence of the micro-organisms described by Klebs, Pasteur, and others, shows that they are in all probability the real and exclusive cause of pyogenesis, a conclusion placed almost beyond doubt by the fact that, when introduced into the animal system, these organisms invariably produce suppuration and abscesses. A classification is given of the microbes in question, which are divided into two distinct groups: (1) pyogenic microbes, properly so called, which are normally present, such as the orange, lemon, white, and other varieties of Micrococcus and Diplococcus; (2) those which occur irregularly in the purulent matter, but which may exist normally in the system apart from any pyogenic symptoms or centres of suppuration—various kinds of Bacteria, Vibriones, Bacilli, &c. A classification follows of abscesses themselves, based on the etiology of pyogenesis as well as on their pathological anatomy and physiology.—Inscription giving the details of a lunar eclipse, by M. Oppert. This inscription, the text of which was first published by Strassmaier in the *Zeitschrift für Assyriologie*, vol. ii., is referred to the year 24 B.C., 232 of the era of the Arsacides. It describes the eclipse as having been predicted by the astronomer Uruda (Orodes), and as taking place, as predicted, in the month of Nisan, on the 13th night, at the hour of 5 and 51 parts, which is reduced to Monday, March 23, 9h. 30m. p.m., Paris mean time.—The fluorescent compounds of chromium and manganese, by M. Lecoq de Boisbaudran. These substances are studied and prepared synthetically with a view to determining their several degrees of oxidation.—Note on the position of some points on the Brazilian seaboard, extracted from a memoir of the Commissão de Longitudes, by M. Cruls. The places, whose positions are here astronomically determined by the officers attached to the Brazilian Hydrographic Service, are Cape Frio, 0h. 4m. 34·05s. (with probable error 0·12s.), east of Rio de Janeiro; and Santos, 0h. 12m. 33·44s. (with probable error 0·20s.), west of Rio de Janeiro.—On the measurement of the refraction indices of crystals with double axis, by M. Charles Soret. These measurements are here effected by the observation of the limiting angles of total reflection on any facets.—Physiological action of the chloride of ethylene on the cornea, by M. Raphael Dubois. In a previous paper (*Comptes rendus*, vol. civ., No. 26, 1887) the author showed that the chloride of ethylene ($C_2H_4Cl_2$) introduced in any way into the system produces in the dog, several hours after waking, an opacity of the cornea of a very remarkable character. Here he studies the nature of this phenomenon, and determines the mechanism by which it is produced.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Eclectic Physical Geography: R. Hinman (Cincinnati).—Solutions of the Examples in an Elementary Treatise on Conic Sections: C. Smith (Macmillan).—Chart for Great Circle Sailing. Nos. 1 and 2: R. A. Proctor (Stanford).—Les Tremblements de Terre: F. Fouqué (Baillière, Paris)—Die Structur und Zusammensetzung der Meteoreisen, Liefg. 1, 2, 3: A. Brezina and E. Cohen (Stuttgart).—The Speaking Parrots, Part 5: Dr. K. Russ (U. Gill).—The Flowering Plants of Wilts: Rev. T. A. Preston (Wilts Archæological and Natural History Society).—Results of Experiments at Rothamsted on the Growth of Root Crops: J. H. Gilbert.—Memoranda of the Origin, Plan, and Results of the Field and other Experiments at Rothamsted.—On Infant Feeding and the Value of Preparations of Pure Alpine Milk: Dr. Nachtigal (Ridgway).—Proceedings of the Bristol Naturalists' Society, vol. v. Part 3 (Bristol).—Proceedings of the American Academy of Arts and Sciences, New Series, vol. xv. Part 1 (Boston).—Meteorological Record, vol. vii. No. 28 (Stanford).—Quarterly Journal of the Royal Meteorological Society, July (Stanford).

CONTENTS.

THURSDAY, SEPTEMBER 20, 1888.

A TEXT-BOOK OF PHYSIOLOGY.

A Text-book of Physiology. By J. G. McKendrick, M.D. LL.D., F.R.S. Including "Histology," by Philipp Stöhr, M.D. In Two Volumes. Vol. I. General Physiology. (Glasgow: MacLehose and Sons. London: Macmillan and Co. 1888.)

THE present volume deals with the general principles of biology, the chemistry of the body, the early stages of development, the microscope, and the methods of microscopical research, the histology of the tissues and the physiology of muscle. It is no doubt very difficult to say what should and what should not be included in a text-book of physiology. The primary object is to explain as much as we can of the phenomena of the animal organism by physical and chemical laws. To understand such an explanation, a knowledge of chemistry, physics, and of the structure of the organism is essential. These subjects are treated of in special text-books which do not contain any physiology, and their introduction into a work devoted to this subject cannot fail to exert an injurious influence on the full exposition of the actual state of the science.

The present work is noticeable for the large amount of subsidiary matter which has been introduced, rather than as being a very complete account of modern physiology. The book is, however, intended by its author to aid the student to an intelligent knowledge of physiology, or rather, of all the subjects which are commonly dealt with by lecturers on physiology. It supplies the physical and chemical information more immediately required in physiological problems; it explains the methods by which the more important results have been obtained; and it gives a general insight into important biological facts.

Considering the very wide range of subjects, the choice of matter has been very well adapted to the object in view, and the book will doubtless find a larger circle of readers than the Professor's own class, for which it is especially intended. However, the degree to which the various sections have been brought up to date is very unequal. Some of the subjects have evidently been thoroughly worked up, whilst others appear to have been chiefly compiled from existing and not wholly modern text-books. In a work of this character, unless the author be endowed with almost superhuman industry, such a result is inevitable, and is fully foreseen by the author himself.

The section devoted to the general structure and physiology of the cell, the phenomena of fertilization, and the modern views on heredity, will certainly be much appreciated. General biological knowledge of this kind is often eagerly sought for by the student, and not always readily obtainable.

The microscope and the methods of microscopical research are very good and modern, but this is a subject which is hardly expected in a text-book of physiology. The histology of the tissues calls for no special comment.

In connection with the physiology of muscle, the object and use of the graphic method is explained with great care, very clear and good illustrations being given of the apparatus used. Muscle physiology itself is treated in considerable detail, to which is added the physiology of the electrical organ in fishes, containing the recent researches of Prof. Sanderson and Mr. Gotch. The physiology of smooth muscle is very scantily touched on, and the figures in connection with the heat produced by muscle are not correct; nor is any reference made to the observations of Ludwig and Meade Smith, on the heat produced in the mammalian muscle when tetanized under different conditions of blood-supply. Surely they are much more to the point than the observations of Billroth and Fick, which are only applicable to the organism as a whole.

The best feature in the chemical part of the work is the introduction of sections on the general chemical processes of the organism and on fermentation. With regard to the former, the paragraph devoted to reduction—as an important chemical process of the organism—is too short: the interesting observations of Ehrlich on the reducing powers of the tissues (as shown by the injection of alizarin-blue, endophenol-white) are surely worthy of mention. The undoubted fact that the blood of asphyxiated animals contains reducing substances is not alluded to, nor is the *rôle* which modern physiological chemists ascribe to these reducing substances in producing nascent oxygen, and so bringing about the oxidations of the tissues, pointed out with sufficient clearness. Fermentation is considered in its historic aspect, and from its chemical and biological sides. The history of organized ferments is adequately treated, as are also the early and important observations of Pasteur. What we actually know about the relationship of enzymes and organized ferments is not clearly expressed, no account being given of the researches of Musculus, Lea, and others, which have shown that enzymes can be obtained from organized ferments. Nor is the question of the chemical nature of enzymes sufficiently discussed.

The remainder of the section of chemistry contains numerous defects. Thus a long chapter is devoted to the signification of chemical formulæ, but we are later told of the albumins that their "chemical constitution oscillates round the following: $C_{54}H_7N_{16}O_{22}S$." No mention is made of the observations of Schmiedeberg, Drechsel, or Grubler, on artificial albumin crystals—observations of the highest importance for all future work on proteids. The accounts given of casein, mucin, and nuclein are not in accordance with our present knowledge. The chemical relations of indigo are given in detail, but the indican of the urine is said to have the formula $C_{26}H_{31}NO_{17}$, and no mention is made of indoxyl potassium sulphate. So with uric acid, nothing is said about the most important facts of Horbaczewski and E. Ludwig on the formation of uric acid from glycocoll and urea, which correspond so well with Strecker's view of uric acid as a body analogous with hippuric acid (the benzoic acid being replaced by cyanic), and with the remarkable physiological fact observed by Wöhler, that calves, as long as they feed on milk, excrete only uric acid, and no hippuric, whilst the reverse is the case when they take to a vegetable diet. Again, in regard to the formation of uric acid, two extremely important researches have been made—that of Schroeder on the influence of ammonia salts in producing uric acid in birds, and the remarkable confirmation of this by Minkowski, who found, after extirpation of the liver, the uric acid of the bird's urine replaced by ammonia.

The subject most fully treated is that of the pigments, but here again the important works of Nencki and Sieber on hæmoglobin and its derivatives, find no mention. A work like the present is necessarily a compromise. It does not give so equable and well-judged an account of what it is important to know in physiology as might be expected from the reputation of the author and the size of the book; but it shows the judgment of an experienced teacher in endeavouring to make every subject perfectly intelligible and in leaving no branch of physiological science untouched. L. C. WOOLDRIDGE.

OUR BOOK SHELF.

The Mind of the Child. Part I. The Senses and the Will; Observations concerning the Mental Development of the Human Being in the First Year of Life. By W. Preyer, Professor of Physiology in Jena. Translated from the original German by H. W. Brown. "International Education Series." (New York: Appleton and Co. London: Whittaker and Co. 1888).

IT is with no small satisfaction that we notice the issue of this work in the English language. It has already remained much too long in the German and French tongues only; and it speaks ill for the enterprise of British publishers that now the name of Appleton appears upon the cover. For, although comparisons as a rule are invidious, in the present instance there can be no doubt that the work in question holds the first place in the literature of the subject with which it deals. And since the study of infant psychology was inaugurated by M. Taine and Mr. Darwin, it has become so popular a branch of scientific literature that an English translation of "Die Seele des Kindes" must be an assured success, even from a commercial point of view.

In the case of a book already so well known, it is needless to say much by way of analysis. We must remark, however, that the present volume comprises only Parts I. and II. of the original—the remainder being reserved for publication as a second volume. Hence the instalment of the translation now before us deals only with the senses and the will; the next instalment having to treat of the intellect, and all supplementary matter. As everyone who has read the original is aware, Prof. Preyer has devoted himself to his subject with an assiduity and a thoroughness which only an assured conviction of its importance could inspire. And, in the result, his patiently continuous observation, his skilled intelligence as a well-read psychologist, together with his high attainments as a professed physiologist, combine to render his work, not only as before remarked the most important, but also in many respects the most interesting, that has hitherto appeared upon the subject of psychogenesis. Therefore we recommend this work to all our English readers as the best that they can procure on "the mind of the child"—and this whether their interest in such a mind be scientific only or likewise parental.

G. J. R.

Arithmetical Exercises. By H. S. Hall, M.A., and S. R. Knight, B.A. (London: Macmillan and Co., 1888.)

IN this book we have a collection of examples comprising about eighty progressive miscellaneous exercises and a set of fifty papers taken from such examinations as the London University, Oxford and Cambridge Local, Previous Cambridge, Army Preliminary, &c. The examp'es are judiciously chosen, and great care seems to have been taken to make the work as progressive as possible. An appendix is added, consisting of two hundred graduated questions in logarithms and mensuration, preceded by a list of the numerical constants and formulæ used in the latter. The answers to the examples are all collected together at the end.

An Elementary Treatise on Mensuration. By E. T. Henchie. (London: School Books Publishing Company, 1888.)

IN this work we have an excellent treatise for those who are about to begin the study of this subject. All reference to trigonometry has purposely been avoided, and the author has in the second chapter added the enunciations of Euclid's propositions which bear on the work, together with an explanation of each.

Plain rectilinear figures, curvilinear areas, the circle, surfaces and volumes of solids, are dealt with in turn, and each chapter is accompanied by a set of illustrative examples thoroughly worked out and explained, followed by a separate set to be worked out by the student. Land surveying forms the subject of the eighth chapter, in which are described the various instruments with the methods of using them. The figures throughout are very clear, and the shading used in those of the chapter on solids is excellent. The book concludes with a set of miscellaneous examples, making in all about 1260, together with the answers to the above.

LETTERS TO THE EDITOR.

The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.*]

Lamarckism *versus* Darwinism.

I HAD hoped that my previous letter might have closed this correspondence, but Mr. Poulton's reply suggests to me the propriety of making one additional remark. This is, that while writing the sentence in the *Contemporary Review* to which he has taken exception, it never occurred to me that anyone would gather from it that I intended to disparage the work of an eminent man, who happens to be also a personal friend. But, as this appears to be the impression conveyed to Mr. Poulton, I should not like to allow his statement of it to pass unnoticed. As a matter of fact, no one can appreciate more thoroughly than I do the extensive knowledge, the clearness of thought, and the great powers of original research which are now being so conspicuously displayed by Prof. Weismann.

From the first it has been sufficiently obvious to me how the present misunderstanding arose; and if, instead of affirming that I was ignorant of Prof. Weismann's writings, Mr. Poulton had begun as he has ended, by asking me to "explain" my remark with reference to them, of course I should at once have done so. However, as stated in my last letter, it is my intention at no very distant date to deal with the whole question of so-called "Lamarckism *versus* Darwinism"; and, therefore, my only object in this communication is to stop from going further the impression that I hold in light esteem the highly important achievements of Prof. Weismann.

GEORGE J. ROMANES.

Geanies, Ross-shire, September 8.

Mr. Gulick on Divergent Evolution.

MR. GULICK'S paper on this subject appears in the last number of the Journal of the Linnean Society as having been "communicated by Alfred Russel Wallace, F.L.S." It may therefore be supposed that I recommended its publication, or that I agree with its main argument; and as this is not the case, I ask permission to say a few words on the subject in the columns of NATURE.

In 1872, Mr. Gulick sent me his paper on "Diversity of Evolution under One Set of External Conditions," requesting me, if I thought fit, to communicate it to the Linnean Society. As the paper contained a body of very interesting facts observed by the author, I had no hesitation in recommending its acceptance by the Society, although I did not agree with the conclusions Mr. Gulick drew from his facts.

Last year Mr. Gulick sent me the manuscript of his present paper, informing me that it was the result of long-continued study of the subject, and asking me to forward it to the Linnean Society. I did so, writing to the Secretary that I had not read the paper through, and did not undertake the responsibility of recommending it for acceptance.

Having now read the paper in print, I find very little in it that I can agree with. I can discover in it no additional facts beyond those which were set before us in the former paper sixeen years ago, while there is an enormous body of theoretical statements, many of which seem to me erroneous, and a highly complex classification of the conditions under which the separation or isolation of individuals of a species takes place, with a new and cumbrous terminology, neither of which, in my opinion, adds to our knowledge or comprehension of the matter at issue.

As in almost every page of this long paper I find statements which seem to me to be either disputable or positively erroneous, any extended criticism of it is out of the question ; but I wish to call attention to one or two points of vital importance. Mr. Gulick's alleged discovery is, "the law of cumulative divergence through cumulative segregation" (p. 212). He maintains that any initial variation, if isolated by any of the causes he has enumerated, but remaining under identically the same environment, will increase till it becomes in time a specific or even a generic divergence, and this without any action whatever of natural selection. Now if this is *a fact* it is a most important and fundamental fact, equal in its far-reaching significance to natural selection itself. I accordingly read the paper with continual expectation of finding some evidence of this momentous principle, but in vain. There is a most elaborate discussion and endless refined subdivisions of the varied modes in which the individuals constituting a species may be kept apart and prevented from intercrossing, but no attempt whatever to prove that the result of such complete or partial isolation is "cumulative divergence." The only passage which may perhaps be considered such an attempt at proof is that on p. 219, where he supposes an experiment to be made, and then gives us what he thinks "experienced breeders" will assure us would be the result. In this experiment, however, there is to be constant selection and reassortment of each brood, yet he asserts that "there is no selection in the sense in which natural selection is selection" ; by which he appears to mean that the selection is by "separation" not by "extermination." This, however, seems to me to be a distinction without a difference.

Again, in the various illustrations of how "cumulative segregation" is brought about, natural selection must always come into play—as in the case of a change in digestive powers, and consequent adoption of a different food (p. 223), leading to partial isolation ; and such cases are exactly what is contemplated by Darwin in his brief statement of the effects of "divergence of character" ("Origin," pp. 86–90), while the concurrence of "isolation" as a factor is fully recognized at pp. 81–83 of the same work (6th edition).

It appears to me that throughout his paper Mr. Gulick omits the consideration of the inevitable agency of natural selection, arising from the fact of only a very small proportion of the offspring produced each year possibly surviving. Thus when, at p. 214, he states that "the fact of divergence in any case is not a sufficient ground for assuming that the diverging form has an advantage over the type from which it diverges," he omits from all consideration the fact that at each step of the divergence there was necessarily selection of the fit and the less fit to survive ; and that if, as a fact, the two extremes have survived, and not the intermediate steps which led to one or both of them, it is a proof that *both* had an advantage over the original less specialized form. Darwin explains this in his section on "Extinction caused by Natural Selection" (p. 85). On the whole, I fail to see that Mr. Gulick has established any new principle, either as a substitute for, or in addition to, natural selection as set forth by Darwin. Others, however, may think differently ; and I shall be glad if any naturalists who have studied Darwin's works will point out, definitely, in what way this paper extends our knowledge of the mode in which species have originated.

ALFRED R. WALLACE.

The Death of Clausius.

I DO not know by what unfortunate accident it happened that I did not hear of the death of the great Clausius until after the meeting of the British Association. I write this in order to explain how I neglected to express the sorrow of the scientific world in Britain in the loss, and our sympathy with the scientific world in Germany. It is not the part of a young disciple like me to eulogize the giants of the passing generation, but I regret greatly that any appearance of want of appreciation of the labours of one of the most brilliant lights of the nineteenth century should attach to British science owing to my silence.

GEO. FRAS. FITZGERALD.

Trinity College, Dublin, September 15.

The March Storms.

THE accounts of March storms in England which reach us lead me to think that it would be interesting to note the following. On March 13, barometers in Western Australia had fallen suddenly 0·20 inch ; the cyclone passed rapidly eastward along the south coast of Australia. On the 15th we had a heavy gale of wind at Sydney ; the anemometer showed 55 miles an hour. Lake George was so disturbed that the observer was wind-bound in the small house which holds the recording machine for several days, and the tidal register at Sydney shows considerable disturbance like earthquake-waves during the 15th, 16th, and 17th. On the 15th the level of the Sydney transit instrument was found to have changed suddenly since the 14th, 0″·7, the western pier having fallen. A tidal wave reached New Guinea and New Britain on the 13th ; at the latter place it is supposed to have risen 40 feet.

H. C. RUSSELL.

Sydney Observatory, July 26.

INTERNATIONAL METEOROLOGY.

THE International Meteorological Committee held a meeting at Zürich, in the Polytechnikum, from the 3rd to the 5th of this month. All the members were present. The most important point on which action was taken was the subject of future meetings to be held instead of Meteorological Congresses organized by diplomatic means. The following was the resolution adopted :—

"The Committee, in view of the circumstance that the assembling of an international meeting, of the same character as the Congresses of Vienna and Rome, presents great difficulties, considers that the commission it received at Rome is exhausted, and that it ought to dissolve itself.

"At the same time, in order to continue the relations between the different meteorological organizations, which have been productive of such good results during a series of years, the Committee appoints a small bureau with the duty of using its best endeavours to bring about, at some convenient time, an international meeting of representatives of the different Meteorological Services."

By a subsequent resolution the bureau was made to consist of the President and Secretary of the Committee (Prof. Wild and Mr. Scott).

Among other matters on which action was taken may be mentioned :—

Cloud Classification.—It was decided that the proposals of Messrs. Hildebrandsson and Abercromby were not ripe enough to be recommended for general adoption.

Meteorological Information from Travellers.—On the motion of Dr. Hann certain rules were laid down, to be recommended to all Geographical Societies, &c., as to the conditions which must be observed in order to render published records of meteorological observations of any real service to meteorology. These relate to instruments and their corrections, exposure, methods of calculation, &c., &c.

The Committee finally dissolved itself.

ROBT. H. SCOTT.

Meteorological Office, September 19.

THE NORWEGIAN GREENLAND EXPEDITION.

INFORMATION having been received by the sealer *Jason* of the Norwegian Expedition under Dr. Fridtjof Nansen, which is to attempt traversing Greenland from the east coast to the west coast, having left that vessel on July 17 in lat. 65° 2′ N., and by this time is no doubt fairly on its way across the inland ice, some particulars of the plan and aim of this expedition, furnished by the leader himself, will doubtless prove of interest, and tend to correct various erroneous statements which have appeared.

When leaving the *Jason*, an ice-belt about ten miles in width separated the vessel from the mouth of the Sermilik Fjord, and the Expedition was seen to make good progress, either walking over the ice or rowing through it, and at 6 a.m. it was out of sight. It was Dr. Nansen's intention to land in this fjord, which is inhabited, and proceed to the bottom, where he would attempt to ascend to the inland ice plateau. The mountains around the fjord are very steep, and upwards of 6000 feet in height, but still this spot was recommended by the Danish explorer, Captain Holm, as the most suitable. It is agreed by all competent authorities that once on the inland ice plateau the rest of the journey will be comparatively easy, Dr. Nansen and his followers purposing to journey on the so-called Norwegian *Ski* across the smooth snowy surface of the inland ice. These adjuncts of locomotion are highly recommended by Baron Nordenskiöld in land journeys in the Arctic regions; and as a proof of their utility it may be mentioned that when on the inland ice in 1883, the two Lapps in his train were sent forward, and covered in fifty-seven hours twice as much ground as the rest of the expedition in twenty-seven days. Before, however, describing these means of locomotion on snow, a brief reference to the members of the Expedition should be made.

The Expedition, for which there were thirty-five volunteers, including one Englishman, consists of Dr. Fridtjof Nansen, of the Bergen Museum, leader; Lieutenant in the Norwegian army, Herr O. C. Dietrichson; Herr Otto Sverdrup, an officer in the Norwegian mercantile marine; and Herr Kristian Kristiansen, a land-owner; with two Lapps, Samuel Bulto and Ole Ravna, the latter of whom was "on view" at the Exhibition in London in 1883. All the members are men in their best years, powerful, and accustomed to hardships of all kinds, and last, not least, experts in the craft of *Skilöbning*, or Norwegian mode of journeying on snow. This mode is entirely different from that practised in Canada under the name of "snow-shoeing," and therefore deserves special mention. The *Ski*, or snow "runners," as they might more justly be called, are long strips of carefully selected pine-wood without a flaw, those used by Dr. Nansen being about 8 feet in length, 1 inch in thickness, and 4 inches in width. In the middle is a leather strap covered with sheep's wool for the foot, and a slight catch for the heel, whilst the edges are (in this particular case) protected by means of a steel band. The wood has been carefully seasoned and soaked in tar to prevent the penetration of moisture, whilst underneath the *Ski* are lined with reindeer skin, the hair of which gives the runner a better "grip" on the snow when going up hill. In front they are pointed and bent slightly upwards, so as to pass more easily over obstacles. A good pair of *Ski* will, when carefully prepared, have the elasticity almost of a Toledo blade, and jumps of 25 or 30 feet, when such may be necessary in the mountains, are frequently performed by good *Ski* men, without breaking their *Ski*. The most remarkable feats of agility are performed by experts on these means of locomotion; in fact, many a Norwegian is as much at home on his *Ski* as a Red Indian on his horse. As to the progress made on *Ski*, it is simply astounding, a good runner on dry snow, and across a fair country, being capable of covering a hundred miles a day, and down hill the speed rivals that of the fastest express. Dr. Nansen and his party, who are all celebrated for their achievements in the *Ski* sport, carry with them nine pairs of these. For the conveyance of provisions he has with him five hand sledges of novel construction, being only half the weight of those generally carried in Arctic journeys. They are 9 feet long, and 2 feet wide, greatly curved at both ends, and shod with steel bands, whilst at the back is a steering-pole. The weight is 25 pounds. Dr. Nansen had occasion to test the quality of one of these sledges when travelling last winter alone across Norway on *Ski*, from Eidsfjord to Nummedal, a distance of about fifty miles. The adoption of this kind of sledge has been made at the instance of Baron Nordenskiöld, who, during his journey across the inland ice, found those then used too heavy. The Expedition is also provided with a tent, brown in colour, in order to afford a rest to the eye on the vast dazzling snow-fields, and it may be separated into five pieces, each forming a sail for the boats. Naturally it was absolutely necessary that the baggage of the Expedition should be as small as possible, consequently only what is absolutely required has been included, such as the usual scientific apparatus, a camera, cooking utensils, and provisions, the latter consisting chiefly of pemmican, meat cakes and biscuits, preserves, tea, chocolate, &c. Every article carried has been specially prepared, some in Christiania, and others in Copenhagen, London, and Paris. One article which previous Greenland Expeditions have been much in want of are Alpine ropes for use in climbing, and these have been specially made for Dr. Nansen in London.

Having reached the inland ice plateau, Dr. Nansen purposes travelling in a north-westerly direction, with Disco Bay on the west coast for his goal, as further south the land is intersected by deep fjords and mountains, which may cause difficulties in crossing. The distance from coast to coast is estimated at 425 miles, and allowing for a rate of progress of only fifteen miles a day, the whole journey should be accomplished in about thirty days. The leader considers it a great advantage to cross from east to west, and not *vice versâ* as previously attempted, as in the former case provisions need only be carried for *one* journey, the west coast being well provided in this respect. The most serious obstacles expected by Dr. Nansen on the inland ice are crevices in the ice, which are formed by the water from the melting snow, and wet snow. The former he intends to attempt evading by sending the Lapps forward as scouts, and on the latter Canadian snow-shoes will be used, as in wet snow the *Ski* are of little use, the snow clogging to them and retarding progress. It is, however, expected that at this season the snow will be crisp and dry. It should also be mentioned that by crossing from east to west the Expedition will have the advantage of travelling continually down an incline, as the country slopes gradually down from a height of 6000 feet on the east coast to only a few hundred on the west coast, whilst the wind also nearly always blows from that quarter.

Dr. Nansen further anticipates that the curious lofty basalt rocks of Disco Island will be seen a good way inland, and serve as a landmark.

As regards the scientific aspects of the expedition, not too great results may be expected, although Dr. Nansen has especially qualified for his task, and visited Greenland some years ago; as with the means at his disposal, and in view of the mode of travelling, the number of members and the weight of the baggage had to be scrictly limited. However, the leader feels confident that it will contribute in some degree to solve the scientific problems facing us in that continent, which has always had such fascination to the geographer, geologist, and botanist in particular, and may lead to the despatch of an Expedition on a larger scale and with a wider scientific scope.

It may be of interest here briefly to recall the attempts which have been made from time to time to cross the Greenland continent.

As is well known, Greenland has never been crossed by human being, although there is a tradition, confirmed by Holm and Garde, that a young girl from Pikiudelek, on the east coast, driven from home by cruelty, wandered on foot across the ice to the west coast. However, in modern times many attempts have been made, as, for instance, by Dalager (a Dane), Dr. John Rae, Messrs. Whymper and Brown, Messrs. Jensen, Kornerup, and Groth, and Nordenskiöld in 1870 and 1883. All of these attempts were failures, with the exception of that of Nordenskiöld in 1883 referred to, when he succeeded, in lat. 68½° N., in reaching 75 miles inland, and his two Lapps 140 miles further, or 215 miles, *i.e.* a little more than half the width of the country. Finally, we have the scantily-known wandering, in June of last year, of Mr. Peary, an Amerian engineer, and Herr Maigaard, a Dane, who claim to have reached about 100 miles inland on the ice from Jakobshavn, and reached an elevation of about 7000 feet above the sea; but the weather was unfavourable. It is worthy of note that this elevation is far higher than that recorded by Nordenskiöld a little further south, viz. about 6000 feet.

It is impossible to close this *résumé* of Dr. Nansen's plans without referring to the much-disputed theory of there being, if not a fertile interior somewhere in Greenland, at all events land free from ice and snow, as advocated by Nordenskiöld, but which he failed to find. We have it however now, on the authority of Dr. Nansen, that in spite of this failure the famous Swedish explorer is still of opinion that such conditions may exist somewhere to the north or south of the track followed by himself. Dr. Nansen also supports this theory, which is, leaving the "Föhn" wind theory out of the question, based, firstly, on the circumstance that the reindeer herds on the west coast disappear from the coast in the summer, when it is surmised that they proceed to this interior "oasis," as it has been termed; and, secondly, on the discovery by Nordenskiöld of reindeer horn far in on the ice; thirdly, the theory is claimed to be supported by the fact of Nordenskiöld's two Lapps having in the middle of Greenland seen two ravens coming from the north to "have a look at them," and return in the same direction. Hence, it is maintained, some ice-free land must exist further north. But as to the wanderings of the reindeer, such take place every summer in Norway, when the animals repair to the glaciers in order to escape from their dread tormentors the gadfly and the heat. It is, however, curious that the Greenlanders themselves, as well as the Eskimo, according to Captain Holm, firmly believe in an ice-free and populated interior, the inhabitants of which are of enormous stature, fierce, and dangerous magicians, and it is this latter belief which is the cause of the natives refusing to act as guides or participate in explorations of the interior. The east coast natives by the way maintain, too, that Scoresby Sound in the extreme north (Holm, "East Coast Expedition, 1883–85") is a fjord separating Greenland from the rest of the Arctic regions; that once a Greenlander sailed through it from west to east, and that near its southern shores resides a warlike tribe of Greenlanders.

It was Dr. Nansen's intention to have attempted to land in the neighbourhood of Scoresby Sound, where no European has ever set foot, but it was impossible to get further north than Cape Dan on account of ice. It should be mentioned that the present expedition is in a great degree due to the munificence of Herr Augustus Gamél, of Copenhagen, who despatched Lieutenant Hovgaard's Arctic Expedition of 1880, and has received valuable assistance from such Greenland explorers as Nordenskiöld, Rink, Holm, Ryder, and Marigaard, as well as the Royal Geographical Society.

If all goes well, it may return to Europe before the last vessel leaves Greenland at the end of September.

If successful, it cannot fail to throw some further light upon the interesting scientific problems of that mystic northern continent, and incite other explorers to follow in Dr. Nansen and his colleagues' footsteps.

THE CENTENARY OF THE CALCUTTA BOTANIC GARDEN.

THE Report of Dr. George King, the Superintendent of the Botanic Garden of Calcutta, for the past year gives a brief history of the work of that institution during the century of its existence, which has just been completed. The suggestion for its foundation was made to the Government in Calcutta in 1786 by Colonel Robert Kyd, then Superintendent of the East India Company's dockyard at Kidderpore. The adoption of the proposal was urged upon the Board in London by the Governor-General, and upon their sanctioning it a large piece of land at Shalimar was chosen as the site, and Colonel Kyd was elected the first Superintendent. He held the post till his death in 1793. At the outset it was understood that the Garden was to be made a source of information for the Company's servants, and a place in which experiments could be made on those exotics which were of economic value. It was also intended to be a horticultural and agricultural garden, which would assist in introducing indigenous Indian products to new markets. The earliest efforts of Colonel Kyd were directed to the introduction of trees yielding nutmeg, cloves, and cinnamon, and to attempt to cultivate them. This, however, was a failure, the climate being shown to be quite unsuitable to them. The equatorial fruits, such as mangosteen and bread-fruit were tried with a similar result, and also the temperate fruits of Europe, and thus at an early stage it was demonstrated that any such effort was quite useless. Colonel Kyd introduced tea cultivation, and in this he was highly successful, and it was owing to his efforts that the tea-industry has become one of the most important in India. On the death of Colonel Kyd, Dr. William Roxburgh, the Company's Botanist in Madras, was appointed to the post, and continued in it till 1814. He was an ardent botanist, and was the first who attempted to draw up a systematic account of the plants of India. His *Flora Indica* contained descriptions of all the indigenous plants he had met, and also of the exotics in cultivation at Calcutta. This book was not published till 1832, and it was, till Sir Joseph Hooker commenced his work on the "Flora of British India" in 1872, the only book from which a good knowledge of Indian plants could be acquired. Besides his "Flora Indica," Roxburgh published "Plantæ Coromandalianæ," descriptions of three hundred of the most representative plants on the Coromandel Coast. Dr. Roxburgh, who left India on account of failing health, was succeeded by Dr. Buchanan-Hamilton, who collected a mass of information about the fauna and flora of India, a portion of which he published in his own name, but the greater part was issued in Montgomery Martin's "History, Topography, and Statistics of Eastern India." In 1817, Dr. Wallich became Superintendent. Wallich was a most energetic man, and during his term of office he made collections in Kumaon, Nepal, Tenasserim, Singapore, Penang, and other places. His collections of dried plants were taken by him to London, and after their classification they were distributed to the chief botanical institutions in Europe. Dr. Wallich published three fine volumes, "Plantæ Asiaticæ Rariores," illustrated with excellent figures. On Dr. Wallich's retirement in 1846, Dr. Hugh Falconer, who is well known on account of his researches on the Sivalik fossil Mammalia, succeeded to the post. Dr. Thomas Thomson, the co-worker of Sir Joseph Hooker in the collection and distribution of an extensive East Indian Herbarium, was the next Superintendent. His successor, Dr. Thomas Anderson, died in 1870 from disease contracted when labouring to introduce the quinine-yielding Cinchonas into the Himalayas. This latter work—that is, the cultivation of the Cinchonas of the Andes—has been a great success. The Garden authorities, in connection with the Agri-Horticultural Society of India, made great and successful efforts to improve the quality of Indian cotton, and to push its sale and that of jute in the European markets. The united bodies also imported better kinds of sugar-cane from the West Indies, and thus improved the quality and the amount of the sugar-crop in India. The various Superintendents made from time to time experiments in the cultivation of plants and products of economic value, as, for instance, tapioca, india-rubber, sarsaparilla, aloes, cocoa, and many others. Many of the various kinds of exotics now grown in India have been introduced through the instrumentality of the Garden, and the authorities have shown to the inhabitants of India the advantages of better systems of cultivation than they previously pursued.

In the year 1864 the Garden was devastated by a terrible cyclone, and the few plants that escaped the general ruin were very much thinned by another cyclone which a few years after burst over Calcutta. In fact, at the present moment there are in the Garden only a few trees, including the great banyan, which were there in 1867. When the shade of the trees was thus removed, the weed *Imperata cylindrica* spread rapidly over the whole Garden, and when Dr. King was appointed to be Superintendent of the Garden, in 1871, he found it in rather a sorry plight. By the assistance that the local authorities gave him he was enabled to plant it afresh, to lay it out for landscape effect, to form ornamental ponds, and to build the Herbarium and conservatories. The most noticeable feature from a botanical stand-point is, of course, the Herbarium. On Dr. Wallich dispersing in 1828 the splendid collection of dried plants, the foundations of another were laid. Almost every botanical student in India has contributed to the present collection, and also many specimens have been sent from Europe. Of course it is above all an Indian Herbarium, but there are also good collections of plants from Asia Minor, Persia, Japan, and South-Eastern Asia. In fact, in all but African and American plants it is a very representative collection. For the last fifty years there has been a constant exchange of specimens with Kew Gardens, and to Sir William Hooker, and Sir Joseph Hooker, and Mr. Thiselton Dyer, the Herbarium owes some of its choicest specimens. Exchanges have also been systematically made with the British Museum Herbarium, the Jardin des Plantes, Paris, the Imperial Gardens at Berlin and St. Petersburg, and with the institutions at Ceylon, Java, and Saharanpore, and many of the best-known botanists have been among the most active contributors.

During the past year the collection of dried plants has been largely increased, the most noteworthy additions being those collected by Dr. Aitchison with the Afghan Boundary Commission, and those by Dr. Giles during the Gilgit expedition, the latter having been sent from Kew. From Kew were also received many specimens of Singapore and Penang plants. Many plants from Central Asia were sent by the Director of the Imperial Garden at St. Petersburg, and a Natal collection was sent from Durban. Four hundred named species from Mexico, a large box of dried plants from New Guinea, a quantity of plants from Sikkim, trees from the Khasia Hills, specimens from the North-Western Himalayas, and from Southern India, were among the many collections presented to the Garden in the past year. The Government Botanist of Perak, Father Scortechini, who had been sent by Sir H. Low, came to the Garden in November to study, so that he might arrange his collections, but he died shortly after his arrival. During the year 8064 plants were received and 46,109 given out; 903 packets of seeds were received, and 2534 distributed. Dr. King concludes his Report by saying that the acclimatized English potatoes have everywhere turned out badly the past season.

THE BRITISH ASSOCIATION.

SECTION G.

MECHANICAL SCIENCE.

OPENING ADDRESS BY WILLIAM HENRY PREECE, F.R.S., M.INST.C.E., &c., PRESIDENT OF THE SECTION.

"CANST thou send lightnings, that they may go, and say unto thee, Here we are?" were pregnant words addressed to Job unknown centuries ago. They express the first recorded idea in history of the potentiality of electricity to minister to the wants of mankind. From Job to Franklin is a long swing in the pendulum of time. It was not until that American philosopher brought down atmospheric electricity by his kite-string in 1747, and showed that we could lead it where we willed, that we were able to answer the question addressed to the ancient patriarch. Nearly another century elapsed before this mysterious power of Nature was fairly conquered. It has been during this generation, and during the life of the British Association, that electricity has been usefully employed; and it is because I have taken a subordinate position in inaugurating nearly all of its practical applications, that I venture to make the developments of them the text of my address to this Section.

People are singularly callous in matters affecting their own personal safety: they will not believe in mysteries, and they ridicule or condemn that which they do not understand. The Church itself set its face against Franklin's "impious" theories, and he was laughed to scorn by Europe's scientific sons; and even now, though Commissions composed of the ablest men of the land have sat and reported on Franklin's work in England, France, and nearly every civilized nation, the public generally remains not only ignorant of the use of lightning-conductors, but absolutely indifferent to their erection, and, if erected, certainly careless of their proper maintenance. I found in a church not very far from here the conducter leaded into a tombstone, and in a neighbouring cathedral the conductor only a few inches in the ground, so that I could draw it out with my hand. Although I called the attention of the proper authorities to the absolute danger of the state of affairs, they remained in the same condition for years.

Wren's beautiful steeple in Fleet Street, St. Bride's, was well-nigh destroyed by lightning in 1764. A lightning-rod was fixed, but so imperfectly that it was again struck. In July last (1887) it was damaged because the conductor had been neglected, and had lost its efficiency.

As long as points remain points, as long as conductors remain conductors, as long as the rods make proper connection with the earth, lightning protectors will protect: but if points are allowed to be fused, or to corrode away; as long as bad joints or faulty connections are allowed to remain; as long as bad earths, or no earths exist, so long will protectors cease to protect, and they will become absolute sources of danger. Lightning-conductors, if properly erected, duly maintained, and periodically inspected, are an absolute source of safety; but if erected by the village blacksmith, maintained by the economical churchwarden, and never insp cted at all, a loud report will some day be heard, and the beautiful steeple will convert the churchyard into a new geological formation.

We have not yet acquired that mental confidence in the accuracy of the laws that guide our procedure in protecting buildings from the effects of atmospheric electrical discharges which characterizes most of the practical applications of electricity. Some of our cherished principles have only very recently received a rough shaking from the lips of Prof. Oliver Lodge, F.R.S., who, however, has supported his brilliant experiments by rather fanciful speculation, and whose revolutionary conclusions are scarcely the logical deduction from his novel premises. The whole subject is going to be thoroughly discussed at this meeting.

We are now obtaining much valuable information about the nature of lightning from photography. We learn that it does not, as a rule, take that zigzag course conventionally used to represent a flash on canvas. Its course is much more erratic and sinuous, its construction more complicated, and pictures have been obtained of *dark* flashes whose *raison d'être* has not yet been satisfactorily accounted for. The network of telegraph wires all over the country is peculiarly subject to the effects of atmospheric electricity, but we have completely mastered the vagaries of lightning discharges in our apparatus and cables. Accidents are now very few and far between.

The art of transmitting intelligence to a distance beyond the reach of the ear and the eye, by the instantaneous effects of electricity, had been the dream of the philosopher for nearly a century, when in 1837 it was rendered a practical success by the commercial and far-sighted energy of Cooke, and the scientific knowledge and inventive genius of Wheatstone. The metallic arc of Galvani (1790) and the developments of Volta (1796) had been so far improved that currents could be generated of any strength; the law of Ohm (1828) had shown how they could be transmitted to any distances; the deflection of the magnetic needle by Oersted in 1819, and the formation of an electro-magnet by Ampère and Sturgeon, and the attraction of its armature, had indicated how those currents could be rendered visible as well as audible.

Cooke and Wheatstone in 1837 utilized the deflection of the needle to the right and the left to form an alphabet. Morse used the attraction of the armature of an electro-magnet to raise a metal style to impress or emboss moving paper with visible dots and dashes. Steinheil imprinted dots in ink on the different sides of a line on paper, and also struck two bells of different sound to affect the ear. Bréguet reproduced in miniature the actual movements of the semaphore, then so much in use in France; while others rendered practical the favourite idea of moving an indicator around a dial, on which the alphabet and the numerals were printed, and causing it to dwell against the symbol to be read—the A, B, C instrument of Wheatstone in England, and of Siemens in Germany. Wheatstone conceived the notion of printing the actual letters of the alphabet in bold Roman type on paper—a plan which was made a perfect success by Hughes in 1854.

At the present moment the needle system of Cooke and Wheatstone, as well as the A, B, C dial telegraph, are very largely used in England on our railways and in our smaller post-offices. The Morse recorder and the Hughes type-printer are universally used on the Continent; while in America the dot-and-dash alphabet of Morse is impressed on the consciousness through the ear by the sound of the moving armature striking against the stops that limit its motion. In our larger and busier offices the Morse sounder and the bell system, as perfected by Bright, are very largely used, while the Press of this country is supplied with news which is recorded on paper by ink dots and dashes at a speed that is almost fabulous.

Sir William Thomson's mirror—the most delicate form of the needle system—where the vibratory motions of an imponderable ray of light convey words to the reader; and his recorder, where the wavy motion of a line of ink spirted on paper by the frictionless repulsion of electricity perform, the same function, are exclusively employed on our long submarine cables.

Bakewell, in 1848, showed how it was possible to reproduce facsimiles of handwriting and of drawing at a distance; and, in 1879, E. A. Cowper reproduced one's own handwriting, the moving pen at one station so controlling the currents flowing on the line wire that they caused a similar pen to make similar motions at the other distant station. Neither of these plans, the former beautifully developed by Caselli and D'Arlincourt, and the latter improved by Robertson and Elisha Gray, have yet reached the practical stage.

The perfection of telegraphy has been attained by that chief marvel of this electrical age—the speaking telephone of Graham Bell. The reproduction of the human voice at a distance, restricted only by geographical limits, seems to have reached the confines of human ingenuity; and though wild enthusiasts have dreamt of reproducing objects abroad visible to the naked eye at home, no one at the present moment can say that such a thing is possible, while in face of the wonders that have been done no one dare say that it is impossible.

The commercial business of telegraphy, when our thoughts and wishes, orders and wants, could be transmitted for money, was inaugurated in this country by the establishment of the Electric Telegraph Company in 1846, and until 1870 it remained in the hands of private enterprise, when it was purchased by the Government, and placed under the sole control of the Postmaster-General. It has been the fashion to decry the terms of purchase of the various undertakings then at work by those who have not understood the question, and by those who, being politically opposed to the Government in power at the time, saw all their acts, not only through a glass darkly, but through a reversing lens. A business producing £550,000 per annum was bought at twenty years' purchase, and that business has now increased to £2,000,000 per annum. 6,000,000 messages per annum have increased to 52,000,000.

Every post-office has been made a telegraph-office, every village of any size has its wire; messages which used to cost 12*s*. 6*d*. are now sent for 6*d*.; a tariff which was vexatious from its unfair variation is now uniform over the United Kingdom, and no one can justly complain of error or delay in the transmission of their messages. Silly complaints are sometimes inserted in the Press, of errors which the most elementary knowledge of the Morse alphabet would detect, and little credit is given to the fact that the most perfect telegraph is subject to strange disturbances from terrestrial and atmospheric causes which admit sources of error beyond the control of the telegraphist. A flash of lightning in America may cause an extra dot in Europe, and *man* may become *war*. An earthquake in Japan may send a dash through France, and *life* would become *wife*. A wild goose flying against a telegraph-wire might drive it into momentary contact with another wire, and *sight* might become *night*. Everyone should know his Morse alphabet, and people should learn how to write. Nine-tenths of the errors made are due to the execrable calligraphy of the present day. As a matter of fact, in ninety-nine cases out of a hundred, the telegraphist delivers to the editor of a newspaper "copy" far more accurate than the first proof of his own leader submitted by the printer. The quantity of news transmitted is enormous: an average of 1,538,270 words are delivered per day. The recent Convention in Chicago, when the Republican party of the United States nominated their candidate for the Presidentship, created so much business that every American paper has chronicled this big thing as unique. 500,000 words were sent on one night; but we in England, when Mr. Gladstone introduced his celebrated Home Rule Bill on April 8, 1886, sent from the Central Telegraph Office in London 1,500,000 words.

The growth of business has led to vast improvement in the carrying capacity of the wires. Cooke and Wheatstone required five wires for their first needle instrument to work at the rate of four words per minute. One wire can now convey six messages at ten times the speed. The first Morse apparatus could work at about five words a minute: we now transmit news at the rate of 600 words a minute. In 1875 it was thought wonderful to transmit messages to Ireland at 80 words a minute. When I was recently in Belfast I timed messages coming at the rate of 461 words a minute. Duplex working—that is, two messages travelling on the same wire at the same time in opposite directions, the invention of Gintl, of Vienna—is now the normal mode of working; Edison's quadruplex is common; and the Delany system of multiplex working is gradually being introduced, by which six messages are indiscriminately sent in either direction. The telegraphic system of England has been brought to the highest pitch of perfection. We have neither neglected the inventions of other countries, nor have we been chary of exercising inventive skill ourselves, and we have received our full meed of that reward which is always freely bestowed on a British Government official, neglect and abuse. All parts of the civilized world are now united by submarine cables. The *Times* every morning has despatches from every quarter of the globe, giving the news of the previous day. 110,000 miles of cable have been laid by British ships, and nearly £40,000,000 of British capital have been expended by private enterprise in completing this grand undertaking. A fleet of 37 ships is maintained in various oceans to lay new cables and to repair breaks and faults as they occur—faults that arise, among other causes, from chafing on coral reefs, ships' anchors, the onslaught of insects, and earthquakes. The two cables connecting Australia and Java were recently simultaneously broken by an earthquake.

The politician, unmindful of the works of the engineer, is apt to apply to the credit of his own proceedings the growing prosperity of the world. The engineer, however, feels that steam and electricity in his hands have done more to economize labour, to cheapen life, to increase wealth, to promote international friendship, to alleviate suffering, to ward off war, to encourage peace, than all the legislation and all the verbosity of the politician.

The railways of this country are entirely dependent for the conduct of their traffic on the telegraph, and the security of their passengers is mainly due to the working of the block system. A railway—say between London and Bath—is broken up into certain short sections, and only one train is allowed on one section at one time. The presence, motion, and departure of trains are announced and controlled by electric signals, and the out-door signals are governed by these electric signals. There are few more interesting places to visit than a well-equipped signal-box on one of our main railways. The signalman is able to survey the lines all around and about him by aid of his electric signals; he can talk by telegraph or by telephone to his neighbours and his station-master; he learns of the motion of the trains he is marshalling by the different sounds of electric bells; he controls his out-door signals by the deflection of needles, or the movement of miniature semaphores; he learns the true working of his distant signals by their electrical repetition; machinery governs and locks every motion he makes, so that he cannot make a mistake. The safety of railway travelling is indicated by the fact that, while in the five years ending 1878 thirty-five people were killed annually from causes beyond their own control, in the five years ending 1887 the average has been reduced to sixteen. One person is killed in 35,000,000 journeys made by train. Wherever we are dependent on human agency we are subject to human error, and a serious accident very recently at Hampton Wick has shown how the most perfect machinery may be rendered valueless to protect life when perversity, thoughtlessness, or criminality enter as factors into the case.

At the meeting of the Association in Plymouth in 1877, I was able for the first time in this country to show the telephone at work. Since then its use has advanced with giant strides. There are probably a million instruments at work now throughout the civilized world. Its development has been regularly chronicled at our meetings. As far as the receiving part of the apparatus is concerned, it remains precisely the same as that which I brought over from America in 1877; but the transmitter, ever since the discovery of the microphone by Hughes in 1878, has been entirely remodelled. Edison's carbon transmitter was a great step in advance; but the modern transmitters of Moseley, Berliner, D'Arsonval, De Jongh, leave little to be desired. The disturbances due to induction have been entirely eliminated, and the laws regulating the distance to which speech is possible are so well known, that the specification of the circuit required to connect the Land's End with John o' Groats by telephone is a simple question of calculation. A circuit has been erected between Paris and Marseilles, 600 miles apart, with two copper wires of 6½ gauge, weighing 540 pounds per mile, and conversation is easily maintained between those important cities at the cost of three francs for three minutes. One scarcely knows which fact is the more astounding—the distance at which the human voice can be reproduced, or the ridiculously simple apparatus that performs the reproduction. But more marvellous than either is the extreme sensitiveness of the instrument itself, for the energy contained in one heat unit (gramme-water-degree) would, according to Pellat, maintain a continuous sound for 10,000 years.

The influence which electric currents exert on neighbouring wires extend to enormous distances, and communication between trains, and ships in motion, between armies inside and outside besieged cities, between islands and the main-land, has become possible without the aid of wires at all, by the induction which is exerted through space itself. On the Lehigh Valley Railway, in the United States, such a system of telegraphing without wires is in actual daily use.

The conduct of the telephonic business in England is still in the hands of those who hold the patents, and who maintain a most rigid monopoly. These patents have only a short period to run, and when they expire we may expect to find that England will not occupy the very retired position she holds now as a telephone country. Stockholm has more subscribers than London; there are 15,000 subscribers in and about New York, while the number in London is only 4851.

Electric lighting has become popular, not alone from the beauty of the light itself, but from its great hygienic qualities in maintaining the purity and coolness of the air we breathe. The electric light need not be more brilliant than gas, but it must be more healthy. It need not be cooler than a wax candle, but it must be brighter, steadier, and more pleasant to the eye. In fact, it can be rendered the most perfect artificial illuminant at our disposal, for it can illumine a room without being seen directly by the eye; it can be made absolutely steady and uniform without irritating the retina; it does not poison the air by carbonic acid and carbonic oxide, or dirty the decorations by depositing unconsumed carbon; it does not destroy books or articles of vertu and art by forming water which absorbs sulphur acids; and it does not unnecessarily heat the room.

In our Central Savings Bank in London it has been found, after two years' experience of electric lighting, that the average amount of absences from illness has been diminished by about two days a year for each person on the staff. This is equivalent to a gain to the service of the time of about eight clerks in that department alone. Taking the cost at the "overtime" rate only, this would mean a saving in salaries of about £640 a year. The cost of the installation of the electric light was £3349, and the annual cost of working £700 per annum, say a total annual cost of £1034. The cost of the gas consumed for lighting purposes was about £700 a year, so that on the whole there was a direct saving of something like £266 a year to the Government, besides the material advantage of the better work of the staff resulting from the improved atmospheric conditions under which their work is done.

The production of light by any means implies the consumption of energy, and this can be measured in *watts*, or the rate at which this energy is consumed. A watt is $\frac{1}{746}$ part of a *horse-power*. It is a very convenient and sensible unit of power, and will in time replace the meaningless horse-power.

One candle light maintained by		tallow . . .	absorbs	124	watts.
,,	,,	wax	,,	94	,,
,,	,,	sperm . . .	,,	86	,,
,,	,,	mineral oil .	,,	80	,,
,,	,,	vegetable oil .	,,	57	,,
,,	,,	coal gas . . .	,,	68	,,
,,	,,	cannel gas . .	,,	48	,,
,,	,,	electricity (glow)	,,	3	,,
,,	,,	electricity (arc)	,,	55	,,

The relative heat generation of these illuminants may be estimated from these figures.

Though the electric light was discovered by Davy in 1810, it was not until 1844 that it was introduced into our scientific laboratories by Foucault; it was not until 1878 that Jablochkoff and Brush showed how to light up our streets effectually and practically; it was not until 1881 that Edison and Swan showed how our homes could be illuminated softly and perfectly. Unpreparedness for such a revolution produced a perfect panic among gas proprietors; inexperience in the use of powerful electric currents resulted in frequent failure and danger; speculation in financial bubbles transferred much gold from the pockets of the weak to the coffers of the unscrupulous; hasty legislation in 1882 restricted the operations of the cautious and the wise; and the prejudice arising from all these causes has, perhaps fortunately, delayed the general introduction of electricity; but now legislation has been improved, experience has been gained, confidence is being restored, and in this beautiful town of Bath fifty streets are about to be lighted, and we see everywhere around and about us in our English homes the pure glow-lamp replacing filthy gas and stinking oil. The economical distribution of the electric current over large areas is annually receiving a fresh impetus. The expensive systems defined in the Act of Parliament of 1882 have entirely disappeared. Hopkinson in England, and Edison in America, showed how a third wire reduced the weight of copper needed by 66 per cent. Gaulard and Gibbs in 1882 showed how the conversion of alternate currents of high electromotive force to currents of low electromotive force by simple induction coils would enable a mere telegraph wire to convey sufficient electricity to light a distant neighbourhood economically and efficiently. Lane Fox in 1879 showed how the same thing could be done by secondary batteries; and Planté, Faure, Sellon, and Parker have done much to prove how batteries can be made to solve the problem of storage; while King and Edmunds have shown how the distribution by secondary batteries can be done as economically as by secondary generators. The Grosvenor Gallery Company in London have proved the practicability of the secondary generator principle by nightly supplying 24,000 glow-lamps scattered over a very wide area of London. The glow-lamp of Edison, which in 1881 required 5 watts per candle, has been so far improved that it now consumes but $2\frac{1}{4}$ watts per candle. The dynamo, which in the same year weighed 50,000 pounds, absorbed 150 horse power, and cost £4000 for 1000 lamps, now weighs 14,000 pounds, absorbs 110 horse-power, and costs £500 for the same production of external energy; in other words, its commercial output has been increased nearly six times, while its prime cost has been diminished eight times.

The steam-engine has received equal attention. The economy of the electric light when steam is used depends almost entirely on the consumption of coal. With slow-speed low-pressure engines one kilowatt (1000 watts, $1\frac{1}{3}$ horse-power) may consume 12 pounds of coal per hour; in high-speed high-pressure triple-expansion engines it need not consume more than 1 pound of coal per hour. Willans and Robinson have actually delivered from a dynamo one kilowatt by the consumption of 2 pounds of coal per hour, or by the condensation of 20 pounds of steam.

There is a great tendency to use small economical direct-acting engines in place of large expensive engines, which waste power in countershafting and belts. Between the energy developed in the furnace in the form of heat, and that distributed in our rooms in the form of light, there have been too many points of waste in the intermediate operations. These have now been eliminated or reduced. Electricity can now be produced by steam at 3*d.* per kilowatt per hour. The kilowatt-hour is the Board of Trade unit as defined by the Act of 1882, for which the consumer of electric energy has to pay. Its production by gas-engines costs 6*d.* per kilowatt-hour, while by primary batteries it costs 3*s.* per kilowatt-hour. The Grosvenor Gallery Company supply currents at $7\frac{1}{2}d.$ per kilowatt-hour; a 20 candle-power lamp consuming 3 watts per candle, and burning 1200 hours per annum, expends 82,000 watt-hours or 82 kilowatt-hours, and it costs, at $7\frac{1}{2}d.$ per unit, 50*s.* per annum. If the electricity be produced on the premises, as is the case in the Post Office, in the House of Commons, and in many large places, it would cost 20*s.* 6*d.* per annum. I have found from a general average under the same circumstances and for the same light in the General Post Office in London that an electric glow-lamp costs 22*s.* and a gas-lamp 18*s.* per annum. The actual cost of the production of one candle light per annum of 1000 hours is as follows:—

	s.	*d.*
Sperm candles	8	6
Gas (London)	1	3
Oil (petroleum)	0	8
Electricity (glow)	0	9
Electricity (arc)	0	$1\frac{1}{4}$

The greatest development of the electric light has taken place on board ship. Our Admiralty have been foremost in this work. All our warships are gradually receiving their equipment. Our ocean-going passenger ships are also now so illumined, and perhaps it is here that the comfort, security, and true blessedness of the electric light are experienced.

Railway trains are also being rapidly fitted up. The express trains to Brighton have for a long time been so lighted, and now several northern railways, notably the Midland, are following suit. Our rocky coasts and prominent landfalls are also having their lighthouses fitted with brilliant arc lamps, the last being St. Catherine's Point, on the Isle of Wight, where 60,000 candles throw their bright beams over the English Channel, causing many an anxious mariner to proceed on his way rejoicing.

Fontaine showed in Vienna, in 1873, that a dynamo was reversible—that is, if rotated by the energy of a moving machine, it would produce electric currents; or, if rotated by electric currents, it would move machinery. An electric current is one form of energy. If we have at one place the energy of falling water, we can, by means of a turbine and a dynamo, convert a certain portion of the energy of this falling water into an electric current. We transmit this current through proper conductors to any other place we like, and we can again, by means of a motor, convert the energy of the current into mechanical energy to do work by moving machinery, drawing tram-cars, or in any other way. We can in this way transmit and utilize 50 per cent. of the energy of the falling water wherever we like. The waste forces of Nature are thus within our reach. The waterfalls of Wales may be utilized in London; the torrents of the Highlands may work the tramways of Edinburgh; the wasted horse-power of Niagara may light up New York. The falls of Bushmills actually do work the tramway from Portrush to the Giant's Causeway, and those of Bessbrook the line from Newry to Bessbrook.

The practicability of the transmission of energy by currents is assured, and the economy of doing this is a mere matter of calculation. It is a question of the relative cost of the transmission of fuel in bulk, or of the transmission of energy by wire. Coal can be delivered in London for 12*s.* per ton. The mere cost of the up-keep of a wire between Wales and London to deliver the same amount of energy would exceed this sum tenfold. For long distances the transmission of energy is at present out of the question. There can be no doubt, however, that for many purposes within limited areas the transmission of energy by electricity would be very economical and effective. Pumps are worked in the mines of the Forest of Dean, cranes are moved in the works of Easton and Anderson at Erith, lifts are raised in banks in London; water is pumped up from wells to cisterns in the house of Sir Francis Truscott, near East Grinstead; ventilation is effected and temperature lowered in collieries; goods, minerals, and fuel can be transmitted by telpherage.

The transmission of power by electricity is thus within the range of practice. It can be distributed during the day by the same mains which supply currents for light by night. Small industries, such as printing, watch-making, tailoring, boot-making, can be cheaply supplied with power. It is thus brought into direct competition with the distribution of power by steam as in America, or by air-pressure as in Paris, or by high-pressure water as in London; and the relative advantages and economies of each system are simple questions of calculation. When that evil day arrives that our supply of natural fuel ceases, then we may look to electricity to bring to our aid the waste energies of Nature—the heat of the sun, the tidal wave of the ocean, the flowing river, the roaring falls, and the raging storm.

There is a mode of transport which is likely to create a revolution in the method of working tramways. A tramcar carries a set of accumulators which supplies a current to work a motor geared to a pair of wheels of the car. The weight, price, day's work, and life of the accumulator is curiously the same as the weight, price, day's work, and life of horseflesh; but the cost of maintenance, the liability to accident, and the chances of failure are much less. Although very great improvements in batteries have been made, and they are now really practical things, sufficient experience in tramcar working has not yet been obtained to say that we have reached the proper accumulator. Nor have we yet acquired the best motor and mode of gearing; but very active experiments are being carried out in various countries, and nothing can prevent their ultimate success.

The property which the electric current possesses, of doing work upon the chemical constitution of bodies so as to break up certain liquid compounds into their constituent parts, and marshal these disunited molecules in regular order, according to a definite law, upon the surfaces of metals in contact with the liquid where the current enters and exists, has led to immense industries in electro-metallurgy and electro-plating. The extent of this industry may be gathered from the fact that there are 172 electro-platers in Sheffield and 99 in Birmingham. The term electro-metallurgy was originally applied to the electro-deposition of a thin layer of one metal on another; but this is now known as electro-plating.

In 1839, Jacobi in St. Petersburg and Spencer in Liverpool laid the foundations of all we know of these interesting arts. Copper was deposited by them so as to obtain exact reproductions of coins, medals, and engraved plates. The first patents in this country and in France were taken out by Messrs. Elkington, of Birmingham, who still occupy the foremost position in the country.

The fine metals, gold and silver, are deposited in thin layers on coarser metals, such as German silver, in immense quantities. Christofle, of Paris, deposits annually six tons of silver upon articles of use and of art, and if the surfaces so electro-plated were spread out continuously they would cover 140 acres.

The whole of the copper plates used in Southampton for the production of our splendid Ordnance Survey maps are deposited by copper on matrices taken from the original engraved plates, which are thus never injured or worn, are always ready for addition or correction, while the copies may be multiplied at pleasure and renewed at will.

Nickel-plating, by which the readily oxidizable metals like iron are coated with a thin layer of the more durable material nickel, is becoming a great industry; the trappings of harness, the exposed parts of machinery, the fittings of cycles and carriages, and innumerable articles of daily use, are being rendered not only more durable but more beautiful.

The electro-deposition of iron, as devised by Jacobi and Klein, in the hands of Prof. Roberts-Austen, F.R.S., is giving very interesting results. The dies for the coins which were struck at our Mint on the occasion of the Jubilee of the Queen were modelled in plaster, reproduced in intaglio by the electro-deposition of copper, and on these copper moulds hard excellent iron in layers of nearly $\frac{1}{10}$ of an inch was deposited.

The exact processes of measurement, which have led to such vast improvement in our telegraphic systems, have scarcely yet penetrated into this field of electrical industry, and little is known at present of the exact relations of current and electromotive force with respect to surfaces of contact, rate of deposit, and resistance of liquids. Captain Sankey, R.E., of the Ordnance Survey Department, has done some useful work in this direction.

The extraction of metals from their ores by deposition has received wide application in the case of copper. In 1871, Elkington proposed to precipitate copper electrolytically from the fused sulphide of copper and iron known to the copper smelter as "regulus." Thin copper plates were arranged to receive the deposited copper, while the foreign metals, including gold and silver, fell to the bottom of the solution, the process being specially applicable, it was supposed, to regulus containing small quantities of the precious metals.

The electrical purification of copper from impure "blister copper" or "blade copper" has also made great progress, and special dynamos are now made which will, with an expenditure of 100 horse-power, precipitate 18 tons of copper per week. The impure metal is made to form the anode in a bath of sulphate of copper, the metal being deposited in the pure form on a thin copper cathode.

It was not very long ago considered very economical to absorb 0·85 horse-power in depositing 1 pound of copper per hour, but now the same work can be done with 0·3 horse-power. Mr. Parker, of Wolverhampton, has done good work in this direction, and his dynamos in Messrs. Bolton's works have revolutionized this process of purification.

Both at Swansea and Widnes, immense quantities of copper, in spite of the restrictive operations of the Copper Syndicate, are being produced by electro-deposition. Copper steam-pipes for boilers are now being built up of great firmness, fine texture, and considerable strength, by Mr. Elmore, at Cockermouth, by electro-deposition on a rotating mandril in a tank of sulphate of copper. By this process one ton of copper requires only a little more than one ton of coal to raise the requisite steam to complete the operation.

It has been shown that the electrolytic separation of silver from gold by similar methods is perfectly practicable. The value of the material to be dealt with may be gathered from the fact, communicated to the Gold and Silver Commission now sitting, that nearly 90,000,000 ounces of silver are annually produced, and the greater portion of this amount contains sufficient gold to render refining remunerative. Although the old acid process of "parting" gold and silver remains practically undisturbed, there seems no reason to doubt that in the future electricity will render us good service in this direction, as it has already in the purification of copper.

There is not much actual progress to report in the extraction of gold from its ores by electrical agency. The conversion of gold into chloride of gold by the direct, or indirect, action of chlorine is employed on a very large scale in [Grass Valley] California and elsewhere. This fact has led to well-directed efforts to obtain, by electrolytic action, chlorine which should attack finely-divided gold suspended (with the crushed ore) in the solution from which the chlorine was generated, the gold, so converted into soluble chloride, then being deposited on a cathode. The process would seem to be hopeful, but is not as yet a serious rival to the ordinary chlorination method.

In the amalgamation of gold ores much is expected from the possibility of keeping clean, by the aid of hydrogen set free by the electric current, the surfaces of amalgamated plates.

It is well known that the late Sir W. Siemens considered that the electric arc might render good service in the fusion of metals with high melting-points, and he actually succeeded in melting 96 ounces of platinum in ten minutes with his electrical furnace. The experiments were interrupted by his untimely death; but in the hands of Messrs. Cowles the electric arc produced by 5000 amperes and 500 horse-power is being employed on a very large scale for the isolation of aluminium (from corundum), which is immediately alloyed (*in situ*) with copper or iron, in the presence of which it is separated.

The heating power of large currents has been used by Elihu Thomson in the United States, and by Bernardos in Russia, to weld metals, and it is said to weld steel without affecting its hardness. It has even been proposed to weld together in one continuous metallic mass the rails of our railways, so as to dispense entirely with joints.

The production of chlorine for bleaching and of iodine for pharmaceutical purposes, the economical production of oxygen, are also processes now dependent on the electrolytic effect of the electric current.

It is almost impossible to enumerate the various general purposes to which electricity is applied to minister to our wants and to add to our comforts. Everyone appreciates the silent efficiency of the trembling electric bell, while all will sooner or later derive comfort from the perennially self-winding electric clock. Correct mean time is distributed throughout the length and breadth of the land by currents derived from Greenwich Observatory. Warehouses and shops are fitted with automatic contact pieces, which, on any undue increase of temperature due to fire, create an alarm in the nearest fire-station; and at the corner of most streets a post is found with a face of glass, which on being broken enables the passer-by or the watchful and active policeman to call a fire-engine to the exact spot of danger. Our sewers are likely to find in its active chemical agency a power to neutralize offensive gases, and to purify poisonous and dangerous fluids. The germs of diseases are attacked and destroyed in their very lairs. The physician and the surgeon trust to it to alleviate pain, to cure disease, to effect organic changes beyond the reach of drugs. The photographer finds in the brilliant rays of the arc lamp a miniature sun which enables him to pursue his lucrative business at night, or during the dark and dismal hours of a black November fog in London.

We learn from the instructive and interesting advertising columns of our newspaper that "electricity is life," and we may perhaps read in the more historical portion of the same paper that by a recent decision of the New York Parliament, "electricity is death." It is proposed to replace hanging by the more painless and sudden application of a powerful electrical charge; but those who have assisted at this hasty legislation would have done well to have assured themselves of the practical efficacy of the proposed process. I have seen the difficulty of killing even a rabbit with the most powerful induction coil ever made, and I know those who escaped and recovered from the stroke of a lightning discharge.

The fact that the energy of a current of electricity, either when it flashes across an air space, or when it is forced through high resistance, assumes the form of heat of very high temperature led early to its employment for firing charges of gunpowder; and for many civil, military, and naval purposes it has become an invaluable and essential agent. Wrecks like that of the *Royal George* at Spithead were blown up and destroyed; the faces of cliffs and quarries are thrown down; the galleries of mines and tunnels are excavated; obstructions to navigation like the famous Hell Gate, near New York, have been removed; time-guns to distribute correct time are fired by currents from Greenwich at 1 p.m. In the operations of war, both for attack and defence, submarine mining has become the most important branch of the profession of a soldier and a sailor. Big guns, whether singly or in broadside, are fired; and torpedoes, when an enemy's ship unwittingly is placed over them, are exploded by currents of electricity.

An immense amount of research has been devoted to design the best form of fuse, and the best form of generator of electricity to use to explode them. Gun tubes for firing consist of a short piece of very fine wire embedded in some easily fusible compound, while the best form of fuse is that known as the Abel fuse, which is composed of a small, compact mass of copper phosphide, copper sulphide, and potassium chlorate. The practice in the use of generators is very various. Some, like the Austrians, lean to the high-tension effects of static electricity; others prefer magneto-machines; others use the dynamo; while we in England cling with much fondness to the trustworthy battery. Since the electric light has also become such a valuable adjunct to war purposes, it is probable that secondary batteries will become of immense service. The strong inductive effects of atmospheric electricity are a source of great danger. Many accidental explosions of fuses have occurred. An experimental cable with a fuse at one end was laid below low water mark along the bank of the Thames at Woolwich. The fuse was exploded during a heavy thunderstorm. The knowledge of the causes of a danger is a sure means for the production of its removal, or of its reduction to a minimum. Low-tension fuses and metallic circuits reduce the evils of lightning, but have not removed them. Should war unhappily break out again in Europe, submarine mining will play a very serious part; and, paradoxical as it may appear—as has been suggested by the French Ambassador, M. Waddington—its very destructiveness may ultimately prove it to be a powerful element of peace.

It seems incredible that, having utilized this great power of Nature to such a wide and general extent, we should be still in a state of mental fog as to the answer to be given to the simple question—What is Electricity? The engineer and the physicist are completely at variance on this point. The engineer regards electricity, like heat, light, and sound, as a definite form of energy, something that he can generate and destroy, something that he can play with and utilize, something that he can measure and apply. The physicist—at least some physicists, for it is difficult to find any two physicists that completely agree with each other—regard electricity as a peculiar form of matter permeating all space as well as all substances, together with the luminiferous ether, which it permeates like a jelly or a sponge. Conductors, according to this theory, are holes or pipes in this jelly, and electrical generators are pumps that transfer this hypothetical matter from one place to another. Other physicists, following Edlund, regard the ether and electricity as identical; and some, the disciples of Helmholtz, consider it as an integral constituent of Nature, each molecule of matter having its own definite charge, which determines its attraction and its repulsion. All attempts to revive the Franklinian, or material, theory of electricity, have, however, to be so loaded with assumptions, and so weighted with contradictions, that they completely fail to remove electricity from the region of the mysterious. It is already extremely difficult to conceive the existence of the ether itself as an infinitely thin, highly elastic medium, filling all space, employed only as the vehicle of those undulatory motions that give us light and radiant heat. The material theory of electricity requires us to add to this another incomprehensible medium embedded or entangled in this ether, which is not only a medium for motion, but which is itself moved. The practical man, with his eye and his mind trained by the stern realities of daily experience, on a scale vast compared with that of the little world of the laboratory, revolts from such wild hypotheses, such unnecessary and inconceivable conceptions, such a travesty of the beautiful simplicity of Nature.

He has a clear conception of electricity as something which has a distinct objective existence, which he can manufacture and sell, and something which the unphilosophic and ordinary member of society can buy and use. The physicist asserts dogmatically: "Electricity may possibly be a form of matter—it is not a form of energy." The engineer says distinctly: "Electricity is a form of energy—it is not a form of matter; it obeys the two great developments of the present generation—the mechanical theory of heat and the doctrine of the conservation of energy." There must be some cause for this strange difference of views. It is clear that the physicist and the engineer do not apply the term electricity to the same thing. The engineer's electricity is a real form of energy; the speculative philosopher's electricity is a vague subjective unreality which is only a mere factor of energy and is not energy itself. This factor, like force, gravity, life, must, at any rate for the present, remain unknowable. It is not known what force is; neither do we know what is matter or gravity. The metaphysician is even doubtful as regards time and space. Our knowledge of these things commences with a definition. The human mind is so unimpressionable, or language is so poor, that writers often cannot agree even on a definition. The definition of energy is capacity for doing work. We practical men are quite content to start from this fiducial line, and to affirm that our electricity is a something which has a capacity for doing work; it is a peculiar form of energy. The physicist may speculate as much as he pleases on the other side of this line. He may take the factors of energy, and mentally play with them to his heart's content; but he must not rob the engineer of his term *electricity*. It is a pity that we cannot settle our difference by changing the term. Physicists might leave the term *electricity* to the form of energy, which is an objective reality, and which the ordinary mortal understands; while engineers would be quite content if speculative physicists and enthusiastic mathematicians would call their subjective unreality, their imaginary electrical matter, by some other term. If it be necessary to mentally create some imaginary matter to fulfil the assumptions and abstractions of their mathematical realizations, let them call it *coulombism* or *electron*, and not appropriate the engineer's generic and comprehensive term electricity. The engineer finds the motions of existing matter and of the ether quite sufficient to meet all his requirements, and to account for all those phenomena which are called electrical.

It seems paradoxical to assert that two unrealities can form a reality, or that two subjective ideas can become an objective one; but it must be remembered that in all electrical phenomena that which makes them real and objective is derived from without. The motion that renders an electrical phenomenon evident is imparted to it from some other form of energy. The doctrine of the conservation of energy asserts that energy is never destroyed, it is only transformed—work must be done to render it evident. No single electrical effect can be adduced which is not the result of work done, and is not the equivalent of energy absorbed. The engineer's notion of work—something done against resistance; and of power—the rate at which this change of condition is effected—are the key-stones to the conception of the character of those great sources of power in Nature whose direction to the uses and convenience of man is the immediate profession of those who generally assemble together in Section G of the British Association to discuss the "practical application of the most important principles of natural philosophy, which has, in a considerable degree, realized the anticipations of Bacon and changed the aspect and state of affairs in the whole world."

I cannot pretend to have given a survey of all the practical applications of electricity. I have but briefly indicated the present area covered by the new and rapidly-growing industry. Five million people upon the globe are now dependent on the electric current for their daily bread. Scarcely a week passes without some fresh practical application of its principles, and we seem to be only on the shore of that sea of economy and beneficence which expands with every new discovery of the properties of electricity, and spreads already beyond the mental grasp of any one single worker.

NOTES.

THE Geological Congress held its first meeting on Tuesday. This week we print the President's address and one of the papers referring to one of the most important points to be considered by the Congress—that of the Crystalline Schists.

INTELLIGENCE has been received of the murder of Major Barttelot, Mr. Stanley's principal lieutenant, by some of his followers when on the way from Stanley Falls with reinforcements for his chief.

THE sudden death of Mr. R. A. Proctor was announced from New York about a week ago. In addition to his writings on various subjects for which his name is so widely known, he made some contributions to the science of astronomy. Some of his books, such as "Saturn and its System," his various star atlases, and others we might name, have a permanent value. Elected a Fellow of the Royal Astronomical Society in 1866, he was for a considerable number of years the most prolific contributor to the *Monthly Notices*. In 1871 he was elected to the Council, and in the following year was appointed the Secretary. The determination of the rotation-period of Mars, a chart of Mars from the collation of a large number of drawings, a long series of papers on transits of Venus, especially the transits of 1874 and 1882, and a yet more important series on the distribution of stars and nebulæ, were communicated to the Astronomical Society during these years. It was in connection with this last series that his greatest single work for science was carried out, viz. the copying of the 324,198 stars of Argelander's "Survey of the Northern Heavens," on an "equal surface" projection chart, a work that involved 400 hours of the most unremitting labour. Mr. Proctor was born at Chelsea, in March 1834, and was educated at King's College, London, of which he was Honorary Fellow, and at St. John's College, Cambridge, where he won a Scholarship. He obtained his degree of B.A. in 1860, and his name appears as twenty-third in the Wrangler's List.

AN evening class in organic chemistry, adapted to the requirements of candidates for the second B.Sc. examination of London University, will be held at the Birkbeck Institution in Chancery Lane during the ensuing session, under the direction of Mr. Frank Gossling, B.Sc. This is said to be the first session in which an evening class of this character has been attempted.

THE *Times* publishes the following interesting letter from Sir William Thomson :—"In the *Times* of to-day (Sept. 14) I see a slight mistake regarding myself. A British Association correspondent says :—'Sir William Thomson in one paper cautiously made what must be regarded as a somewhat noteworthy admission with reference to Clerk-Maxwell's fundamental theory of electromagnetic induction for incomplete circuits. He considered Maxwell's fundamental assumption "not wholly tenable." In all his previous utterances on the subject Sir William has described Maxwell's views on this point as completely untenable.' The paper referred to by your correspondent is my very first public utterance on the subject. An uncorrected proof of it in print contained the words 'wholly untenable,' which I altered to 'not wholly tenable' in reading it to the Section. The fact is, I had always believed in the possibility and probability of Maxwell's assumption (he only gave it himself as probable or possible) until a few months ago, when I saw what seemed to me reasons for wholly discarding it ; but two days of the British Association before my paper was read gave me the inestimable benefit of conversation with others occupied with the same subject, and of hearing Prof. Fitzgerald's presidential address in Section A, by which I was helped to happily modify my opinion. In your leading article of to-day I do not think you quite do justice to the British Association and its objects. Your remarks would be wholly just, and, if I may be allowed to say so, very useful criticism, if the British Association were an institution for teaching ascertained scientific results to its members, or 'an annual setting forth of scientific wares.' Its object is the advancement of science. It contributes to this object in a manner altogether peculiar to itself, by bringing together from all parts of the world persons engaged in scientific investigation, and giving them facilities for helping one another in their work, and being helped in it by what they see and hear. No one not following the course of scientific progress, generally or in some particular department, can fully understand how much of practical impulse is owing to the British Association for the contributions made in the course of the year to the scientific societies and magazines, in which achieved results of scientific investigation are recorded and published."

IN the last issue of the Transactions of the Seismological Society of Japan, Prof. Milne discusses the effects of earthquakes on animals. The records of most great earthquakes refer to the consternation of dogs, horses, cattle, and other domestic animals. Fish also are frequently affected. In the London earthquake of 1749, roach and other fish in a canal showed evident signs of confusion and fright ; and sometimes after an earthquake fish rise to the surface dead and dying. During the Tokio earthquake of 1880, cats inside a house ran about trying to escape, foxes barked, and horses tried to kick down the boards confining them to their stables. There can, therefore, be no doubt that animals know something unusual and terrifying is taking place. More interesting than these are the observations showing that animals are agitated just before an earthquake. Ponies have been known to prance about their stalls, pheasants to scream, and frogs to cease croaking suddenly a little time before a shock, as if aware of its coming. The Japanese say that moles show their agitation by burrowing. Geese, pigs, and dogs appear more sensitive in this respect than other animals. After the great Calabrian earthquake it is said that the neighing of a horse, the braying of an ass, or the cackle of a goose was sufficient to cause the inhabitants to fly from their houses in expectation of a shock. Many birds are said to show their uneasiness before an earthquake by hiding their heads under their wings and behaving in an unusual manner. At the time of the Calabrian shock little fish like sand-eels (*Cirricelli*), which are usually buried in the sand, came to the top and were caught in multitudes. In South America certain quadrupeds, such as dogs, cats, and jerboas, are believed by the people to give warning of coming danger by their restlessness ; sometimes immense flocks of sea-birds fly inland before an earthquake, as if alarmed by the commencement of some sub-oceanic disturbance. Before the shock of 1835 in Chili all the dogs are said to have escaped from the city of Talcahuano. The explanation offered by Prof. Milne of this apparent prescience is that some animals are sensitive to the small tremors which precede nearly all earthquakes. He has himself felt them some seconds before the actual earthquake came. The alarm of intelligent animals would then be the result of their own experience, which has taught them that small tremors are premonitory of movements more alarming. Signs of alarm days before an earthquake are probably accidental ; but sometimes in volcanic districts gases have emanated from the ground prior to earthquakes, and have poisoned animals. In one case large numbers of fish were killed in this way in the Tiber, and at Follonica, on the morning of April 6, 1874, "the streets and roads were covered with dead rats and mice. In fact, it seemed as if it had rained rats. The only explanation of the phenomenon was that these animals had been destroyed by emanations of carbon dioxide."

THE Animals' Institute, which was opened this season for the reception of patients, has already more than verified its founders' fears that much suffering amongst the animals belonging to the poorer classes existed without proper surgical treatment. The gratuitous advice daily given is taken full advantage of, and the hospital accommodation for the worst cases is now too small to admit the great number of horses, dogs, cats, and other animals requiring treatment. A supplementary institution is wanted—a sanatorium in the suburbs—where cases requiring prolonged treatment can be kept. Such an addition, if the preliminary expenses were forthcoming, can, it is stated, be made quite self-supporting. The scheme is to be placed on a practical basis at a meeting to be held in the Committee-room of the Animals' Institute, 9 Kinnerton Street, Belgrave Square.

A COMMITTEE of the American Association presented a report at the last meeting on the teaching of physics in schools, which was very fully discussed by both the Mathematical and Physical Sections. The following is a summary of the recommendations :—(1) It is the opinion of the Committee that instruction in physics may begin, with profit, in what is generally known as the "grammar school." At the same time it is decidedly opposed to any general recommendation that it must begin there or in the primary school. Here, perhaps more than anywhere else, nearly everything depends upon the teacher. One who has a strong liking for and a good knowledge of physics will be tolerably certain to succeed, while another not thus equipped for the work is equally certain to fail. (2) When taught in the grammar school and by a competent teacher, it should be done mainly by and through illustrative experiments. These may be of the simplest character, involving and exhibiting some of the fundamental principles of science ; and they should generally be made by the teacher, the pupils being encouraged to repeat, to vary, and to extend. (3) In any discussion of the character of instruction in physics in the high school, one fact of the utmost importance must not be lost sight of. It is that a large majority of the young people who are educated in the public schools receive their final scholastic training in the high school. Its course of study must be in harmony with this fact, such provision as may be made for those who continue their studies in college or university being merely incidental. It is important that the student should be made acquainted, if only to a limited extent, with the methods of physical investigation, and that he should be able himself to plan and carry out an attack upon some of the simpler problems of the science. It is believed that these two very desirable ends can be reached without giving an undue share of the time and energy of the pupil to the subject. Assuming the high-school course to consist of four years of three terms each, it is recommended that the study of physics should begin not earlier than the third year; that it should continue through one year, three hours a week being devoted to it, not including the time necessary for the preparation of the lesson; and that during the first two terms the work should be text-book work, accompanied by illustrative experiments performed by the instructor, and made as complete as his facilities will allow, while the last term should be devoted to simple laboratory exercises. (4) As to the requirements in physics for admission to college, it is sufficient to say that the course indicated above should be required for admission to any and all courses in the college. (5) In reference to the minimum course in physics for undergraduate students in the college, it seems important to avoid the mistake of asking too much. In many institutions, and especially where the elective system largely prevails, it is possible at present for students to receive a degree and yet be almost absolutely ignorant of the principles of physics. It is the judgment of the Committee that a knowledge of this subject constitutes one of the necessary and essential elements of a liberal education, and a minimum course of three hours per week for one year is recommended. What is usually known as the junior year is most desirable for this work, as at that time the student is sufficiently mature and has acquired the necessary training in mathematics to enable him to make the best of what he does. It is recommended that this course consist entirely of text-book and recitation work, with lectures fully and completely illustrated on the professor's table. The report is signed by T. C. Mendenhall, William A. Anthony, H. S. Corbait, and F. H. Smith.

A CORRESPONDENT of the *Times* calls attention to the new light now shown from the St. Catherine's Point Lighthouse in the Isle of Wight. Prior to May 1 of this year the light exhibited at this station was described in the Admiralty list of lights as fixed, dioptric, of the first order. That is, it was a steady light produced by means of a six-wick concentric oil-burner and refracting lenses, the intensity of the naked flame being equal to about 730 candles. At the present moment an electric light is being shown at St. Catherine's, the full-power intensity of which was recently stated by Captain Sydney Webb, the Deputy Master of the Trinity House, to be equal in illuminating power to rather more than 7,000,000 candles. Every half-minute, in fact—for the light now revolves—a mighty flash of five seconds' duration sweeps around the sea, and is visible at distances that seem incredible. To effect this improvement a commodious engine-room has been added to the establishment, containing three steam-engines of 12 horse-power each, and two magneto-electric machines of the De Meritens type. Two of the engines are intended to work for lighting purposes, the third being meant to work the fog-signal. As a precaution against break-down, everything is in duplicate at least, with an oil light in reserve at well. The only other lighthouses on the coast of England as which the light is produced by means of electricity are Souter Point, on the coast of Durham, between the mouths of the Tyne and the Wear; the South Foreland, and at the Lizard, on the Cornish coast. But the St. Catherine's light is ten times more powerful than the best of them, the one on Souter Point. It is, in fact, one of if not, as is believed, actually the most intensely brilliant light in existence, and one which the country as a maritime nation may certainly feel proud to see on its shores.

ON the 25th ult. the ascent of Mount Elburz was successfully made from the eastern side by Baron Ungern Sternberg. In notifying the event to the Tiflis Geographical Society, the Baron wrote:—"We set out at 11, and crossed the glaciers Iriktchat, Atrium, and Djelkaoughenkes, hitherto deemed impassable. At an altitude of 15,200 feet, I discovered an enormous crater. We passed three nights on the mountain at the different heights of 9000, 14,760, and 17,840 feet. At the last height we passed through a terrific snowstorm. Breathing was not attended with any great difficulty. The health of my men has been good. I descended by the southern side between Azaou and the Terek.'

THE last number of the *Mittheilungen* of the Vienna Geographical Society has an account by Dr. Svoboda, surgeon of the Austrian man-of-war *Aurora*, of a visit of that ship, in 1886, to the Nicobar Islands. This archipelago is usually divided into three groups:—(1) The northern islands, including Batti Malive and Kar Nicobar, which are thickly populated, some of them being flat and some mountainous and covered with jungle. Kar Nicobar has an extensive trade with Ceylon, Burmah, Singapore, and other places, as many as between forty and fifty vessels touching there annually; in fact, its harbour is never without a number of ships. The sole industry of the inhabitants is the manufacture of a kind of earthenware vessels, which they export to the other islands. Other articles of trade are "birds'-nest soup" and "sea-slug soup." The two other groups of islands are (2) the southern islands, including Great and Little Nicobar, and (3) the central islands, comprising Teressa, Chowra, Katchall, Bompoka, and many others. The inhabitants of these groups of islands are divided into classes by Dr. Svoboda—namely, the Shab-Dwa, the inhabitants of the coast, and the Shom-Pen, the inhabitants of the interior. The first class resemble the inhabitants of Siam and Burmah, but are, in general, lighter in colour than these latter. Both men and women are repulsive in appearance, though they are generally well formed. The men wear very long hair, and are, as a rule, weak and inactive. Visitors to the islands find it almost impossible to see the young unmarried women, so closely are they kept from the eyes of strangers. Prior to the visit of Dr. Svoboda, nothing appears to have been known of the Shom-Pen, or inhabitants of the interior. They are completely isolated from the outer world, and are very simple in their habits. The men wear the ordinary loin-cloth, and the women a short skirt, usually their own manufacture, and the only personal ornaments they have are small pieces of bamboo in their ears, and necklaces of variously coloured glass beads or ribbons many feet in length. Malaria is very prevalent in all the islands, especially in October and November, when the weather is hot and dry. Dr. Svoboda gives a short historical and geographical sketch of the islands, which now have a population of about 6000 souls. The Arabs appear to have been the earliest visitors, and Portuguese vessels used to call there frequently; indeed, many Portuguese words are in common use amongst the natives.

A PAPER was recently read before the French Academy of Sciences by M. Emile Lavasseur on the "Centenarians now living in France." The first reports collected gave the number of persons who had attained 100 years and upwards as 184, but on these being thoroughly sifted no less than 101 were struck out, leaving 83, but even of these there were no fewer than 67 who could not furnish adequate proof of their reputed age. In 16 cases, however, authentic records of birth or baptism were found, including that of a man born in Spain, and baptized August 20, 1770. His life was spent almost wholly in France. All the other centenarians were reputed to be between 100 and 105 years of age, with the exception of a widow claiming to be 112 years old. Of the 83 persons said to be centenarians women formed a large majority, the proportion being 52 women to 31 men. There were but few married couples, 6 male and 16 female celibates, 23 widowers, and 41 widows. One of the latter was Madame Rostkowski, 103 years of age. She enjoys a pension of 60 francs a month, allowed her by the French Government in consideration of her late husband's military services. More centenarians exist in the south-western departments, than in the rest of the Republic, while the basin of the Garonne—from the Pyrenees to the Puy de Dôme—contains as many as all the rest of France put together. M. Lavasseur finds that the chances of a person in the nineteenth century reaching 100 years of age are one in 18,800.

In a recent number of *La Nature* Colonel Hennebert, of the Belgian army, describes underground forts which have come into use in Belgium, as one of the principal methods of national defence. One of these underground forts is like an enlarged mole-hill, and is built of concrete. Measuring 50 metres in length by from 30 to 40 in width, it is about 12 metres below the surface of the ground, and its greatest height above the earth is no more than 3 or 4 metres. It presents the appearance of an elliptical cap placed on the ground, and is scarcely visible to the eye of an observer. At the centre of this artificial rock are three armoured towers, each with two heavy guns. There are also four small forts, which are pulled in and run out at pleasure, each armed with two rapid-firing guns. At three suitable places there are armoured points of observation, from two of which at night the electric light can be flashed to watch the operations of the enemy. Below this surface the earth is hollowed out in the form of a huge well with armoured sides, which is divided up into sections, each part protected with heavy armour, one part for provisions and ammunition, another for machinery, which includes the dynamos and accumulators for the lighting of the whole fort, hydraulic machines for working the movable turrets and sending them ammunition, pumps for supplying these machines with water, and a series of ventilators to keep the air pure. Communication with the outer world is made by a subterranean gallery, the length of which varies according to surrounding circumstances. The ceiling of this gallery is from 8 to 10 metres below the surface. To gain access to the fort an hydraulic piston is worked, and this raises a ladder which runs along the whole length of the fort, and lowers the door of the outlet, which is protected by armour 20 centimetres in thickness, and is under the fire of two of the movable forts. All movements, such as changes of guard, arrivals of supplies, &c., are reported by telephone or telegraph. The guard does not work the hydraulic piston, except at command, and when the sentries in one of the movable forts have reconnoitred the visitors. Finally, the gallery communicating with the outer world is strongly fortified by an armoured door defended by two mitrailleuses. One of the greatest objections by generals to forts, that they absorb numbers of men who are wanted in the field, cannot be urged against these subterranean forts, for the garrison consists of thirty or forty mechanics and specialists only, whose absence would not appreciably weaken the regiment from which they are drawn. The cost of one of these forts is only about £100,000.

A correspondent of the *Times* gives an interesting description of the Brünig Railway, which has recently been opened between Lucerne and Bernese Oberland. The gradient is in places very steep, being as much as 1 in 8; and on this account special precautions had to be taken both in the up and the down journeys. Generally speaking, the Rigi system has been adopted. The locomotive turns a cog-wheel which runs on a toothed rack placed between the rails, and so the train slowly travels, or rather is dragged, up hill. The cog-wheel is stopped and the engine works in the ordinary way when a moderate gradient or a level piece is met with. To check the too rapid descent of the train, the engine is fitted with a pneumatic counter-pressure action brake, which of itself is sufficient to stop the train. Besides this, each vehicle in the train is fitted up with a cog-wheel and rack similar to those used in the ascent, with drums on the axle to which clip-brakes are applied. By these appliances the speed can be regulated and the train stopped at any moment. There was another danger, however, incident to all steep railways, to be encountered—namely, the risk to the couplings during an ascent. Though the brakes on each vehicle would probably be sufficient in such a case, yet it was thought fit to take further precautions. When the train is at rest, the brake is kept fully applied by heavy weights. These weights are lifted by steam-power, which is conveyed from the engine in flexible tubes. If a coupling breaks, the flexible tube conveying the steam also breaks, and the weights fall down automatically and check the motion of the carriages. It only remains to say that the gauge is a very narrow one, being only 1 metre.

The *American Meteorological Journal* for August contains:—(1) An article by C. C. McCaul, on the climatic effects of the Chinook wind in South Alberta, the country of the great cattle ranges in Canada, extending from lat. 49° to the Red Deer River to the northwards, and from the Rocky Mountains, on the west, to about 140 miles east. The Chinook wind blows from west to south-west, in varying degrees of strength; and the thermometer often rises in a few hours from 20° below to 40° above zero, while the snow, which may have been a foot deep in the morning, disappears before night. (2) A sketch of Prof. Abbe's work, with a portrait. He was appointed to the Weather Bureau at Washington in January 1871, and at once urged the desirability of establishing the State weather services which now form so important a part of the policy of the Signal Service. Among the many recommendations of Prof. Abbe we may mention the establishment of a "Scientific and Study Division," which was formed early in 1881, and the compilation of a Meteorological Bibliography which, although still unpublished, has grown to considerable dimensions. (3) Mr. A. L. Rotch continues his description of the meteorological service in Switzerland.

We have received from Dr. G. Hellmann, of the German Meteorological Office, an account of the torrential rainfall of August 2 to 3 last, which caused disastrous inundations of some of the Silesian tributaries of the Oder. The storm lasted from 15 to 18 hours, during which time nearly 8 inches of rain fell over a large district, and more or less affected Galicia, Bohemia, and Poland. These heavy rains do not seem to have been caused by the same storm which gave us 1¼ inch of rain in London, on August 1 to 2, but by a distinct subsidiary depression which gradually formed over Germany on the 2nd, and moved away towards the Baltic.

Messrs. Swan Sonnenschein and Co. have the following works on natural history and science in the press:—"The Nature of Harmony and Metre," by Moritz Hauptmann, translated and edited by W. E. Heathcote, M.A.; "Atlas of Fossil Conchology," being the original steel plates in Brown's "Fossil Conchology," with descriptive letterpress; "The Naturalist in Siluria," by Captain Mayne Reid, illustrated; "Land and Fresh-Water Shells," by Dr. J. W. Williams; "An Introduction to Zoology," by B. Lindsay; "The Wanderings of Plants and Animals," by Prof. Victor Hehn, edited by J. S. Stallybrass.

The additions to the Zoological Society's Gardens during the past week include two Central American Agoutis (*Dasyprocta isthmica*), obtained by purchase; a Large Hill Mynah (*Gracula intermedia*) from India, presented by Lieut.-Col. R. Thompson; a White-backed Piping Crow (*Gymnorhina leuconota*) from

Australia, presented by Mr. R. Hall; two White-fronted Amazons (*Chrysotis leucocephala*) from Cuba, a Prince Albert's Curassow (*Crax alberti*) from Columbia, a Mexican Guan (*Penelope purpurascens*), obtained by purchase; a Herring Gull (*Larus argentatus*), British, presented by the Marchioness of Cholmondeley; a Tuberculated Iguana (*Iguana tuberculata*) from Brazil, presented by Mr. H. E. Blandford; a Chameleon (*Chamæleon vulgaris*), three Lacertine Snakes (*Cœlopeltis lacertina*), and two Horseshoe Snakes (*Zamenis hippocrepis*) from Morocco, presented by Mr. Herbert E. White.

OUR ASTRONOMICAL COLUMN.

Comet 1888 *c* (Brooks).—Dr. H. Kreutz has more recently computed for this comet more exact elements than those which he had obtained from the observations of August 9, 10, and 11. These later elements are based on observations made at Vienna on August 9, at Hamburg, August 14 and 24, and at Strassburg, August 19; aberration and parallax being corrected for.

T = 1888 July 31·25115, Berlin M.T.

$$\left.\begin{aligned}\omega &= 59^\circ\ 19'\ 2''5\\ \Omega &= 101\ 32\ 50\cdot1\\ \iota &= 74\ 12\ 13\cdot7\end{aligned}\right\}\ \text{Mean Eq. 1888·0.}$$

$$\log q = 9\cdot955456$$

Error of middle places (O – C),

August 14	...	$\Delta\lambda \cos\beta = -3''\cdot5$;	$\Delta\beta = -3''\cdot2$
19		$+3\cdot3$;	$-3\cdot4$

Prof. A. Krueger (*Astr. Nach.*, No. 2855) has computed the following ephemeris for Berlin midnight from the foregoing:—

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Sept. 21	14 57 40	21 13·7 N.	0·1084	0·2310	0·45
23	15 5 34	19 44·1			
25	15 13 7	18 16·4	0·1242	0·2456	0·39
27	15 20 22	16 51·0			
29	15 27 19	15 27·9 N.	0·1395	0·2611	0·34

The brightness on August 9 is taken as unity.

On August 11 the comet was observed at the Observatory of Algiers, and the nucleus was estimated as being about equal in brightness to a star of the tenth magnitude; the nebulosity was about 1′ in diameter, and there was a faint tail in the direction of the diurnal movement. Prof. L. Boss, observing the comet at Albany, N.Y., estimated it on August 10 as of mag. 9, and on August 19, in bright moonlight, as mag. 11. The tail on August 10 was estimated as 10′ in length, and was of the same breadth as the head.

Discovery of a New Comet, 1888 *e*.—Mr. E. E. Barnard, formerly of Nashville, Tennessee, now at the Lick Observatory, discovered a new comet on September 3 at 0h. 33m. G.M.T., R.A. 6h. 52m. 16s., Decl. 10° 59′ N. The comet is described as circular, 1′ in diameter, of the eleventh magnitude, with tolerably well-defined nucleus, but with no tail. Dr. Kobold observed it at Strassburg on September 5 at 13h. 44·1m. G.M.T., R.A. 6h. 52m. 1·5s., Decl. 10° 49′ 33″ N.

Comet 1888 *d* (Faye).—Placing the perihelion passage of this comet as 2·60d. later than given in Dr. Möller's elements, an alteration according well with the observations at Nice, August 9-17, Dr. H. Kreutz has computed (*Astr. Nach.*, No. 2856) the following ephemeris for it for Berlin midnight:—

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Sept. 21	6 47 41	15 36 N.	0·2472	0·2244	1·33
23	6 51 59	15 16			
25	6 56 12	14 56	0·2489	0·2177	1·36
27	7 0 21	14 35			
29	7 4 24	14 14 N.	0·2509	0·2110	1·39

The brightness on August 9 is taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 SEPTEMBER 23-29.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on September 23

Sun rises, 5h. 50m.; souths, 11h. 52m. 6·7s.; sets, 17h. 54m.: right asc. on meridian, 12h. 3·1m.; decl. 0° 20′ S. Sidereal Time at Sunset, 18h. 6m.

Moon (at Last Quarter September 28, 9h.) rises, 19h. 18m.*; souths, 2h. 0m.; sets, 8h. 54m.: right asc. on meridian, 2h. 9·5m.; decl. 7° 44′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	7 56	13 10	18 24	13 21·0	9 53 S.
Venus ...	7 42	13 7	18 32	13 18·2	7 30 S.
Mars ...	12 21	16 15	20 9	16 26·9	23 20 S.
Jupiter ...	11 33	15 48	20 3	15 59·3	19 59 S.
Saturn ...	1 34	9 6	16 38	9 16·4	16 40 N.
Uranus...	7 18	12 51	18 24	13 2·0	5 57 S.
Neptune..	20 5*	3 52	11 39	4 2·0	18 57 N.

* Indicates that the rising is that of the preceding evening.

Occultation of Star by the Moon (visible at Greenwich).

Sept.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
28	ζ^2 Geminorum	4	22 20	23 11	55 245

Sept.	h.	
23	22	Mercury at greatest distance from the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei ...	0 52·4	81 16 N.	Sept. 26,	4 33	*m*
ζ Geminorum ...	6 57·5	20 44 N.	,, 24,	0 0	*M*
			,, 29,	4 0	*m*
T Ursæ Majoris ...	12 31·3	60 6 N.	,, 28,		*m*
R Boötis ...	14 32·3	27 13 N.	,, 27,		*m*
δ Libræ ...	14 55·0	8 4 S.	,, 27,	20 24	*m*
U Coronæ ...	15 13·6	32 3 N.	,, 29,	20 31	*m*
U Ophiuchi ..	17 10·9	1 20 N.	,, 25,	20 34	*m*
Z Sagittarii ...	18 14·8	18 55 S.	,, 24,	19 0	*M*
β Lyræ...	18 46·0	33 14 N.	,, 24,	2 0	*M*
S Sagittæ ...	19 50·9	16 20 N.	,, 27,	21 0	*m*
X Cygni ...	20 39·0	35 11 N.	,, 29,	5 0	*m*
T Vulpeculæ ...	20 46·7	27 50 N.	,, 28,	19 0	*M*
			,, 29,	20 0	*m*
Y Cygni ...	20 47·6	34 14 N.	,, 23,	3 18	*m*
			,, 26,	3 12	*m*
δ Cephei ...	22 25·0	57 51 N.	,, 27,	3 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl.	
Near α Arietis ...	30	18 N.	
	105	50 N.	Very swift.
,, δ Draconis ...	290	70 N.	Swift.

THE INTERNATIONAL GEOLOGICAL CONGRESS.[1]

I DEEPLY regret that, in consequence of his state of health, Prof. Huxley is unable to be present to-day to bid you welcome to England. But if one voice is here wanting, let me assure you that the unanimous voice of English geologists unites in the same sentiment, and also thanks you, gentlemen, our foreign colleagues, for having responded in a manner so flattering to the invitation of English geologists to meet this year in London. For in this assembly there are representative geologists from Germany, Austria, Belgium, Denmark, Spain, France, Holland, Hungary, Italy, Norway, Portugal, Roumania, Russia, Sweden, Swizerland, as well as from the United States, Canada, Mexico, the West Indies, the Argentine Republic, and Australasia. From all these countries eminent and illustrious men honour us with their presence, and are here to aid us by their

[1] Inaugural Address delivered by Prof. J. Prestwich, President of the Congress, on September 17, 1888. (Translated from the French.)

knowledge in the discussion of the questions brought before the International Congress. The number of geologists present on this its fourth meeting indicates the continued and deep interest that they take in it.

Among the more permanent officers are the Secretaries of the Congress and of its Committees to whose important and gratuitous services we are so deeply indebted. We have unfortunately to deplore the untimely death of one amongst them—M. Charles Fontannes—and we lose on this occasion the benefit of his long experience and valuable aid.

According to custom, our discussions are, as in the diplomatic world, held in French; but it is to be hoped that the *entente cordiale* will be better maintained than it sometimes is in the other case, where such councils have not always succeeded in avoiding strife. If I may be permitted to speak after an experience of half a century, an *entente* of the most cordial character between us English geologists and our colleagues and friends abroad has been during these long years the normal condition. May these friendly and loyal relations prove a legacy to our science for all time. These friendly meetings were, however, only occasional, so that the opportunities for personal interchange of ideas were few. But more lately, instead of discussing unsettled questions, each nationality apart, the happy idea arose of submitting certain questions, which concern us all, to the arbitration of this General Council. In this manner the different national centres of our science, which have each their local colouring and their special experience, are enabled to combine the results arrived at in a wider and more uniform manner than if each apart worked out its ideas, based necessarily on more restricted observations. Nevertheless, in giving to our science the uniformity of terms and of classification which is so necessary, care must be taken not to draw lines too tight, such as, instead of developing, might retard its progress. It is desirable that these lines should be so elastic as to adjust themselves to the rapid development we have reason to expect in geological science. It is highly necessary that we should agree upon the colours and symbols to be used for the different strata, rocks, and disturbances that the terrestrial crust presents to us, but petrology is still far from being placed on firm foundations, and the synchronism of the beds, even between near countries, is not always easy to determine with exactitude, and still less between distant countries. Let us then try to avoid that error of Congresses—of arrogating an infallibility which is little in accordance with the progress of science.

Let me now say a few words upon what the Congress has already accomplished, and on what remains to be done.

At Bologna, Prof. Capellini gave the history of the Congress so fully that there is no need that I should speak of it unless it be to remind you that the idea of the Congress originated in America at the Exhibition of Philadelphia in 1876, and doubtless this idea, as well as that of the Exhibition itself, was only the expression of a desire that had been very generally felt for some time, to treat certain questions of science and art, not only, so to speak, in a national family reunion, but in a cosmopolitan reunion—to treat the great questions that concern all humanity, as belonging to the whole civilized world, and for the purposes of discussion, to make of the various nationalities a brotherhood, established on their common interests and their common weal.

The Paris Congress.—At the first Congress, which met in Paris in 1878, the primary questions of nomenclature and of classification were sketched out, as well as the unification of geological works with regard to colours and figures, so that in all countries their signification should be the same. A proposal, which was at first well received, was to make use of the solar spectrum, and to adopt the three primary colours—red, blue, and yellow—for the three divisions of the first rank of Primary, Secondary, and Tertiary rocks; that the subdivisions of the second order should be distinguished by shades of these colours, and those of the third order by hatchings of these same colours. But subsequently this scale was found to be too restricted, and at Bologna and Berlin several modifications and complementary colours were introduced, although always retaining to a certain degree the original idea. As a corollary it has been suggested that the labels of fossils should, as has already been done in several Museums, be of the same colour as that used for the strata from which they come, and that thus one would at a glance see the horizon and age of the fossil.

As to the question of unification of nomenclature for the great divisions of the earth's crust, it was felt that it is in the first place essential that there should be perfect agreement about the terms in use, and therefore that a dictionary of geology comprising the etymology or the origin of each geological name, its synonym in other languages, a definition in French, and a demonstrative figure after the manner of technological dictionaries, would be of very great use. The publication of such a work, which ought to be in at least six languages, was strongly supported. Finally, the consideration of the foregoing questions was referred to the International Commissions to report upon to the meeting of the Bologna Congress.

With regard to the classification of the strata, memoirs were received upon the Pre-Cambrian rocks, and on the nomenclature of the Palæozoic strata of North America; on the limits of the Carboniferous and Permian in various parts of Europe and in America; on the relations of the zones of extinct Vertebrates in North America and in Europe; these two last memoirs being accompanied by valuable lists of Invertebrates, plants, and reptiles of different countries. These memoirs raised very important stratigraphical and palæontological questions with regard to the wide distribution of families and of genera. Each of the faunas of the primary divisions of geological periods has been in part recognized as occurring at the same time in the two continents—in Europe and in North America: and Prof. Cope has been led to inquire whether the organic types proceed from a special centre from which they have spread; or whether the same types of generic structure have appeared independently at different points of the surface of the globe; and if so, whether they are contemporary or of varying periods. These synchronous appearances form a subject full of mystery, from whatsoever side they may be viewed. The geological record is at present too incomplete for the problem to be solved. In each country there are gaps that can only be filled by aid of continued observations in the other parts of the world. One of the most useful functions of the Congress is to encourage these.

The classification of Quaternary deposits was also discussed in relation to the remarkable history of the caves of Central France; the glacial deposits and dunes of Holland; the Tertiary beds of Portugal, which are limited to the Miocene and Pliocene; the Tertiary eruptive rocks of Hungary, viewed as to whether there is not a certain relation between the mineralogical constitution and the relative age of the various trachytic types.

The Congress was also occupied with some high physical questions, such as those of the deformations and fractures of the earth's crust; the strike and dip of faults and of chains of mountains; the origin of volcanoes, and the probable causes of great earthquakes; the structure of the Alps, and the folds of the Chalk.

Less in connection with the fundamental objects of the Congress, but having nevertheless an interest of their own, were the memoirs on the feldspars, on the alteration of the superficial deposits, on the use of the polarizing microscope, on the conductivity of heat in rocks, and other special subjects.

The Bologna Congress.—In the handsome volume of the Proceedings of the Session at Bologna, will be found the Report of the International Jury appointed to judge the competing memoirs on the unification of colours and geological signs, towards which the King of Italy generously gave 5000 francs to be awarded to the best memoir considered practically applicable. Six memoirs were received, of which the three selected for the award are published with coloured illustrations which leave nothing to be desired. The authors of these papers were of opinion that although the solar spectrum offers a very advantageous fixed base, the scale of colours is insufficient, and that it would be necessary to introduce complementary colours, or those having relation to the primary colours. The divisions, in short, of the sedimentary strata are so numerous that it will be necessary, not only to employ those colours, but also several shades of the same, or different hatchings, in reserving rose colour for the crystalline Archæan schists. For the eruptive rocks, they all agreed to use dark and bright tints of red, green, and purple, the intensity of which will render them to be readily distinguishable from the primary colours of the sedimentary rocks and from the clear colour of the schists. It was attempted to distinguish the acid and basic rocks, both with respect to their petrological composition and their age, by the use of different tints of the same colours in coloured dots, or by hatchings of various patterns, and with the letters of the Greek alphabet. Thus it is proposed to show by signs the principal varieties of granitic, porphyritic, trachytic, andesitic, and basaltic rocks, &c.; but the varieties are so numerous that one hardly knows where to draw the limits; according to one plan, the use of seventy-six signs and hatchings would be required. You will be able to judge of the various methods proposed by the fine plates which illustrate the Reports. The sections given of some of the mountains of Switzerland, and others which serve as specimens, have an excellent effect. Conventional signs are also made use of to indicate the strike and dip of the strata, faults, fossiliferous localities, sources of cold, thermal, and mineral springs, travertines, quarries, mines, &c. A geological map will thus be a veritable hieroglyphic chapter, with a universal signification.

As a result of the discussions at Bologna, and with a view to a practical application, it was decided to publish a geological map of Europe on the scale of 1/1,500,000, in which the scale of colours used would be that definitely adopted by the Congress. This map, of which the execution is well advanced, is under the direction of a Committee at Berlin.

With respect to the unification of geological terms, Reports were received from nine National Committees, viz. from Austria, Belgium, Spain, Portugal, France, Great Britain, Hungary, Italy, Russia, and Switzerland. Besides these, eleven have been received from individual members. It can be well imagined that with so many opinions they were not all in agreement, but with the good will shown by everyone, although there were differences on points of detail, they were almost unanimous on the essential points, and a preliminary general agreement was arrived at for the stratigraphical terms, such as system, group, series, stage; and for chronological terms, such as era, epoch, age, &c., leaving to future Congresses the consideration of certain subordinate points. This subject reminds me, gentlemen, of a difficult question which has yet to be faced. If your resolutions are carried by the votes of all the members of Congress, the result must be affected by the varying number of the nationalities in the changing places of meeting. For example, at Bologna there were 149 Italian members and 19 English; at Berlin there were 163 Germans and 11 English; here, on the contrary, we are . . . English and . . . foreign geologists. Therefore, if all vote, the opinion of the seat of the Congress may too much preponderate unless you find means of placing some limits upon it.

Thanks to the loyalty of the Bologna Council, the greater number of the resolutions were carried unanimously, a few only were referred to various Committees for future consideration.

With respect to the stratigraphical divisions it was resolved:—(1) That the term "group" should be applied to each of the great divisions of Primary, Secondary, and Tertiary rocks. (2) That the subdivisions of these groups should be named "systems." You have thus a Primary or Palæozoic group, and the Silurian system, the Jurassic system. (3) As to the divisions of first order of the systems, the term "series" was applied (the Oolitic series); to those of the second order, the term "stage" (the Bajocian stage); and to those of the third order the word "zone" (the zone of *Ammonites humphresianus*). The unity of the stratified masses is the stratum or bed. With regard to a word much in use in England, and dating from the primary period of geology—the word "formation," the majority of the Congress decided not to employ it in the sense of *terrain* in French, as we do, but only in the sense of origin or mode of formation, and so on. It is necessary, therefore, to seek some word to replace with us the familiar terms of "Chalk formation," "London Clay," &c.

For the chronological divisions corresponding with the stratigraphical, it was proposed that (1) "era" should correspond with "group," as the Primary era, the Secondary era; (2) "period" with "system," as the Silurian period, the Cretaceous period; (3) "epoch" with "series," as the Lower Oolitic epoch, the Lower Cretaceous epoch; "age" with "bed," as the Portlandian age, the Bathonian age, &c.

On the subject of colours and signs, the final decision was remitted to the Committee of the Geological Map; and with regard to the rules to be followed in the nomenclature of species, it was resolved that the name attached to each genus and to each species should be that by which they have been earliest known, on the condition that the characters of the genus and species have been published and clearly defined. The priority not to date beyond Linnæus, twelfth edition, 1766.

There were only four special and local memoirs presented to the Congress at Bologna, and these were in support of collections and documents exhibited.

THE BERLIN CONGRESS.—The official Proceedings of this session having only been issued during the last few days, were not available when this address was prepared. I have therefore had recourse for information to the independent notices of Messrs. Renevier, Klebs, Choffat, Frazer, Blanford, and Dewalque. At Berlin, special attention was given to the construction of the geological map, of which the Committee, profiting by the liberty given to it by the Bologna Congress, revised the colours for the sedimentary series in the following manner:—

1. Recent deposits (Alluvium, &c.)	Very pale cream colour.
2. Quaternary (Diluvium)	Naples yellow.
3. Tertiary	Various shades of yellow.
4. Cretaceous	Green tints and hatchings.
5. Jurassic	Blue tints.
6. Triassic	Violet tints and dots.
7. Permian and Carboniferous ...	Gray tints and hatchings.
8. Devonian	Brown tints.
9. Silurian	Grayish-green tints.
10. Archæan	Rose tints.

And for the ten divisions of eruptive rocks, various brilliant and dark red tints and points.

In the use of monograms to accentuate the tints, it was decided to employ Latin initials for the sedimentary deposits, and Greek initials for the eruptive rocks.

It is on this plan that the large and grand geological map of Europe in course of execution at Berlin is to be coloured, and of which the publication will realize one of the principal practical objects of the Congress—the unification of the colours employed in geology.

As to stratigraphical unification, the Congress adopted, for the most part, the resolutions passed at Bologna. But the French and Portuguese Committees proposed to substitute the term "series" for "group" in the first and third great divisions of sedimentary strata; thus, instead of Primary group, Secondary group, &c., it will be Primary series, Secondary series, &c. The word "group" will then take the place of divisions of systems, such as Oolitic group instead of series. This replacement will perhaps recommend itself to many of us.

Further, the Committees were not unanimous on the proposition to substitute, for the various existing terminations of systems, homophone terminations in *ic*. Instead of speaking of the Eocene, Cretaceous, Carboniferous, Silurian, &c., system, it was proposed to use the terms Eocenic, Cretacic, Carbonic, Siluric, &c., system. Is it essential thus to change the ancient ensigns of our science? Etymology is lost, and signification destroyed. It is well to have these terminations for things positive, such as the crystalline and eruptive rocks—for example, granitic rocks, porphyritic rocks, basaltic rocks—for here it indicates their characters; but can we subject, or is it needful to subject, several series of deposits that have no character in common to the same rigid rule, from the circumstance that they all come under the same ideal classificatory name? This question will be discussed, and it is for you, gentlemen, to judge what solution may be the most advisable.

Among other subjects, gentlemen, that you will have to consider, is that of the classification of the Cambrian and Silurian strata. According as these two great systems have been taken in descending or ascending order, the boundary between the two has been placed lower or higher, because the discordances between the series are rare, and the palæontological chain between the two systems is but little interrupted. In England, Sedgwick, who commenced from below, found himself stopped by no discordance until he reached the Mayhill Sandstone, whereas Murchison, who commenced from above, saw no reason to stop until Palæozoic life failed him; he hesitated, therefore, where to place his base line. In the same way, in those countries where they followed Murchison, whose classification was better known, the stratigraphical barriers were, according to the partisans of the one, passed over; whilst, according to the partisans of the other, there was an absence of palæontological proofs. In this country—their native stratigraphical country—the Cambrian and Silurian occupy comparatively a small area; and it is only since the death of their founders that the palæontological proofs have been increased to an extent sufficient to bring out clearly their distinctive characters. These two systems are found elsewhere (especially in America, where it is a question whether they should be associated with a Taconic system), either better developed, or with special characters which may help to determine more precisely their mutual relations. It is here, again, gentlemen, that the knowledge that you bring from many parts of the world may aid us in throwing light on this difficult subject.

Among the other questions which preceding Congresses have not decided, are:—

(1) The relation between the Carboniferous and the Permian.
(2) Between the Rhætic and the Jurassic.
(3) Between the Tertiary and the Quaternary.

When there is no interruption in the continuity of the strata, and no discordant stratification, the systems pass one into another without apparent break, like the colours of the solar spectrum; but, as you all know, if one link is wanting, the chain is broken, and the line of separation of the disunited beds becomes sharply defined. If, for example, the Caradoc should be absent in the Cambrian-Silurian, or the Pliocene should be wanting in the Tertiary, there would be between these systems a break which would give the necessary relief to the superimposed strata. The primary colours of the spectrum are not less distinctive because they pass one into the other with intermediate shades; nor does it follow that, because there are passage-beds, the systems form one whole. There must be, somewhere, passage-beds between them, as there are between the colours.

Apart from these international questions, the Berlin Congress was occupied with several special memoirs, but we are yet without particulars, and besides, whatever may be their interest, they concern us less for the moment than international questions. Among others of the latter, a great palæontological project has been mooted, and the Congress has appointed a Commission of distinguished palæontologists to co-operate towards its realization. A work is proposed, on the plan of the "Enumerator et Nomenclator" of Brown, and of the "Prodrome" of Alcide d'Orbigny; but such is the progress that palæontology has made, that at present, for the enumeration of all the known fossils, of animals as well as plants, a publication of some fifteen large volumes would be required. A work of this kind will make a handsome pendant to the large polyglot dictionary of geological terms, projected at Bologna.

Such, gentlemen, are some of the questions and subjects that you have to consider. You have to revise and to settle, when possible, questions already discussed, and also to discuss new problems. Among the latter there is especially the fundamental question of the crystalline schists—a subject remarkable for the great progress that it has made during the last few years, and the entirely new aspect that it is assuming; for it is evident at present that it is not only a chemical question of metamorphism by heat, but that it is a subject which entails questions of weight, pressure, and motion, which necessitate a wide co-operation, and the combined efforts of the physicist, the chemist, the petrologist, and the stratigraphist.

Although the greater number of the subjects considered by the Congress are eminently practical and positive, they also include theoretical questions of the highest interest. The classification of the strata and their synchronism over great areas, which you have to determine, rest both upon stratigraphy and upon palæontology. In order to adjust their precise relation, you have to note the identities as well as the differences of fossil species, and to know if the order of the beds in distant countries follows a synchronous order or is only homotaxial. In the one case, we can hardly expect to find similar species; in the other, the identity of species may be taken as a proof to the contrary, unless it may be supposed, as Edward Forbes thought, that species have had more than one centre of origin.

To solve these problems you have to trace the dawn of life, the appearance, the duration, and the disappearance of species, and the source from which they come. Are we to believe in the evolution of species, or are we to regard them as shoots of short duration, and the genera or families as the branches or permanent trunks? If I have ventured to touch upon these problems of fact and theory, it is not to express an opinion, but merely to point out how vast the field is, and how many fellow-labourers and how long is the time required to make all the necessary studies.

It must not be thought that when the fundamental questions of fact are determined the work of the Congress approaches completion. General agreement on these international questions will only smooth the way, and one can foresee in the cosmopolitan problems of theory already considered, and in many others that cannot fail to arise, what will occupy in a long and useful future all the efforts of this International Congress.

ON THE CONSTITUTION AND STRUCTURE OF THE CRYSTALLINE SCHISTS OF THE WESTERN ALPS.[1]

TEN years have elapsed since Prof. Lory first formulated his views on the crystalline schists of the Western Alps, at the Congrès International de Géologie held in Paris in 1878. These he subsequently developed at the Réunion de la Société Géologique de France at Grenoble in 1881. Since then further work in the field has strikingly confirmed these views, and Prof. Lory has taken advantage of the opportunity given by the invitation of the Organizing Committee of the Geological Congress to summarize briefly the more important facts, derived from the study of the Western Alps, that have a direct bearing on the general question of the crystalline schists.

The crystalline schists appear in the Alps in *massifs* of greater or less extent, protruding through the sedimentary formations. These *massifs* are distributed in two principal zones, arched in agreement with the general curvature of the Alps. These the author proposes to designate *the first Alpine zone*, or *Mont-Blanc zone*, and *the fourth Alpine zone*, or *Monte-Rosa zone*. The intermediate zones (*second* and *third Alpine zones*) are of less importance, the outcrops being rare and of small extent. As they resemble *the fourth zone* in their principal characters, they are treated in its connection.

(1) The *fourth Alpine zone*, or *zone of Monte-Rosa*, is by far the largest. In it the crystalline schists are exposed over the greater part of the Italian slopes, and skirt the plain from Cuneo to Lake Maggiore. Their stratification is often nearly horizontal, and always conformable with the sedimentary formations (Trias or Jura) resting upon them.

It is subsequent to the deposition of these Secondary rocks and, very probably, even much later—in Tertiary times—that this part of the Alps has been fashioned into mountains by the lateral pressure resulting from the gradual subsidence of the vast regions represented by the plains of Italy and the basin of the Adriatic. The result of these important dynamic processes was the formation of a complex of great folds, which are often much complicated by faulting.

The succession of the different groups of crystalline schists in this zone is conformable to the order indicated, long since, by Cordier. It is necessary to point out, however, that this upper group—that of the *talcites* (talc-schists)—contains talc only as an accessory constituent; the unctuous (talcoid) aspect being due, in reality, to the presence of certain indistinctly cleavable and fibrous varieties of mica, especially sericite. These schists may be termed sericite-schists or, abbreviated, serischists. In the purer varieties they are of a nacreous white or clear gray colour; but by the addition of chlorite they assume greenish tints and pass into chloritic and quartzose schists—the chloritoschists which attain so great a development in the whole of the Western Alps. Alternating frequently with these rocks are hornblendic schists, of which the development is very variable. In certain parts of the Italian Alps, however, especially between Ivrea and Domo d'Ossola, they become predominant.

This upper division of the crystalline schists is characterized by a more or less pronounced green tint, due to the presence of chlorite or hornblende, which recalls the name *pietre verdi*, given to these and other schists by Gastaldi and several other Italian geologists.

Below the chloritic and hornblendic schists occurs a large series of mica-schists, with which are intercalated, in conformable bedding, cipolin-limestones (*calcaires cipolins*), granular dolomites, and pure saccharoidal limestones, alternating with mica-schists and evidently forming part of the same formation.

The mica-schists become charged with felspar and pass thus into gneiss, with which they alternate. Black and white micas are associated in these rocks. In proportion as the series is descended, orthoclase becomes more abundant, and the gneisses predominate with a foliation which decreases until they pass into granitoid gneiss, in which the foliation disappears, but the broader features of stratification remain visible. This is well shown in the section of the Simplon *massif*, where the gorges of the Diveria are hollowed out, to a depth of 700 metres, in the horizontal beds of the granitoid gneiss known as the gneiss of Antigorio.

[1] "Sur la Constitution et la Structure des Massifs de Schistes Cristallins des Alpes Occidentales," par M le Professeur Ch. Lory. "Etudes sur les Schistes Cristallins." London, 1888. (Abstracted from the French by Dr. F. H. Hatch.)

Prof. Lory does not recognize in the *Monte-Rosa zone* any beds belonging to the Carboniferous; and he believes that the crystalline schists of this part of the Alps have been exposed during the whole of Palæozoic times, without having been disturbed from their primitive horizontal position. They have gradually subsided during the Triassic period. The lower stages of this formation are not much developed in this zone; but the upper stage, represented by the *schistes lustrés*, have acquired an enormous thickness.

These Triassic beds are characterized by a remarkably crystalline texture. The limestones and dolomites which form the middle stage are granular and saccharoidal, and inclose authigenic crystals of albite. The *schistes lustrés* are composed in great part of crystallized minerals (quartz, mica, tourmaline, garnets, &c.), which are also certainly authigenic. This crystalline condition is uniform and constant, and independent of all dislocations and contortions which the beds have subsequently undergone.

The crystalline character of the sedimentary formations may be of assistance in understanding the origin of the crystalline schists. The foliation is generally parallel to stratification, the latter being always very distinct. Characters so uniform cannot be explained by the phenomena of slaty cleavage and crystallization under the influence of local mechanical actions. It is rather a general, universal, and original crystallization of the primitive rocks, which took place anterior to the deposit of all sedimentary formations.

The most important element of Prof. Lory's *third zone* are anthracitic sandstones. These sandstones belong to the Upper Coal-measures (*houiller supérieur*). The boundary between them and the crystalline schists is usually marked by a fault. But sometimes, as at the bridge of St. André, near the railway station at Modane, the latter appear under the sandstones, and then the foliation of the crystalline schists is conformable with the bedding of the Carboniferous sandstones. At this and other localities there occur in the lower portions of these sandstones conglomerates formed of slightly rolled fragments of crystalline schists, identical with those which crop out in the neighbourhood. It is therefore evident that the foliation and crystallization of the crystalline schists must be earlier than the Carboniferous period. Conglomerates, composed of fragments of the most diversified rocks from the crystalline schists, occur in the Upper Trias, in the Lias (*Col du Golet*), and in the Nummulitic Eocene (*massif des Encombres*). Each of these conglomerates contains fragments of all the preceding formations. Since these rolled pebbles have the characteristic structure, crystalline or foliated, of the rocks they are derived from, and since the foliation of the pebbles has no uniform direction in the conglomerates, it follows that the foliated or crystalline texture of the rocks of these various formations is, each for each, of earlier origin than the deposition of that which overlies it, and absolutely independent of the powerful mechanical actions which only fashioned these formations into mountains subsequently to the Eocene period.

Again, all the formations, from the Trias to the Eocene, contain microscopic crystals of silicates (felspars, mica, quartz, tourmaline), which are of contemporaneous origin with the rocks containing them, and do not, therefore, owe their existence to any of the dynamic processes which have subsequently acted upon this part of the Alps.

Since these silicates, which are identical with, or very analogous to, those of the crystalline schists, were formed in the Secondary and Tertiary deposits independently of all eruptive actions or special emanations, and anteriorly to all dynamic processes, it is unnecessary for the explanation of the origin of the primitive crystalline schists to assume physical conditions absolutely different from those of the Secondary or Tertiary periods.

In the remote epoch in which these schists were formed there were no terrestrial features, and consequently no detrital formations. The existence of organisms in a universal ocean, warmer and more heavily charged with saline matters than actual seas, was not yet possible; and there resulted combinations of crystallized minerals, the formation of which in later times became more local and restricted. But even as late as Tertiary times we still find traces of analogous reactions in the deposits of those remarkable *fiords* of the Eocene period which extend over a part of the actual site of our Alpine chains.

(2) Prof. Lory's *first Alpine zone*, or *Mont-Blanc zone*, comprises, in Switzerland, the *massifs* of the Bernese Alps and of St. Gothard; in Savoy, those of the Aiguilles Rouges and of Mont-Blanc; the chain of Belledonne; the small *massif* of Rocheray, near St.-Jean-de-Maurienne; the *massif* of Rousses, in Oisans; the *massif* of Pelvoux, between Drac and Durance; finally, the *massif* of the Maritimes Alps, between the Col de l'Argentière and the Col de Tende.

The characteristic feature common to all these *massifs* consists in the crystalline schists composing them being nearly always highly inclined or almost vertical. They do not appear to present the regular structure—the great anticlinal folds of the *Monte-Rosa zone*. This indicates that the *Mont-Blanc zone* is really the ancient part of the orogenic system of the Alps, and that its structure has resulted from the dislocations of different epochs.

Anthracitic sandstones occur also in this zone, but they are less developed and less continuous than in the *third zone*, and, as indicated by their plant remains, are of more recent date, being intermediate between the Coal-measures of Rive-de-Gier and those of Saint-Etienne.

On the western slope of this zone traces of dislocations, anterior to the deposition of these Carboniferous sandstones, can be recognized. They are indicated by clear unconformities at various points in the Mure basin and other places. But on the eastern slope of the same zone the Carboniferous sandstones and the crystalline schists are generally conformable.

These Carboniferous sandstones of the *first zone*, like those of the *third*, are accompanied by conglomerates containing numerous fragments of foliated crystalline schists, of which the petrographical characters are identical with those of the underlying crystalline rocks. These conglomerates are well known on both western and eastern slopes (*poudingues* of Valorsine, Grandes-Rousses, &c.) Since the Carboniferous sandstone on the eastern slope is conformable with the crystalline schists, the existence of large fragments of the schists in these conglomerates, clearly demonstrates that their foliation is anterior to all dislocations which have affected the *massif*. It was after the deposition of the anthracitic sandstone, between the Carboniferous and Triassic periods, that the principal dislocations took place, which have upheaved and contorted the crystalline schists and the anthracitic sandstones of the *first zone*. Wherever the Triassic beds appear nearly horizontal they rest, in conformable stratification, on the upturned edges of the older formations, whether anthracitic sandstones or crystalline schists.

The horizontal position of numerous shreds of Secondary rocks to be found at very variable heights indicates the character of the dislocations which have taken place at more recent periods in this part of the Alps. The ancient formations, already upheaved and contorted before the deposition of the Trias, have behaved like rigid masses, and have not lent themselves to the newer folding. They have been traversed by faults; and displacements have taken place along the planes of fracture, while at the same time following the divisional planes of stratification. The Secondary rocks, on the other hand, have behaved like flexible, and even, when argillaceous, like plastic bodies. They have only been completely fractured by the more important major faults; everywhere they have moulded themselves by multiplex folding to the new forms of their dislocated base. This flexible covering has slipped into the depressions formed by the subsidence, due to dislocation, of certain parts of its base. In this way the Secondary rocks present themselves on the flanks of the Alpine valleys in beds which are inclined and contorted in repeated folds, contrasting thus with the uniform curvature of the ancient rocks.

The powerful mechanical actions resulting from these dislocations of the *first Alpine zone* have often superinduced, in the argillaceous limestones of the Lias, phenomena of "stretching," lamination, and, above all, a slaty cleavage in a direction different from that of stratification. As to the crystalline schists, of which the plication took place at the end of the Carboniferous and before the Triassic period, the more recent dislocations have destroyed the regularity of their anticlinal and synclinal folds. Along the axes of the anticlinal ruptures, or following the bands of mica-schists—that part of the crystalline schists which offers least resistance—occurred the subsidences which have given rise to the actual Alpine valleys; it is following these directions, and *nearly always following the old synclinal folds*, that the ancient rocks have been cut up into *massifs*, separated by the bands of depression, where the Secondary rocks, adapting themselves to the new forms assumed by their base, have descended while undergoing plication; and their beds, highly inclined and often curiously folded, clothe the lateral walls of these depressions. The valley of Chamonix and l'Allée Blanche, the Combe d'Olle, the lower valley of the same stream, at Allemont, and that of Bourg-d'Oisans, are examples of this type of longitudinal Alpine valleys of the *Mont-Blanc zone*.

The *massifs* of crystalline schists represented in this zone are large remnants which have remained standing in ruins, the other parts of the primitive rocks having subsided either *en masse*, following great faults, or in detail, by a series of small slides, following the numerous joints, or the divisional planes of bedding. *Not one of them represents a regular and complete anticlinal fold.*

The various types of crystalline schist comprised in the Mont-Blanc zone succeeded one another in the same order as in the *Monte-Rosa zone*. They are also divided into two groups: the upper group—sericitic, chloritic, and hornblendic schists; and the lower group—mica-schists and true gneisses.

In the lower group there is a tendency towards the granitoid structure, and the rocks appear more or less massive, but yet in the main stratiform. They become rich in white mica, and assume a granulitic texture. These phenomena are developed along the anticlinal axes.

The crystalline schists of the upper group have a tendency to become richer in felspar the nearer one approaches the intra-Alpine limit of the zone. It seems that this corresponds with the direction in which alkaline emissions, accompanying the formation of these rocks, took place, the same direction afterwards becoming that of the great limiting fault of the zone. The schists pass thus into chloritic gneisses similar to those occurring near the station at Modane (*third zone*), or to the gneiss of Arolla (*fourth zone*); sometimes also into granitoid gneisses, both chloritic and hornblendic, as, for instance, at Cevins, in Tarantaise.

The tenacity of the chloritic and hornblendic schists, which is generally much superior to that of the mica-schists and true gneisses, and their tendency to develop felspar, which gives them greater consistency, explain the important *rôle* played by these rocks in the constitution of the culminating ridges and steeper *massifs* of the *first zone*. In the Mont-Blanc *massif* and in the eastern portion of the Pelvoux *massif* these "needles" and abruptly culminating ridges characterize the type of rock known as *protogine*. This name, the etymological sense of which must be forgotten, has been created to designate the type of rocks which predominates in the principal ridge of Mont-Blanc. The special character of these rocks consists in the mica being penetrated and partly replaced by chlorite. The granitoid protogine always contains two felspars—orthoclase and oligoclase, part of the orthoclase being usually replaced by microcline.

Prof. Lory thinks the protogine belongs to the upper group—that of the chloritic schists. In that case Mont-Blanc cannot be regarded as a central arch of elevation, and its "fan-structure" becomes simply a very sharp synclinal fold of the crystalline schists of the upper group, isolated by two faults, along which they have subsided, while acquiring a **U**-shaped fold.

In the Pelvoux-*massif* the protogine is even more largely developed than at Mont-Blanc. Here also it is stratiform, and alternates with chloritic gneisses like those of the western parts of the *massif*. A series of anticlinal and synclinal folds can be made out. The anticlines correspond to the Vallon des Étages, the Barre des Escrins (west slope), and the Combe d'Alefroide; and the synclines to the Combe de la Pilatte, the eastern slope of the Escrins (Glacier Noir), and the summits of Mont-Pelvoux.

From observations made near Bourg-d'Oisans, the author arrives at the conclusion that the *protogine* has originated by a modification of the chloritic schists. During their formation, a considerable increase in their felspathic constituent was produced by granulitic emissions which took place through the gneiss and mica-schists.

Like other important features in the structure of the Eastern Alps this replacement of chloritic schists by protogine follows the intra-Alpine limit of the *Mont-Blanc zone*, which limit is marked by the great fault-line which can be traced over 60 *lieues*, from Vallonise to Airolo. This must have been the direction in which took place those granulitic emissions, which, without giving birth to true eruptive masses, have modified the character of the old gneiss and mica-schists and developed in the chloritic and hornblendic schists the felspathic character which distinguishes the granitoid rock known as protogine.

THE ELECTRIC TRANSMISSION OF POWER.[1]

WHAT is power, and why should we wish to transmit it? Power has one very definite meaning in science, and several rather vague meanings in practice. We speak of a powerful athlete, the power of the law; we sing of the power of love; we say knowledge is power, and so on, using the word in several different senses. Now, in spite of the fact that a general audience feels a little anxious as to what troubles may be in store for it when a lecturer begins by being painfully exact, my telling you that by power an engineer understands the rate of doing work will not, I hope, make you fear that my remarks will bristle with technicalities.

When you walk upstairs you exert power—only, perhaps, the one-twentieth of a horse when you go up slowly, talking to other people. But when you run upstairs because you have forgotten something that you intended to bring down, then your exertions represent, perhaps, the one-tenth of a horse-power. You only get to the top of the stairs in either case, but the breathless sensation of running fast upstairs tells you that the more quickly you go the harder you are working. A person exercises power in the engineer's sense when he exerts himself physically, and the greater the exertion the greater the power. The exercise of power by the ruling classes, however, is unfortunately not necessarily accompanied by any exertion, physical or mental.

Probably the most familiar example of exerting power at a distance—that is, of transmitting power—is pulling a handle and ringing a bell in another room. I pull the handle, exerting myself slightly, and as the result the bell at the other end of the platform rings. Were not this such a very familiar operation I would call it experiment No. 1. You have doubtless all of you performed this experiment several times to-day, and—what is all important with an experiment—performed it successfully.

And yet it was not until just one hundred years ago that it dawned on people that if one person, A, wanted to attract the attention of another person, B, the place where the bell ought to sound was where B was, and not where A was. Indeed, in many English villages down to the present day the knocker principle of attracting attention is alone resorted to, with the result which you may remember happened when Mr. Pickwick was staying in Bath at lodgings in the Royal Crescent, and Mr. Dowler undertook to sit up for Mrs. Dowler, but "made up his mind that he would throw himself on the bed in the back room and *think*—not sleep, of course. . . . Just as the clock struck three there was blown into the crescent a sedan-chair with Mrs. Dowler inside, borne by one short fat chairman and one long thin one. . . . They gave a good round double knock at the street door. . . . 'Knock again, if you please,' said Mrs. Dowler, from the chair. 'Knock two or three times, if you please.' The short man stood on the step and gave four or five most startling double knocks of eight or ten knocks a-piece, while the long man went into the road and looked up at the windows for a light. Nobody came—it was as silent and as dark as ever." But the tall thin man, you may remember, "kept on perpetually knocking double knocks of two loud knocks each, like an insane postman," till Mr. Winkle, waking up from a dream "that he was at a club where the chairman was obliged to hammer the table a good deal to preserve order," met with the catastrophe which the readers of "Pickwick" will remember.

This episode shows what comes of having plenty of power and no means of transmitting it.

But if some houses can still dispense with mechanical or other methods of transmitting power, even to ring bells, factories cannot. The looms, the lathes, or whatever the machinery used in the factory may be, must either be worked by hand or foot in the old style, or it must be connected with the steam-, gas-, or water-engine in the new. On entering a large factory you see lines of rapidly-rotating shafting, and a net-work of rapidly-revolving belting, all employed in transmitting power. As a contrast to this, I now throw on the screen a photograph of Sir David Salomon's workshop at Tunbridge Wells, in which every machine is worked by a separate electric motor, thus saving to a great extent the loss of power that usually accompanies the mechanical transmission.

In America there are 6000 electromotors working machinery; in Great Britain hardly 100.

But it is not only in transmitting the power from the steam-, gas-, or water-engine of a factory to the various machines working in it, that electricity can be utilized. An incredible amount of power is daily running to waste in this and other countries because many of the rapid streams of water are too far away from towns for their power to have been hitherto utilized.

The holiday tourist, when admiring the splashing water dashing over the stones, hardly realizes that the money loss is as if the foam were composed of flakes of silver.

[1] Lecture delivered by Prof. Ayrton, F.R.S., at the Drill Hall, Bath, on Friday, September 7, 1888.

If we take as a low estimate that a large well-made steam-engine burns only 2 pounds of coal per horse-power per hour, the coal consumption which would be equivalent to the waste of power at Niagara would exceed 150,000,000 tons per annum, which at only 5*s*. or 6*s*. per ton means some £40,000,000 sterling wasted. And descending from big things to small, the River Avon, flowing through Bath, which, so far from being a roaring cataract, especially in dry weather, pursues its course with only a respectable orderly swish, still represents a certain amount of lost power. It has been estimated that from 25 to 130 horse-power runs to waste at the Bathwick Weir behind the Guildhall, depending on the season. If we take as an all-round average that the fall of this weir represents 50 horse-power, and that a steam-engine producing this power burns 150 pounds of coal per hour, it follows that with steam coal at 16*s*. per ton—the price at Bath—the waste at Bathwick Weir represents an income of £450 per annum, not a princely fortune, it is true, but too large to be utterly thrown away as at present.

This state of things will I hope, however, be shortly remedied, for, as you will see from the large map on the wall, it is proposed to put up eighty-one electric arc lamps throughout the streets of Bath, and to supply the 50 horse-power required for these lamps by the fall of the Bathwick Weir, supplementing the fall with a steam-engine at dry seasons.

The next large diagram shows the use that Lord Salisbury has made of the River Lea to electrically light Hatfield House, and to supply electric motive power to the various machines working on his estate. The following diagram shows the course of the Portrush electric railway, six and a half miles long, which is worked by the Bushmill Falls, situated at about one mile from the nearest point of the railway. And lastly, this working model on the table, kindly lent me by Dr. E. Hopkinson, as well as the diagram on the wall, represent the Bessbrook and Newry electric tramway, a little over three miles in length, which is also worked entirely by water power, the turbine and dynamo which convert the water power into electric power being at about three-quarters of a mile from the Bessbrook terminus. [Model electric railway shown in action.]

The newspapers of last week contained a long account of the spiral electric mountain railway that has just been opened to carry people up the Burgenstock, near Lucerne, and worked by the River Aar, three miles away, so that we see electric traction worked by distant water power is extending. But, splendid as are these most successful uses of water power to actuate distant electromotors, it is but a stray stream here and there that has yet been utilized, and countless wealth is still being squandered in all the torrents all over the world.

The familiarity of the fact makes it none the less striking, that, while we obtain in a laborious way from the depths of the earth the power we employ, we let run to waste every hour of our lives many many times as much as we use.

It is also a well-established, time-honoured fact that large steam-engines can be worked much more economically than small ones, and that therefore if it were possible to *economically* transmit the power from a few very large steam-engines to a great number of small workshops there would be a great saving of power, as well as a great saving of time from the workmen in these many small workshops having only to employ this power for various industrial purposes, instead of having to stoke, clean, repair, and generally attend to a great number of small, uneconomical steam-engines.

When delivering the lecture which I had the honour to give at the meeting of the British Association at Sheffield nine years ago, I entered fully into Prof. Perry's and my own views on this subject, and therefore I will not enlarge on them now. You can all realize the difference between the luxury of merely getting into a train instead of having to engage post-horses; of being able to send a telegram instead of employing a special messenger; or being able to turn on a gas tap and apply a match when you want a light, instead of having to purchase oil and a wick, and trim a lamp. Well, a general supply of power to workshops is to the manufacturer what a general supply of light or a general supply of post-office facilities is to the householder: it is all part of the steady advance of civilization that leads the man of to-day to go to the tailor, the shoemaker, the baker, the butcher, instead of manufacturing his own mocassins and lassoing a buffalo for dinner. And in case any of you may be inclined to think that we have gone far enough in these new-fangled notions, and we are all perhaps prone to fall into thls mistake as we grow older, let me remind you that while each age regards with justifiable pride the superiority of its ways to those of its ancestors, that very age will appear but semi-civilized to its great-grandchildren. Let us accept as an undoubted fact that a general distribution of power would enable the wants of civilized life to be better satisfied, and therefore would greatly benefit industry.

There are four methods of transmitting power to a distance: (1) by a moving rope; (2) by air compressed or rarefied at one end of a pipe operating an air motor at the other end; (3) by water forced through a pipe working a water motor; (4) by electricity.

We have an example of the transmission of power through a short distance by an endless belt or rope in the machine geared together by belts on this platform, and in the rotatory hair-brushes at Mr. Hatt's establishment in the Corridor, Bath. At Schaffhausen, and elsewhere in Switzerland, the principle is employed on a large scale. Spain and other countries use it in connection with their mining operations; and lastly, wire ropes replace horses on many hilly tramways. Do not look, however, for the wire rope of the Bath cable tramways, for cable is only to be found painted on the sides of the cars.

For short distances of a mile or so there is no system of transmitting power in a *straight line* along the open country so cheap to erect, and so economical of power as a *rapidly*-moving endless rope; but the other systems give much greater facilities for distributing the power along the line of route, are much less noisy, and far surpass wire rope transmission in economy when the rope must move somewhat slowly, as in tramway traction, or when the distance is considerable over which the power is transmitted, or when the line of route has many bends.

In the same sense that an ordinary house-bell may be considered as a crude example of the transmission of power by a moving rope, the pneumatic bell at the other end of the hall which I now ring by sending a puff of air through the tube is a crude example of the transmission of power by compressed air. [Pneumatic bell rung.] Compressed air is employed to work from a distance the boring-machines used in tunnelling. The continuous vacuum-brakes used on many of the railways are also probably familiar to you, and the pneumatic system of transmitting power to workshops is shortly to be tried on a fairly large scale at Birmingham.

But distribution of power by water pressure is the plan that has hitherto found most favour in this country. That little water motor at the other end of the platform rapidly revolves when I work this garden syringe, and serves as a puny illustration of the transmission of water pressure. [Experiment shown.] Pressure water has been employed for years on a large scale at Hull for distributing power; also by Mr. Tweddle, as a means of communicating a very large amount of power through a flexible tube to tools that have to be moved about; but the grandest illustration of this principle is the vast system of high-pressure mains that have been laid throughout London, as you will see from the photograph that I now project on the screen of the map kindly lent me by Mr. Ellington.

The economy of this system is so marked and the success that has attended its use is so great that, did I not feel sure that electricity offers a grander system still, it would be with fear and trembling that I should approach the subject of this evening, the "Electric Transmission of Power." *Punch* drew six years ago the giant Steam and the giant Coal looking aghast at the suckling babe Electricity in its cradle. That baby is a strong boy now; let the giant Water look to its laurels ere that boy becomes a man. For the electric transmission of power even now bids fair to surpass all other methods in (1) economy in consumption of fuel; (2) more perfect control over each individual machine, for see how easily I can start this electric motor, and how easily I can vary its speed [experiment shown]; (3) ability to bring the tool to the work instead of the work to the tool—this rapidly-rotating polishing-brush, with its thin flexible wires conveying the power, I can handle as easily as if it were a simple nail-brush; (4) in greater cleanliness, no small benefit in this dirty, smoky age; (5) and lastly, there is still one more advantage possessed by this electric method of transmitting power that no other method can lay claim to—the power which during the day-time may be mainly used for driving machinery can, in the easiest possible way, be used during the night for giving light. I turn this handle one way, and the electric current coming by one of these wires and returning by the other works this electromotor; now I turn the handle the other way, and the current which comes and returns by the same wires as before keeps this electric lamp glowing. [Experiment shown.]

It might be said that the transmission of power by coal-gas, which I have excluded from my list, fulfils this condition, but so also does the transmission of power by a loaded coal-waggon. In both these cases, however, it is fuel itself that is transmitted, and not the power obtained by burning the fuel at a distant place.

Let us study this electric transmission a little in detail. I pull this handle, and the bell at the other end of the room rings; but in this case there is no visible motion of anything between the handle and the bell. [Electric bell rung by an electric current produced by pulling the handle of a very small magneto-electric machine.] Whether I ring the bell by pulling a wire, or by sending an air puff, or by generating an electric current by the exertion of my hand, the work necessary for ringing the bell is done by my hand exactly as if I took up a hand-bell and rang it. In each of the three cases I put in the power at one end of the arrangement, and it produces its effect at the other. In the electric transmission how does this power travel? Well, we do not know. It may go through the wires, or through the space outside them. But although we are really quite in the dark as to the mechanism by means of which the electric power is transmitted, one thing we do know from experience, and that is this: given any arrangement of familiar electrical combinations, then we can foretell the result.

Our knowledge of electrical action in this respect resembles our knowledge of gravitation action. The only thing quite certain about the reason why a body falls to the ground is that we do not know it; and yet astronomical phenomena can be predicted with marvellous accuracy. I mention the analogy, since some people fancy because the answer to that oft-repeated question, "What is electricity?" not only cannot be given exactly, but can only be guessed at in the haziest way, even by the most able, that therefore all electric action is haphazard. As well might the determinations of a ship's latitude at sea be regarded as a mere game of chance because we have not even a mental picture of the ropes that pull the earth and sun together.

This power of producing an action at a distance of many yards, or it may be many miles, by the aid of electricity without the visible motion of any substance in the intervening space is by no means new. It is the essence of the electric telegraph; and electric transmission of power was employed by Gauss and Weber when they sent the first electric message. I am transmitting power electrically whether I now work this small model needle telegraph instrument, or whether I turn this handle and set in motion that little electric fan. [Experiment shown.]

But until about ten years ago the facility that electricity gave for producing signals almost instantaneously at a great distance was the main thing thought of. The electric power consumed for sending the telegraph messages was so small, the amount of power lost *en route* comparatively so valueless, that the telegraph engineer had no need to trouble himself with those considerations that govern us to-day when we are transmitting power large enough to work a factory or an electric tramway. Although there are as many as 22,560 galvanic cells at the Central Telegraph Office, London, which cost some thousands annually to keep in order, what is that compared with the salaries of all the 3089 superintendents, assistants, telegraph-clerks, messengers, and the maintenance of the 1150 telegraph lines that start from the Central Office?

In all the last three systems in my list some form of power, such as flowing water, or the potential energy stored up in coal, wood, zinc, or other fuel, has initially to be utilized. This power is given to some form of air, water, or electric pump, which transfers the air power to the air, water, or electricity, by which it is conveyed to the other end of the system. There it is re-converted into useful mechanical power by means of an air, water, or electric motor.

You will observe that I class together air, water, and electricity; by that I do not mean to imply that electricity is a fluid, although in many respects it acts like a fluid—like a fluid of very little mass, however; or, odd as it may seem, like a fluid moving extremely slowly, for electricity goes round sharp corners with perfect ease, and without any of the phenomena of momentum possessed by rushing water. But what I particularly wish to impress on you by classing air, water, and electricity together is that electricity is not, as some people seem to think, a something that can be burnt or in some way used up and so work got out of it. Electricity is no more a source of power than a bell-wire is, electricity is a marvellously convenient agent for conveying a push or a pull to a great distance, but it is not by the using up of the electricity that electric lights burn or that electromotors revolve. It is by the electricity losing pressure, exactly as water loses head when turning the miller's wheel as it flows down hill, that work is done electrically.

This model shows, in a rough, symbolical way, what takes place in the transmission of power whether by air, water, or electricity. [Model shown.] The working stuff, whichever of the three it may be, is first raised in pressure and endowed with energy, symbolized by this ball being raised up in the model; it then gradually loses pressure as it proceeds along the tube or wire which conveys it to the other end of the system, the loss of pressure being accompanied by an increase of speed or by its giving up power to the tube or wire and heating it. This is shown in the model by the ball gradually falling in its course. At the other end there is a great drop of pressure corresponding with a great transference of power from the working stuff to the motor, and finally it comes back along the return pipe or wire, losing, as it returns, all that remains of the pressure given to it initially by the pump. The ball has, in fact, come back to its original level.

The problem of economically transmitting power by air, water, or electricity is the problem of causing one or other of these working stuffs—air, water, or electricity—to economically perform the cycle I have described.

In each of the four stages of the process—(1) transference of power to the working substance at the pump; (2) conveyance of power to the distant place; (3) transference of power from the working substance to the motor at the distant place; (4) bringing back the working substance—there is a loss of power, and the efficiency of the arrangement depends on the amount of these four losses. The losses may be shortly called (1) loss at the pump; (2 and 4) loss on the road; (3) loss at the motor.

Until 1870 the pump most generally employed for pumping up electricity and giving it pressure was the galvanic battery—scientifically an extremely efficient converter of the energy in fuel into electric energy, only unfortunately the only fuel a battery will burn is so expensive. A very perfect fire-place, in which there was very complete combustion, and very little loss of heat, but which had the misfortune that it would only burn the very best wax candles, would be analogous with a battery. The impossibility of using zinc as fuel to commercially work electromotors has been known for the last half-century, and the matter was very clearly put in an extremely interesting paper "On Electro-magnetism as a Motive Power," read in 1857 by Mr. Hunt before the Institution of Civil Engineers, a copy of which has been kindly lent me by Dr. Silvanus Thompson. Prof. William Thomson (Glasgow)—I quote from the discussion on the paper—put the matter very pithily by showing that, even if it were possible to construct a theoretically perfect electromotor, the best that could be hoped for, if it worked with a Daniell's battery, would be the production of a one horse-power by the combustion of 2 pounds of zinc per hour, whereas with a good actual steam-engine of even thirty years ago, one horse-power could be produced by the combustion of exactly the same weight of the much cheaper fuel coal. This argument against the commercial employment of zinc to produce electric currents is irresistible, unless—and this is a very important consideration, which is only beginning to receive the attention it deserves—unless, I say, the compound of zinc formed by the action of the battery can be reduced again to metallic zinc by a comparatively inexpensive process, and the zinc used over and over again in the battery. If the compound of zinc obtained from the battery be regarded as a waste product, then it would be much too expensive to work even theoretically perfect electromotors, if they were existent, by consuming zinc. Suppose, however, a process be devised by means of which burnt zinc can be unburnt with an expenditure comparable with the burning of the same weight of coal, then it might be that, although coal would still form the basis of our supply of energy, the consumption of zinc batteries might be an important intermediary in transforming the energy of coal, economically, into mechanical energy.

While, then, some experimenters are aiming at possibly increasing the working power of a ton of coal to eight times its present value by earnestly seeking for a method of converting the energy it contains directly into electric energy without the intervention of a wasteful heat engine, it should not be forgotten that in the cheap unburning of oxidized metal may lie another solution.

The solution of this latter problem is quite consistent with the principles of the conservation and dissipation of energy, since the heat required to theoretically unburn 1 pound of zinc is only one-seventh of that given out by the burning of 1 pound of coal. Further, it involves no commercial absurdity like that found in the calculations given in the prospectuses of many primary battery companies, which are based on zinc oxide, a material used in the manufacture of paint, maintaining its present price even if thousands of tons were produced. Unless all those who use primary batteries on this expectation intend to have the painters doing up their houses all the year round, they will find themselves possessed of the stock-in-trade of an oil and colourman on a scale only justified by a roaring business in paint.

Now about waste No. 3, the waste of power at the motor. That also is gone into fully in the discussion on Mr. Hunt's paper, and Mr. Robert Stephenson concluded that discussion by remarking "that there could be no doubt, from what had been said, that the application of voltaic electricity in what ever shape it might be developed was entirely out of the question commercially speaking. . . . The power exhibited by electro-magnets extended through so small a space as to be practically useless. A powerful electro-magnet might be compared for the sake of illustration to a steam-engine with an enormous piston, but with an exceedingly short stroke. Such an arrangement was well known to be very undesirable."

And this objection made with perfect justice against the electromotors of thirty years ago might also have been made to all the machines then existing for the mechanical production of electric currents. I have two coils of wire at the two sides of the platform joined together with two wires. I move this magnet backwards and forwards in front of this coil, and you observe the magnet suspended near the coil begins to swing in time with my hand. [Experiment shown.] Here you have in its most rudimentary form the conversion of mechanical power into electric power, and the re-conversion of electric power into mechanical power; but the apparatus at both ends has the defects pointed out by Mr. Hunt and all the speakers in the discussion on his paper—the effect diminishes very rapidly as the distance separating the coil from the moving magnet increases.

As long as electromotors as well as the machines for the production of electric currents had this defect, the electric transmission of power was like carrying coals to Newcastle in a leaky waggon. You would pay at least 16*s.* a ton for your coals in Bath, lose most of them on the way, and sell any small portion that had not tumbled out of the waggon for, say, 2*s.* a ton at Newcastle—a commercial speculation not to be recommended.

A very great improvement in electromotors was made by Pacinotti in 1860, but although his new form of electromotor was described in 1864 it attracted but little attention, probably because any form of electromotor, no matter how perfect, was commercially almost useless until some much more economical method of producing electric currents had been devised than the consumption of zinc and acids. Pacinotti's invention removed from motors that great defect that had been so fully emphasized by the various speakers at the reading of Mr. Hunt's paper in 1857. When describing his motor in the Nuovo Cimento in 1864, he pointed out that his principle was reversible, and that it might be used in a mechanical current generator. This idea was utilized by Gramme in 1870, who constructed the well-known Gramme dynamo for converting mechanical into electric power—a machine far more efficient than even Pacinotti had contemplated—and gave the whole subject of electrical engineering a vigorous forward impulse. Every subsequent maker of direct-current dynamos, or motors, has followed Gramme's example in utilizing the principle devised by Pacinotti, which was as follows. In all the early forms of dynamos or motors there were a number of magnets and a number of coils of wire, the magnets moving relatively to the coils, or the coils relatively to the magnets, as you see in this rather old specimen of alternate-current dynamo. To produce magnetism by a large number of little magnets is not economical, and Pacinotti's device consisted in arranging a number of coils round a ring in the way shown in the large wooden model [model shown], so that they could all be acted on by one large magnet. Instead of frittering away his magnetism, Pacinotti showed how it could be concentrated, and thus he led the way to dynamos and motors becoming commercial machines. Pacinotti's science, engineered by Gramme, not only made electric lighting commercially possible, but led to electricity being used as a valuable motive power. It was in their work that the electric transmission of power in its modern sense sprang into existence.

Quite recently an improvement in the same direction has been introduced into alternate-current dynamos by Mr. W. N. Mordey, for he has replaced the many magnets of the ordinary alternate-current dynamos with one large magnet, and so with his alternator weighing 41 hundredweight, which you see in this hall, he has succeeded in obtaining at a speed of 650 revolutions per minute an output of 53·6 horse-power with a high efficiency.

It may be convenient to mention at this stage the very valuable work that has been done by the Drs. Hopkinson, Mr. Crompton, Mr. Kapp, and others, in the improving of dynamos and motors by applying scientific principles in the construction of these machines. Were I lecturing on dynamos and motors instead of on the electric transmission of power, I would explain to you how, by putting more iron into the rotating armature, as it is called, and less wire on it, by shortening the stationary magnet, and generally by concentrating the magnetic action, these constructors have raised the commercial efficiency of these machines to actually as high as between 93 and 94 per cent.; further, how, by recognizing the force of the general principles laid down by Prof. Perry and myself, as to the difference that should exist in the construction of a motor and a dynamo, Messrs. Immisch have succeeded in constructing strong, durable electromotors weighing not more than 62 pounds per effective horse-power developed.

The subject is so entrancing to me, the results commercially so important, that I am strongly tempted to branch off, but the inexorable clock warns me that I must concentrate my remarks as they have concentrated the magnetic action.

87½ per cent. of the power put into an Edison-Hopkinson dynamo has actually been given out by the motor spindle when 50 horse-power was being transmitted. How does this compare with the *combined* efficiencies of an air-pump and an air-motor, or of a water-pump and a water-motor? I understand that in either of these cases 60 per cent. is considered a very satisfactory result. As far, then, as the terminal losses are concerned, electric transmission of power is certainly superior to air or water transmission.

(*To be continued.*)

SCIENTIFIC SERIALS.

THE Proceedings of the American Academy of Arts and Sciences for the year May 1887-88 contains many important papers. Among them we may mention one on the relative values of the atomic weights of hydrogen and oxygen, by Prof. J. P. Cooke and Mr. Richards, and a catalogue of all recorded meteorites, by Prof. Huntington. The volume also contains papers on the existence of oxygen, carbon, and certain other elements in the sun; the first two of these papers are chiefly remarkable for the absence of reference to the literature of the subjects, and it is charitable to suppose that this proceeds from the authors' ignorance.

Bulletin de l'Académie Royale de Belgique, June 30.—On the physical aspect of Mars during the opposition of 1888, by L. Niesten. An image of the planet taken by the author on May 5 shows that the so-called continent was again visible, which M. Perrotin had reported as having disappeared during the opposition of 1886. Analogous though less marked modifications in the form and colour of the spots seem to imply that these changes are periodical. The paper is illustrated by two successful photographs of the planetary disk, showing its appearance on April 29 and May 5, 1888.—Fresh researches on the optic origin of the spectral rays in connection with the undulatory theory of light, by C. Fievez. A new interpretation of the spectral rays is here offered by the author, who regards spectral phenomena as a particular case of optical interferences. According to this view luminous rays would produce at a given point of the spectrum a vibratory movement, whose intensity might be maximum or minimum according as one of the rays follows another by an even or uneven number of half wave-lengths. A spectrum presenting dark or bright rays would always proceed, not from a luminous source, but from at least two different sources. It would thus indicate the nature of the rays, whose undulatory movement was disturbed by the simultaneous action of the various luminous sources. M. Fievez concludes that Kirchhoff's absorption theory does not alone suffice to explain the observed facts, which may also be interpreted by means of the undulatory theory of light. His views are supported by a number of ingenious and skilfully executed experiments in spectral analysis.

Rendiconti del Reale Istituto Lombardo, July.—Contribution to the study of unilateral hallucinations, by Prof. A. Raggi. Reference is made to two cases of what may be called "one-sided" hallucination, in one of which the left ear, in the other the left eye, was affected, the corresponding organs on the opposite side remaining perfectly sound. The complex character of the phenomena described, as well as their distinctly psychological nature, left no doubt that these were cases of true hallucination, although a subordinate influence in their production might possibly be attributed to the state of the organs themselves. On the other hand, mention was made of a somewhat doubtful case of double hallucination as connected with the same order of mental phenomena.

Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg, tome xxxii., No. 2.—On the regularity of the structure of continents, by A. Karpinsky (in German).—On a journey to the Karaites of the western provinces of Russia, by W. Radloff (in German). Those of Troki in Lithuania, Lutsk, and Kovno, are speaking a Turkish dialect with a considerable admixture of Polish, Lithuanian, and White-Russian words.—Supplementary notes with regard to the catalogue of stars published by the Pulkova Observatory, by O. Backlund.—Researches into the energy of chemical combination, by N. Beketoff (in French), being a continuation of former researches, now extended to potassium and lithium oxides.—On the polarization-photometer and its application to technical purposes, by H. Wild.—On the influence of iodoform and iodine on the isobutylate of natrium, by A. Gorboff and A. Kessler.—Notes on the new edition of the "Mi'jâr i Jamâlî," published at Kazan in 1887, by C. Salemann, with a plate showing the kinship of various Persian dialects (all in German).

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 10.—M. Des Cloizeaux in the chair.—Remark on a point in the theory of secular irregularities, by M. F. Tisserand. The reference is to Le Verrier's statement regarding the stability of the planetary system, in connection with a certain position between Jupiter and the sun, determined at about double the distance of the earth from the sun. An attempt is made to ascertain whether there exists an analogous position, in which the originally slight eccentricity of the orbit of a small mass might gradually assume proportions calculated to disturb the general equilibrium of the system.—The French vines, by M. A. Chatin. The treatment is described, by which a vineyard at Meyzieux, Isère, has been preserved, like a green oasis, in the midst of the wilderness created round about by the combined attacks of Phylloxera, mildew, and black rot. The treatment consists partly in a systematic process of nippings (*éborgnements*), partly in the application of a strong manure, including granulated phosphorus and products, with a base of nitrogen, potassa, and lime.—Degrees of oxidation in the fluorescent compounds of chromium and manganese (continued), by M. Lecoq de Boisbaudran. Several experiments are described tending to show that the pink compound is the real cause of the fluorescence.—Observations of Barnard's new comet, made at the Paris Observatory (equatorial of the West Tower), by M. G. Bigourdan. This comet, discovered on September 2 at the Lick Observatory, showed on September 5 a round nebulosity from 1' to 1'·5 in diameter, with somewhat stellar nucleus of 11·5–12 magnitude, not occupying the centre of the nebulosity.—Positions of Brooks's comet (August 7, 1888), measured at the Observatory of Besançon, by M. Gruey. The observations are for August 9–12, when the magnitude varied from 7 to 9.—On the planet Mars, by M. Perrotin. These remarks are made in connection with four new designs of Mars, forming a sequel to those published in the *Comptes rendus* of July 16. They still show the two canals—one simple, one double—running from the equatorial region nearly along the meridian towards the North Pole. A new canal is also revealed which presents the appearance of a straight dark band traversing the white Polar ice-cap.—On the chlorides of indium, by MM. L. F. Nilson and Otto Pettersson. To the previously-determined trichloride, $InCl_3$ the authors here add three distinct and stable chlorides. These are a trichloride, $InCl_3$, a dichloride, $InCl_2$, and a monochloride, InCl, showing that a metal of the third group in the natural system of the elements may act as a mono-, a di-, and a tri-valent in clearly-defined combinations.—On the part played by symbiosis in certain luminous marine animals, by M. Raphaël Dubois. In previous communications the authors showed that the fundamental reaction necessary to produce animal luminosity was of the same order as those effected under the action of the ferments. Their further studies of *Bacillus pholas* and *Bacterium pelagia*, the respective parasites of *Pholas dactylus* and *Pelagia noctiluca*, enable them to reconcile their theory of photogenous fermentation with the hypothesis of the oxidation of a phosphorated substance, as proposed by some biologists. These researches also help to explain how marine phosphorescence may be caused by the disintegration of marine animals, and how this phenomenon may cease or reappear, and assume various degrees of intensity, according to circumstances.—On the myelocytes of the Invertebrates, by M. Joannes Chatin. Hitherto spoken of as present in the organism of the Vertebrates alone, the author here shows that the myelocyte formation occurs also in the Invertebrates. He makes it evident that they cannot be assimilated to *free nuclei*, but represent true cellules normally constructed, with all their essential parts. He further points out that the intimate structure and real nature of the myelocytes may be studied much more conveniently in the lower than in the higher organisms.—On *Heterodera schachtii*, by M. Willot. In connection with his recent communication (*Comptes rendus*, August 3), on the destruction of this micro-organism by sea-water, the author points out that Dr. Strubell, of the University of Erlangen, has independently, but subsequently, made the same discovery.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Chambers's Encyclopædia, vol. ii., new edition (Chambers).—British Dogs, No. 23: H. Dalziel (Upcott Gill).—The Constants of Nature, Part 1, A Table of Specific Gravity for Solids and Liquids: F. W. Clarke (Washington).—Index to the Literature of the Spectroscope: A. Tuckerman (Washington).—Biologia Centrali-Americana: Insecta—Coleoptera, vol. i. Part 2: D. Sharp.—The Electrical Engineer, vol. i.—Examples and Examination Papers in Elementary Physics: W. Gallatly (Geo. Bell).—Massage and Allied Methods of Treatment, 2nd edition: H. Tibbits (Churchill).—British Mosses, 2 vols., new edition: F. E. Tripp (Geo. Bell).—Memorial of Asa Gray (Cambridge, Mass.).—Index to the Literature of Columbium, 1801 to 1887: F. W. Traphagen (Washington).—Annals of Botany, August (Frowde).

CONTENTS.

THURSDAY, SEPTEMBER 27, 1888.

THE FAUNA OF BRITISH INDIA.

The Fauna of British India, including Ceylon and Burma. "Mammalia." By W. T. Blanford, F.R.S. Part I. Published under the authority of the Secretary of State for India in Council. (London: Taylor and Francis, 1888.)

AMONG the various methods which may be adopted in the composition of zoological monographs, the two most prevalent are those in which either the natural group or the geographical region is taken as the basis. A particular section of the animal kingdom may be selected, and the structure, history, affinities, varieties, and distribution of its members worked out, or a particular region of the earth's surface may be taken, and the whole of its varied inhabitants described.

Monographs of groups and of fauna both have their value, and the success obtained in undertaking one or the other will depend much upon the special facilities of the investigator. From a strictly scientific point of view the former generally produce the best result. There is more cohesion, or naturalness, so to speak, in such a group, whether genus, family, or order; and anyone seriously endeavouring to trace the modifications of its members through all known forms, especially if the extinct can be united with the existing, has a better chance of getting a complete comprehension of the relations of all the parts of his subject than one who has to deal with the disjointed fragments of a large number of groups, brought by various circumstances together upon one part of the earth's surface—work, moreover, in many parts of which he must necessarily be largely dependent upon the labours of others.

On the other hand, for practical convenience, faunistic works are in greater demand than monographs on groups, especially if they treat of regions so important to the educated and civilized world as British India. We may even, in such a case, allow the weight of social and political rather than purely scientific boundaries in defining the range of the territory comprehended in the work. There is a very natural and laudable desire on the part of the large and continually increasing number of residents and travellers in our Indian Empire to obtain some definite knowledge of the varied and interesting forms of animal life by which they are surrounded, and it is gratifying to see that the Government of that great dependency has recognized its responsibility in this matter, and has given its authority to the preparation of a series of descriptive manuals on Indian zoology. The limits adopted for the fauna are those of the dependencies of India, with the addition of Ceylon, which, although British, is not under the Indian Government. Within the limits thus defined are comprised all India proper and the Himalayas, the Punjab, Sind, Baluchistan, all the Kashmir territories, with Gilgit, Ladák, &c., Nepal, Sikhim, Bhutan, and other Cis-Himalayan States, Assam, the countries between Assam and Burma, such as the Khási and Naga Hills and Manipur, the whole of Burma, with Karennee and Tenasserim, and the Mergui Archipelago, and, lastly, the Andaman and the Nicobar Islands. Afghanistan, Kashgaria, Tibet, Yunnan, Siam, and the Malay Peninsula south of Tenasserim are excluded. A few States, such as Nepal and Bhutan, at present not accessible to Europeans, are comprised, because it would be difficult to leave them out: scarcely an animal occurs in either not found also in British territories or in protected States such as Sikhim.

For the present it is proposed to restrict the publication to the Vertebrata, and to complete the work in seven volumes of about 500 pages each. One of these volumes will contain the Mammals, three will be required for the Birds, one for the Reptiles and Batrachians, and two for the Fishes. The authorship of the volumes on Fishes has been undertaken by Mr. F. Day, C.I.E.; the Reptilia and Batrachia will be described by Mr. G. A. Boulenger; whilst the Birds will, it is hoped, be taken in hand by Mr. E. W. Oates, author of the "Birds of British India." The editorship of the whole has been intrusted to Mr. W. T. Blanford, F.R.S., than whom few men could be found better qualified for such an undertaking. Long-continued employment in connection with the Geological Survey of India has made him familiar with the natural features of every part of the country; his qualifications as a field naturalist have been abundantly displayed in the published results of his scientific excursions to Persia and Abyssinia; and he has had recently, during several years' residence in London, ample opportunity of examining and comparing all that bears upon the subject, which is gathered together or recorded in our national collections and libraries at home.

Mr. Blanford has himself undertaken the volume describing the Mammals, and has now given us the first part as an instalment, consisting of 250 pages, and containing the orders Primates, Carnivora, and Insectivora. Notwithstanding the great advance that this work shows over that of Jerdon, published twenty-one years ago, especially in scientific method, critical discrimination of specific distinctions, and attention to the rules of nomenclature, in all of which it leaves nothing to be desired, it is still interesting to observe how much remains to be done, even in such a comparatively well-worn field as the Mammals of India, and how insufficient even our largest collections still are for perfecting such a work. For instance, the materials for a critical and exhaustive examination of the interesting genus of monkeys, *Semnopithecus*, are obviously wanting at present. Fourteen species of the genus are assigned by the author to British India, but doubts are expressed as to the real distinction of several of them, the characters of which are taken from an extremely limited number of examples, and it is stated that very little is known of their breeding habits and life-history in general. The variations, habits, and geographical distribution of the smaller *Felidæ* and *Viverridæ* offer an interesting field for future investigators, though Mr. Blanford has done much to clear away the confusion in which the synonymy of these groups had been involved by previous and less careful and conscientious workers. The account of the Insectivora has been derived largely from Mr. Dobson's excellent monograph of that order, the concluding still unpublished part of which, containing the *Soricidæ*, has been placed by the author at Mr. Blanford's disposal for the purpose.

The complete though condensed accounts of the habits of the animals described, whenever they are known on good authority, will make the work popular even with not strictly scientific readers; but all padding made up of ill-authenticated, fanciful, or exaggerated stories, or of personal narratives of sport and adventure, has been carefully excluded, as becomes the character of such a work as this is intended to be.

One of the most difficult questions that always arises in editing a work on natural history is that relating to the number and nature of the illustrations most suitable for its purpose. Figures are, without doubt, a great help to all classes of readers, and, other things being equal, the more numerous and better they are the more useful the book. But then comes in the question of cost, the bearings of which have carefully to be considered from a business point of view. A book that is intended to have a fairly extensive distribution must not be overweighted in this respect, or much of its utility will be lost. Mr. Blanford has evidently considered it best to sacrifice something of artistic effect and uniformity of character in his illustrations, for the sake of increasing their number and keeping the work within moderate compass as to price. With regard to the spirited little sketches of the external forms of animals, many of which are taken from the unpublished drawings of Colonel Tickell and Mr. Hodgson in the possession of the Zoological Society, the work of the Typographic Etching Company answers its purpose sufficiently well; but we cannot say the same of the figures of the skulls, which compare badly with woodcuts, of which a sufficient number (mostly, if not all, borrowed from other works) are introduced to make the contrast somewhat striking. These, however, are minor blemishes, which, we trust, are compensated by economy in production, and consequent advantage to the purchaser of the work; but the absence of scale to the figures, which is sometimes embarrassing, is an omission which might easily have been rectified.

The general form and typography of the work are all that can be desired, and we cordially welcome it as an instalment of what promises to be not only a most valuable aid to the knowledge of the natural history of one of the most important portions of our Empire, but also a standard contribution to zoological science in general.

W. H. F.

OUR BOOK SHELF.

Flora of the North-East of Ireland. By S. A. Stewart and the late T. H. Corry. Pp. 331. (Cambridge: Macmillan and Bowes, 1888).

LOCAL "Floras" have not been produced at the same rate in Ireland as in England, but Irish botanists are beginning to exercise more activity in this direction. It is true that there previously existed a catalogue of the plants of this region, together with localities of the rarer ones, in Dickie's "Flora of Ulster" (1864); and the twelfth district of Moore and More's "Contributions towards a Cybele Hibernica" (1866) is conterminous with the area of the book under consideration; but both of these works are incomplete, and imperfect in regard to what are termed "critical species."

The present book, we are informed in the preface, is an attempt to give a full and trustworthy account of the native vegetation of the counties of Down, Antrim, and Derry; an undertaking that was projected some years since by the late T. H. Corry, M.A., and the surviving editor. The lamentable and premature death of Mr. Corry by drowning, together with his friend and companion Mr. Dickson, in Lough Gill, on a botanizing excursion in 1883, will be remembered by most botanists. This sad event considerably retarded the appearance of the work, as Mr. Stewart's duties as Curator of the Belfast Museum left him little time for the task.

A brief history of botanical discovery, and the bibliography of what has been published, precede equally short paragraphs on the geography, geology, climate, &c., of the country. Then follows the enumeration, which includes 803 flowering plants and ferns, 293 mosses, and 73 liverworts. Babington's "Manual of British Botany," which contains 1524 vascular plants in the entire British flora, has been taken as the standard of the "Flora of the North-East of Ireland," though deviations in nomenclature have been made—in accordance with the rules of priority, Mr. Stewart explains.

The volume is a small and handy one, not overladen with localities, which is a distinct advantage over many similar works; but it has also certain defects, which, if pointed out, may possibly be remedied in a later edition. In the first place, there is no map of the country, a serious curtailment of its possible usefulness. Another defect, only the initial letter of the generic name is carried forward from page to page, though there is invariably ample space to repeat the name in full; therefore it is necessary to turn back to the beginning of the genus to ascertain what is intended. The same thing is noticeable in the index.

With regard to the purely literary part of the work, more particularly that relating to the priority and authorship of names, it would obviously have been better had the author adhered strictly to the last edition of Babington's "Manual" or the last edition of the "London Catalogue," for this part of the subject is just now in a transitional stage, and without a very complete botanical library it is impossible to do more than add to the existing confusion. We have no sympathy with those who adhere strictly to the "law of priority," because it entails endless changes of familiar names, and sacrifices convenience without any corresponding advantage. The fall of one genus often carries several others with it, and until the whole of the literature of binominal botany has been thoroughly examined there is no saying where the changes will stop. At the same time, if it is to be done, it should be done thoroughly, once for all.

Having turned up at random about half-a-dozen names concerning which there was some ambiguity, we found that the author was wrong in each instance. Thus, "*Nasturtium palustre* (Willd.), D.C.," should be *N. terrestre*, B. Br.; "*Lepidium Smithii* (Linn.), Hooker," = *L. heterophyllum*, Benth.; "*Hypericum tetrapterum*, Fries," = *H. quadratum*, Stokes; "*Lotus pilosus*, Beeke (*L. major*, Sm.)," = *L. uliginosus*, Schkuhr, and so on to the end. Whether the older names here cited are the oldest of all for the plants in question under the accepted genera is uncertain. Somebody some day may find names for some of these plants a week or two older, and then comes another change!

More interesting are some of the local names cited by Stewart, such as Tormenting Root (*Potentilla Tormentilla*), Mashcorns (*Potentilla Anserina*), Rose-noble (*Scrophularia nodosa*), and Well-ink (*Veronica Beccabunga*). Britten and Holland have all these names, or nearly the same. Thus, mascorns, and other variations, for the same plant in Scotland

W. B. H.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Electric Fishes.

WHILE I was fishing for cod the other day off Walmer, I took up in my hand a small whiting pout that was flopping about in the bottom of the boat, when I received what appeared to me a slight though distinct electric shock in the palm of my hand, which made me exclaim at once, "That fish has given me an electric shock." On asking the fisherman (seventy years of age) if he had known of such a thing occurring before, he said that he had "heard tell of it," and on inquiring further I found that he was referring to whiting pout and not to any other fish. He had never, however, noticed anything of the kind himself.

It will be interesting to know if any of your correspondents can confirm the observation. W. H. CORFIELD.

Savile Row, W., September 22.

Sonorous Sands.

THE communication of Mr. Cecil Carus-Wilson in NATURE of August 30 (p. 415), induces us to state that we are rapidly bringing to completion, and preparing for publication, an exhaustive study of "Sea, Lake, River, and Desert Sands" in their geological, physical, and chemical aspects. Our researches have extended over a period of six years, and are based on studies made in the field, in the laboratory, and with the microscope, and will be found to embrace many novel facts and original views. We have collected in person, by correspondence, and with the aid of the Life Saving Service of the United States, and of the Smithsonian Institution, several hundred specimens of sands and silts from localities in America, Europe, Africa, and Asia: these we have subjected to systematic examination and have tabulated the results.

The interesting phenomena of "musical sands," so called, have also been made special objects of our investigations, resulting in the discovery of many new localities, and of novel properties, as well as of the circumstances connected with the origin, production, and extinction of the sonorous qualities from which these sands receive their name. Furthermore, we have traced the history of musical sands through the literature of many centuries, and have brought together from widely scattered sources memoirs and notices of both scientific and popular interest. Throughout our work the bibliography of the subject has not been neglected, and we have availed ourselves of the photographic art for the purposes of illustration. We beg leave to make this preliminary announcement because our researches have been lengthened far beyond our expectations, and their publication (save in a few abstracts in the Proceedings of the American Association for the Advancement of Science) unavoidably delayed.

With regard to the occurrence of musical sand in Europe, the existence of which is unknown to Mr. Carus-Wilson, we may add that we have specimens from various localities, and the literature of the subject is accessible to everyone.

H. CARRINGTON BOLTON.
ALEXIS A. JULIEN.

London and New York, September 1.

YOUR correspondent in NATURE of the 30th ult. (p. 415), mentions a sea-beach in Dorsetshire as the only place in the Kingdom, besides the Island of Eigg, where "musical" sand is known to occur. This summer I found the sand in Lunan Bay (Forfarshire) to be distinctly sonorous. The sound occurred on moving the foot across the sand, or moving a walking-stick or the finger. The sound was little inferior to that in Eigg. The attention of a fisherman having been directed to the circumstance, he informed me they were quite aware of the occurrence, and that the sound was frequently much louder than on the day I was there; depending, I presume, on the state of the sand and of the atmosphere. He also mentioned that the sound occurs in the sand of Montrose Bay. I observed that the best result was got where the sand was moderately dry, and that little or no effect was produced with such a greater degree of moisture as gave a good result in Eigg. The form and composition of the sand-grains differ considerably in the two localities. It seems probable that sand of this character occurs in more localities than hitherto supposed. K.

Torquay, September 8.

THE LATE ARTHUR BUCHHEIM.

I HAVE been requested, and feel it a melancholy satisfaction, to notice in the columns of NATURE the premature decease on the 9th inst., at the age of twenty-nine, of Mr. Arthur Buchheim, for many years Mathematical Master at the Manchester Grammar School.

He was educated at the City of London School, whence he proceeded to Oxford, and gained an open Scholarship at New College there. He was a favourite pupil of the late Henry Smith, my distinguished predecessor in the Savilian Professorship of Geometry, who always spoke of him as the most promising young mathematician that had appeared in the University of Oxford for a long series of years. I am not able to speak of his earlier work as an original investigator, but know and value highly his contributions to the great subject which engaged the principal part of my own attention during the transition period between my residence in Baltimore and at Oxford, and to which I have given the name of Universal Algebra. He was a man of singular modesty and goodness of heart, which made him beloved by all who were brought into connection with him. Had his life been spared, I think we may safely say of him what Newton said of Horrocks, that "we should have known something" of what may now probably remain long unknown.

His life, it is to be feared, may have been shortened by his intense application to study, as after the arduous labour of the day he would sit up at night to study languages such as Sanskrit, Persian, Chinese, and Russian, almost any one of which was sufficient in itself to occupy his undivided attention.

After leaving Oxford he studied for some time under Prof. Klein at Leipzig. This episode in his life no doubt contributed to widening his intellectual horizon, but at the same time had the unfortunate effect of getting him out of the style of ordinary English University Examinations, in consequence of which he abstained, although strongly pressed by the authorities to do so, from offering himself as a candidate for a vacant Fellowship at the College of which he was a Scholar.

He comes of an intellectual stock, his father being the well-known Prof. C. A. Buchheim, of King's College, London.

Up to the last, after he had been obliged from ill health to resign his appointment at Manchester, he continued in harness, and made a communication to the London Mathematical Society at the monthly meeting in May or June last.

I have been furnished with a list of his published papers, fourteen in number, up to the year 1885 (exclusive), of which four appeared in the Proceedings of the London Mathematical Society, eight in the Cambridge *Messenger of Mathematics*, one in the *American Journal of Mathematics*, and one (November 1884) in the *Philosophical Magazine*. This last was entitled, "On Prof. Sylvester's Third Law of Motion," with which, I regret to say, I was previously unacquainted.

"The three laws of motion" of which it forms one were formulated by me in one of the Johns Hopkins Circulars, and it is a proof of the keenness of his research, that the subject of this notice (probably the only mathematician in Europe) should have made himself so well acquainted with them as to be able to write an independent paper on the subject. They have no direct connection (except in a Hegelian[1] sense) with mechanical principles, but are

[1] By which I mean that sense according to which motion in space is to be regarded as only a particular (visualized) instance of change *in actu*.

three cardinal principles in my Theory of Universal Algebra, between which and Newton's Three Laws of Motion I considered that I had succeeded in establishing a one-to-one correspondence. J. J. SYLVESTER.

Athenæum Club, September 22.

THE BRITISH ASSOCIATION.

SECTION H.

ANTHROPOLOGY.

OPENING ADDRESS BY LIEUTENANT-GENERAL PITT-RIVERS, D.C.L., F.R.S., F.G.S., F.S.A., PRESIDENT OF THE SECTION.

I.

HAVING been much occupied up to within the last week in my own special branch of anthropology, and in bringing out the second volume of my excavations in Dorsetshire, which I wished to have ready for those who are interested in the subject on the occasion of this meeting, I regret that I have been unable to prepare an address upon a general subject as I could have wished to do, and am compelled to limit my remarks to matters on which I have been recently engaged. Also, I wish to make a few observations on the means to be taken to promulgate anthropological knowledge and render it available for the education of the masses.

Taking the last-mentioned subject first, I will commence with anthropological museums, to which I have given attention for many years. In my judgment, an institution that is dedicated to the Muses should be something more than a store, it should have some backbone in it. It should be in itself a means of conveying knowledge, and not a mere repository of objects from which knowledge can be culled by those who know where to look for it. A national museum, created and maintained at the public expense, should be available for public instruction, and not solely a place of reference for *savants*.

I do not deny the necessity that exists for museum stores for the use of students, but I maintain that, side by side with such stores, there should in these days exist museums instructively arranged for the benefit of those who have no time to study, and for whom the practical results of anthropological and other, scientific investigations are quite as important as for *savants*.

The one great feature which it is desirable to emphasize in connection with the exhibition of archæological and ethnological specimens is evolution. To impress upon the mind the continuity and historical sequence of the arts of life, is, without doubt, one of the most important lessons to be inculcated. It is only of late years that the development of social institutions has at all entered into the design of educational histories. And the arts of life. so far as I am aware, have never formed part of any educational series. Yet as a study of evolution they are the most important of all, because in them the connecting links between the various phases of development can be better displayed.

The relative value of any subject for this purpose is not in proportion to the interest which attaches to the subject in the abstract. Laws, customs, and institutions may perhaps be regarded as of greater importance than the arts of life, but for anthropological purposes they are of less value, because in them, previously to the introduction of writing, the different phases of development, as soon as they are superseded by new ideas, are entirely lost and cannot be reproduced except in imagination. Whereas in the arts of life, in which ideas are embodied in material forms, the connecting links are in many cases preserved, and can be replaced in their proper sequence by means of antiquities.

For this reason the study of the arts of life ought always to precede the study of social evolution, in order that the student may learn to make allowance for missing links, and to avoid sophisms and the supposition of laws and tendencies which have no existence in reality.

To ascertain the true causes for all the phenomena of human life is the main object of anthropological research, and it is obvious that this is better done in those branches in which the continuity is best preserved.

In the study of natural history, existing animals are regarded as present phases in the development of species, and their value to the biological student depends, not so much on their being of the highest organism, as on the palæontological sequence by which their history is capable of being established. In the same way existing laws, institutions, and arts, wherever they are found in their respective stages of perfection, are to be regarded simply as existing strata in the development of human life, and their value from an anthropological point of view depends on the facilities they afford for studying their history.

If I am right in this view of the matter, it is evident that the arts of life are of paramount importance, because they admit of being arranged in cases by means of antiquities in the order in which they actually occurred, and by that means they serve to illustrate the development of other branches which cannot be so arranged, and the continuity of which is therefore not open to visual demonstration for the benefit of the unlearned.

It is now considerably over thirty years since I first began to pay attention to this subject. Having been employed in experimenting with new inventions in fire-arms, submitted to H.M. Government in 1852–53, I drew up in 1858 a paper which was published in the *United Service Journal*, showing the continuity observable in the various ideas submitted for adoption in the army at that time.

Later, in 1867–68–69, I published three papers, which, in order to adapt them to the institution at which they were read, I called "Lectures on Primitive Warfare," but which, in reality, were treatises on the development of primitive weapons, in which it was shown how the earliest weapons of savages arose from the selection of natural forms of sticks and stones, and were developed gradually into the forms in which they are now used. I also traced the development of the forms of implements of the Bronze Age and their transition into those of the Iron Age. These papers were followed by others on the same subject read at the Royal Institution and elsewhere, relating to the development of special branches, such as early modes of navigation, forms of ornament, primitive locks and keys, the distribution of the bow, and its development into what I termed the *composite* bow in Asia and America, and other subjects.

Meanwhile I had formed a museum, in which the objects to which the papers related were arranged in developmental order. This was exhibited by the Science and Art Department at Bethnal Green from 1874 to 1878, and at South Kensington from that date to 1885; and a catalogue *raisonné* was published by the Department, which went through two editions. After that, wishing to find a permanent home for it, where it would increase and multiply, I presented it to the University of Oxford, the University having granted £10,000 to build a museum to contain it. It is there known as the "Pitt-Rivers Collection," and is arranged in the same order as at South Kensington. Prof. Moseley has devoted much attention to the removal and rearrangement of it up to the time of his recent, but I trust only temporary, illness, which has been so great a loss to the University, and which has been felt by no one connected with it more than by myself, for whilst his great experience as a traveller and anthropologist enabled him to improve and add to it, he has at the same time always shown every disposition to do justice to the original collection. Since Prof. Moseley's illness it has been in the charge of Mr. H. Balfour, who, I am sure, will follow in the steps of his predecessor and former chief, and will do his best to enlarge and improve it. He has already added a new series in relation to the ornamentation of arrow stems, which has been published by the Anthropological Institute. It appears, however, desirable that the same system should be established in other places, and with that view I have for some time past been collecting the materials for a new museum, which, if I live long enough to complete it, I shall probably plant elsewhere.

Before presenting the collection to Oxford I had offered it to the Government, in the hope that it might form the nucleus of a large educational museum arranged upon the system of development which I had adopted. A very competent Committee was appointed to consider the offer, which recommended that it should be accepted, but the Government declined to do so; one of the reasons assigned being that some of the authorities of the British Museum thought it undesirable that two ethnographical museums should exist in London at the same time; this, however, entirely waives the question of the totally different objects that the two museums (at least that part of them which relates to ethnographical specimens) are intended to serve.

The British Museum, with its enormous treasures of art, is itself only in a molluscous and invertebrate condition of development. For the education of the masses it is of no use whatever.

It produces nothing but confusion in the minds of those who wander through its long galleries with but little knowledge of the periods to which the objects contained in them relate. The necessity of storing all that can be obtained, and all that is presented to them in the way of specimens, precludes the possibility of a scientific or an educational arrangement.

By the published returns of the Museum it appears that there has been a gradual falling off in the number of visitors since 1882, when the number was 767,873, to 1887, when it had declined to 501,256. This may be partly owing to the increased claims of bands and switchbacks upon public attention, but it cannot be owing to the removal of the Natural History Museum to South Kensington, as has been suggested, because the space formerly occupied by those collections at Bloomsbury has been since filled with objects of greater general interest, and the galleries have been considerably enlarged.

The Science and Art Department at South Kensington has done much for higher education, but for the education of the masses it is of no more use than the British Museum, for the same reason, that its collections are not arranged in sequence, and its galleries are not properly adapted for such an arrangement. Besides these establishments, annual exhibitions on a prodigious scale have been held in London for many years, at an enormous cost, but at the present time not the slightest trace of these remain, and I am not aware of any permanent good that has resulted from them. If one-tenth of the cost of these temporary exhibitions had been devoted to permanent collections, we should by this time have the finest industrial museum in the world. Throughout the whole series of these annual temporary collections, only one, viz. the American department of the Fisheries Exhibition, was arranged upon scientific principles, and that was arranged upon the plan adopted by the National Museum at Washington. It appears probable from the experience of the present year that these annual exhibitions are on the decline. Large iron buildings have been erected in different places, some of which would meet all the requirements of a permanent museum. The Olympia occupies 3½ acres, the Italian Exhibition as much as 7 acres. There can be little doubt, I think, that the long avenues of potted meats and other articles of commonplace merchandise, which now constitute the chief part of the objects exhibited in these places, must before long cease to be attractive, and must be replaced by something else, and in view of such a change I venture to put in a plea for a National Anthropological Museum upon a large scale, using the term in its broadest sense, arranged stratigraphically in concentric rings. It is a large proposal, no doubt, but one which, considering the number of years I have devoted to the subject, I hope I shall not be thought presumptuous in submitting for the consideration of the Anthropological Section of this Association.

The Palæolithic period being the earliest, would occupy the central ring, and having fewer varieties of form would require the smallest space. Next to it the Neolithic and Bronze Ages would be arranged in two concentric rings, and would contain, besides the relics of those periods, models of prehistoric monuments, bone caves, and other places interesting on account of the prehistoric finds that have been made in them. After that, in expanding order, would come Egyptian, Greek, Assyrian, and Roman antiquities, to be followed by objects of the Anglo-Saxon, Frankish, and Merovingian periods; these again in developmental outward expansion would be surrounded by mediæval antiquities, and the outer rings of all might then be devoted to showing the evolution of such modern arts as could be placed in continuity with those of antiquity.

In order that the best objects might be selected to represent the different periods and keep up the succession of forms which would constitute the chief object of the Museum, I would confine the exhibition chiefly to casts, reproductions, and models, the latter being, in my opinion, a means of representing primitive arts, which has not yet been sufficiently made use of, but which in my own small local museum at Farnham, Dorsetshire, I have employed to a considerable extent, having as many as twenty-three models, similar to those now exhibited, of places in which things have been found within an area of two miles.

The several sections and rings would be superintended by directors and assistants, whose function it would be to obtain reproductions and models of the objects best adapted to display the continuity of their several arts and periods; and the arts selected for representation should be those in which this continuity could be most persistently adhered to. Amongst these the following might be named: pottery, architecture, house furniture, modes of navigation, tools, weapons, weaving apparatus, painting, sculpture, modes of land transport and horse furniture, ornamentation, personal ornament, hunting and fishing apparatus, machinery, fortification, modes of burial, agriculture, ancient monuments' domestication of animals, toys, means of heating and of providing light, the use of food, narcotics, and so forth.

Miscellaneous collections calculated to confuse the several series, and having no bearing on development, should be avoided, but physical anthropology, relating to man as an animal, might find its place in the several sections.

I have purposely avoided in my brief sketch of this scheme giving unnecessary details. Any cut-and-dried plan would have to be greatly altered, according to the possibilities of the case, when the time for action arrived. My object is to ventilate the general idea of a large Anthropological Rotunda, which I have always thought would be the final outcome of the activity which has shown itself in this branch of science during the last few years, and which I have reason to believe is destined to come into being before long. In such an institution the position of each phase of art development shows itself at once by its distance from the centre of the space, and the collateral branches would be arranged to merge into each other according to their geographical positions.

The advantages of such an institution would be appreciated, not by anthropologists and archæologists only. It would adapt itself more especially to the limited time for study at the disposal of the working classes, for whose education it is unnecessary to say that at the present time we are all most deeply concerned. Although it is customary to speak of working men as uneducated, education is a relative term, and it is well to remember that in all that relates to the material arts they have, in the way of technical skill and handicraft, a better groundwork for appreciating what is put before them than the upper classes. That they are able to educate themselves by means of a well-arranged Museum, my own experience, even with the imperfect arrangements that have been at my command, enables me to testify. Anything which tends to impress the mind with the slow growth and stability of human institutions and industries, and their dependence upon antiquity, must, I think, contribute to check revolutionary ideas, and the tendency which now exists, and which is encouraged by some who should know better, to break drastically with the past, and must help to inculcate Conservative principles, which are urgently needed at the present time, if the civilization that we enjoy is to be maintained and to be permitted to develop itself.

The next subject to which I would draw your attention is the present working of the Act for the Preservation of Ancient Monuments, with the carrying out of which I have been intrusted during the last five years.

It is unnecessary to speak of the measures that have been taken in other countries which have preceded us in the work of protecting ancient monuments. Their system of land tenure and division of property is different from ours, and the same measures are not equally applicable.

In 1882 a Bill was passed through Parliament known as the Ancient Monuments Act, to enable those who desired to do so, to place the ancient monuments belonging to them under the protection of the Government, and to make it illegal for future owners or others to destroy them: also to enable local magistrates to punish summarily, with a fine of £5 or imprisonment for one month, offences committed under the Act. No power is taken to compel any owner to place his monument under the Act, but provision is made for a small annual expenditure in order to preserve the monuments offered voluntarily by their owners. A schedule of certain monuments was attached to the Act, without the consent of the owners, merely to indicate the monuments to which the Act applied, but these, like any others, had to be voluntarily offered before the Government could accept them. Any other monuments not in the schedule could be accepted, but only after the offer of them had been laid forty days before Parliament, in order, I presume, that the country might not become charged with the preservation of monuments that were unworthy of protection.

In November 1882, I was asked by Lord Stalbridge, in a complimentary letter, written by desire of the Prime Minister, to undertake the office of Inspector, intimating at the same time that my position as landowner would place me in a favourable position for dealing with other landowners to whom the monuments belonged, and I accepted the post, hoping to render a public service, not, perhaps, sufficiently considering the difficulties that I should have to encounter, and the amount of time that would have to be devoted to it.

A permissive Act naturally implies that there is some one in the country who desires to make use of it; whereas, as a fact, no owner has voluntarily offered any monument to be put under the Act, except one to whom I shall refer again presently: all have had to be sought out and asked to accept the Act, and of the owners of scheduled monuments the larger number have refused.

Sir John Lubbock was chiefly instrumental in passing the Bill through Parliament, although in the condition in which it actually passed it was not his Bill. He had proposed to make the Act compulsory in the case of some of the more important monuments, but the proposal had been overruled on the ground of its being an improper interference with private ownership.

Being a member of the Liberty and Property Defence League, I have lately received a list of fifty-five measures which have been brought before Parliament in the session of 1888, which that body have thought it desirable to oppose on account of their interference with private property, nearly every one of which would have dealt more hardly with the owners of property than the Ancient Monuments Act would have done had it been made compulsory. But all these measures have been proposed by members of Parliament with the view of catching the votes of particular constituencies, whereas the ancient monuments have no votes to give and very few people to vote for them. Sir John Lubbock, finding that the Act in its approved stage was purely permissive, and not believing, as he told me at the time, that anyone would voluntarily make use of it, naturally being unwilling to put his own property at a disadvantage, by being the only person to come under it, at first refused to include his own monuments, and it was only after I had obtained others, and success appeared probable, that he consented to put Silbury Hill under the Act.

Finding myself involved in the matter, I have done what I could to work it out, and with some success.

(To be continued.)

THE INTERNATIONAL GEOLOGICAL CONGRESS.

THE fourth session of the International Geological Congress began on Monday evening, September 17, in the theatre of the University of London, Burlington Gardens; meetings were held throughout the week, and the session was formally closed on Saturday, September 22. In another form and in different places the Congress may be regarded as continuing throughout this week, for five excursions have been organized to various parts of England; those to North Wales and the Isle of Wight are largely attended, whilst smaller numbers have gone to East Anglia, to East Yorkshire, and to West Yorkshire.

At the opening meeting on Monday evening the Council was chosen as follows:—Hon. President: T. H. Huxley. President: J. Prestwich. Past Presidents: G. Capellini, E. Beyrich. Vice-Presidents: Germany, K. von Zittel; Australia, *F. Liversidge; Austria, M. Neumayr; Belgium, G. Dewalque; Canada, T. Sterry Hunt; Denmark, *M. Johnstrup; Spain, J. Vilanova-y-Piera; United States, P. Frazer; France, A. de Lapparent; Great Britain, W. T. Blanford, A. Geikie, *T. McK. Hughes; Hungary, J. von Szabó; India, *H. B. Medlicott; Italy, F. Giordano; Norway, H. Reusch; Holland, K. Martin; Portugal, J. F. N. Delgado; Roumania, G. Stefanescu; Russia, A. Inostranzeff; Sweden, O. Torell; Switzerland, E. Renevier. General Secretaries: J. W. Hulke, W. Topley. Secretaries: C. Barrois, C. Fornasini, C. Le Neve Foster, C. Gottsche, A. Renard, G. H. Williams. Treasurer: F. W. Rudler. Other Members of the Council: T. G. Bonney, A. Briart, E. Cohen, *H. Credner, *E. Dupont, J. Evans, W. H. Flower, A. Gaudry, J. Gosselet, M. von Hantken, W. Hauchecorne, A. Heim, *J. Hooker, A. Issel, J. W. Judd, *R. Lepsius, C. Lory, *A. Michel-Lévy, T. Macfarlane, O. C. Marsh, E. von Mojsisovics, J. S. Newberry, S. Nikitin, *R. Owen, A. Pilar, F. von Richthofen, T. Schmidt, D. Stur, T. Tschernicheff, E. Van den Broeck, C. D. Walcott. (Those marked * were not present at the meeting.)

The President then delivered his address in French. An English translation of this has already appeared in NATURE. The meetings commenced each morning at 10.30, and lasted till about 1 o'clock. Meetings of the Council were held each morning at 9.30. The *procès-verbal* of each meeting both of Council and Congress was printed, and was placed in the hands of members at the opening of the succeeding meeting. At various times meetings of the International Commissions on Nomenclature, and the Geological Map of Europe, and of various Committees appointed by the Council, were also held.

In the afternoons there were visits to the British Museum, in Bloomsbury, and to the Natural History Museum, South Kensington; also to Kew, Windsor and Eton, Erith and Crayford. In the evenings there were three receptions: on Monday, by the President of the Congress, in the library of the University, fitted up as a temporary Geological Museum; on Wednesday, by the Director-General of the Geological Survey, in the Museum of Geology, Jermyn Street; on Friday, by the President of the Geological Society, in the rooms of that Society.

Three invitations for the fifth meeting of the Congress in 1891 were received from America—from Philadelphia, New York, and Washington. Philadelphia was chosen. A Committee of American geologists was appointed to take such steps as it thought necessary to make the arrangements for this meeting. The Committee consisted of Messrs. J. Hall, Dana, Newberry, Frazer, Gilbert, Hunt, Marsh, and Walcott.

The general opinion is that the Congress was a complete success. So far as members go, this is evidently the case, as it was more largely attended than any previous meeting, both by home and by foreign geologists. As regards the number of members inscribed from the country in which the Congress meets, it is not easy to make comparisons, because many join as members who have no claim to be considered geologists. No doubt this was more largely the case in London than at any previous meeting. But the numbers of foreign visitors may fairly be compared, and may be taken as affording a sufficient gauge of the relative importance of each meeting. These stand as follows: Paris (1878), 110; Bologna (1881), 75; Berlin (1885), 92; London (1888), 142.

The success of such a gathering may, however, be reckoned on other lines, and here opinions on the subject may differ. Those who hold that the first duty of such a Congress is to formulate rules and to fix nomenclature may well feel some disappointment; for although excellent discussions took place, and the general feeling was often evident, no formal vote on any such subject was taken. It was generally felt that votes from such mixed assemblages have no value. They can only carry weight when taken on some fixed principle, not dependent upon the accidents of place and nationalities which vary from time to time as the Congress meets in different countries. A Committee was formed to consider this matter. To its report, and to the general results of the Congress, we shall refer again next week. But upon one point there can be no difference of opinion: that is, the immense advantage resulting from the meeting together of men from different nations, engaged in similar pursuits, who can personally discuss subjects upon which they are at work. The friendships thus formed bear fruit long after the dicussions and votes of the formal meetings are forgotten.

ON CRYSTALLINE SCHISTS.[1]

I.

§ 1. AS a preliminary to the study of the schistose or stratiform crystalline rocks, it is desirable to consider the wider question of the origin of crystalline rocks in general, which are often named *Primary* or *Primitive Rocks* to distinguish them from those derived therefrom by mechanical or chemical disintegration. The designation of "crystalline rocks" is defective, inasmuch as we find, associated with masses having a right to this title, and geologically confounded with them, other rocks, such as serpentine, obsidian, perlite, and others, which are not crystalline in character, but colloidal, or, to use the designation of Breithaupt, porodic. The primary rocks, then, including both crystalline and porodic masses, may be divided geologically into three categories :—

(1) Masses more or less distinctly stratiform, including the fundamental granite, gneisses, micaceous and hornblendic schists, and all others formed, according to the views of the Wernerian school, by slow deposition in an aqueous liquid at the earth's surface. These we call INDIGENOUS ROCKS. (2) Masses which have strong mineralogic resemblances with the last, but appear to have been formed by slow deposition among pre-existing rocks, in which they occur as veins or secondary masses, and which we have consequently designated ENDOGENOUS ROCKS. (3) Masses which have resemblances, both mineralogic and geognostic, with the endogenous rocks, but are distinguished therefrom by the fact that they appear to have attained their present position not by slow deposition, but as the result of displacements which took place while they were in a more or less liquid or plastic state. These masses, which we designate EXOTIC ROCKS, are, as we shall endeavour to show, to be regarded (whatever their geological age) either as more or less modified portions of the original plutonic material of the globe, or as displaced portions of indigenous or endogenous rocks, and thus in either case belong to the primary class.

§ 2. Without taking into account those who, like Lehmann in the last century, maintained that the indigenous crystalline masses, which he called primitive rocks, were created as we now see them, we may say that the geologists of our own time are divided into two classes : those who admit for the indigenous rocks (1) an igneous or plutonic origin, (2) an aqueous or neptunian origin. Among the plutonists properly so called there are, moreover, two schools, one of which regards the foliated structure which characterizes the crystalline schists as due to the lamination of an igneous mass exposed to strong pressure during its extrusion through the already solidified terrestrial crust. For this school, in fact, the crystalline schists, not less than the granites, the trachytes, and the basalts, are eruptive rocks. This manner of explaining the origin of the crystalline schists, advanced by Poulett Scrope in 1825, and since frequently resuscitated, we have named the *exoplutonic* or volcanic hypothesis. For the other plutonist school, these same crystalline schists are the products of the consolidation, beneath a crust already formed by superficial cooling, of the igneous matter of the globe; the schistose structure being the result either of currents established in the still liquid and heterogenous material, or of a segregation therein during crystallization. To the views of this second plutonist school we have given the name of the *endoplutonic* hypothesis.

§ 3. The neptunists are also divided into several schools. Werner and his disciples believed that the crystalline rocks, both granitic and schistose, had been successively deposited from a universal ocean, which they imagined to have been a chaotic liquid holding in solution the elements of all the primitive rocks. We have called this derivation by slow crystallization from a primordial liquid chaos, the *chaotic* hypothesis. In this purely neptunian hypothesis, the action of a heated interior of the earth did not enter, but certain plutonists, admitting this notion, have imagined a *thermochaotic* hypothesis. This was advanced by Poulett Scrope, in 1825, as the complement of his exoplutonic hypothesis, and subsequently sustained by De la Beche and Daubrée.

Another neptunist school, which also held plutonic views, was that of Hutton, who supposed that the crystalline rocks now known to us have been formed by the consolidation and crystallization, through the agency of internal heat, of sediments arranged by water at the bottom of the seas, these sediments being the detritus either of endoplutonic or of exoplutonic rocks. The defect of this explanation, which we call the *metamorphic* hypothesis, is that it does not take into account the chemical changes suffered by most silicated mineral species during the process of disintegration of the crystalline rocks and their conversion into sands and clays. The production of species such as the feldspars, the micas, hornblende, &c., as the result of a recrystallization of sediments which do not contain the elements of these minerals, demands the additional supposition of chemical changes brought about either by substitution or by simple addition. In this manner, attempts have been made to explain supposed transformations, often very surprising, among which may be noted, not only the conversion of siliceous and argillaceous sediments into feldspathic and hornblendic rocks, but that of limestones into gneiss and other feldspathic and siliceous rocks, and also the conversion of these, as well as of diabases and diorites, into serpentine, or into crystalline limestone. This view, which we have called the *metasomatic* hypothesis, is, in the minds of many geologists, confounded with the metamorphic hypothesis of Hutton, of which it is, to a certain extent, the indispensable complement.

§ 4. Of all these hypotheses, that of Werner, which considered the primæval chaos as a watery liquid holding in solution the materials necessary for the formation of all the crystalline rocks, appears to us the one nearest the truth. It is certain, however, that in the present state of our chemical knowledge we cannot admit the simultaneous existence of all these materials in solution, even at the elevated temperature supposed by the thermochaotic hypothesis. We have, however, endeavoured to reconcile with known facts the view that a great part of all the primary rocks, including both the granites and the crystalline schists, have at one time been in the state of aqueous solution, through the action of processes which have operated without cessation from the Primary period. This explanation, which we have elsewhere set forth in detail, after a critical examination of the other hypotheses already mentioned, we have named the *crenitic* hypothesis, from the Greek κρήνη, fountain or spring.

Starting from the conception of a liquid globe of igneous origin, the solidification of which commenced at the centre, we find in its exterior portion—the last to solidify—the source of all the known terrestrial rocks; in other words, the veritable mineral protoplasm. This material we suppose to have been, from the time of its superficial cooling, exposed to the action of water and the atmospheric gases, while it was at the same time heated from below by the internal warmth, and penetrated to a greater or less depth by watery solutions. These, under the influence of the existing thermal differences, must have established a circulation between the surface and the deeper portions of the protoplasmic mass, which, as the result of crystallization and cooling, had already become porous. From the abundant outflow of thermal waters thus produced is derived the name "crenitic," given alike to the mineral deposits formed by them and to the present hypothesis. The action of these waters, removing from the protoplasmic material silica, alumina, and potash, and bringing to it at the same time lime, magnesia, and soda, must have necessarily altered by degrees the composition of this porous mass, heated from below, penetrated by aqueous solutions, and rendered more or less plastic in parts. In the changing mass, moreover, took place processes of crystallization, followed by partial separations determined by differences in specific gravity between the species thus formed. In this way were produced various types of plutonic rocks, which may justly be called Primary, since they are more or less modified portions of the original protoplasmic material.

§ 5. The dissolving action of the circulating waters continued without interruption from a very remote period in the history of the globe, and, extending eventually to depths equal to very many kilometres, while giving rise to the immense thickness of crenitic rocks which cover the surface of the protoplasmic mass, must necessarily have effected a great diminution therein. This decrease of volume beneath the crenitic covering must have resulted in movements giving rise to the more or less marked corrugations everywhere met with in the earlier layers of the crenitic envelope—movements which have continued, though with decreasing force, through all geological periods. Moreover, the accumulated weight, alike of crenitic deposits and of mechanical sediments, would bring about at length the displacement, in a plastic state, of portions of the primitive mass, as well

[1] Translated by the author from his essay on "Les Schistes Cristallins," presented to, and published in French by, the International Geological Congress in London, 1888. The footnote to § 5 has been added in translating

as of parts of the crenitic layers themselves, in the form of eruptive rocks, forming not only *plutonic* masses, but those which we have designated as *pseudoplutonic*—that is to say, masses of crenitic origin which present the geognostic characters of plutonic rocks. Such are apparently the trachytes and the truly eruptive granites. Eruptions of these two classes of rocks seem to have been rare in the more ancient periods, but in later times they have played an important part in the transfer of mineral matters from the depths to the surface of the globe, while at the same time the crenitic activity has progressively decreased. Without questioning the effect of the slow contraction through the secular cooling of the heated anhydrous and solid nucleus of the globe, we believe that the diminution of volume of its more superficial and hydrated portions by the crenitic process, as well as by plutonic eruptions, has played a very important part in geological dynamics.[1]

§ 6. According to the hypothesis just set forth, it follows that the production alike of the crenitic and the plutonic rocks, as the result of the transformations of a primitive material presumed to be of igneous origin, has been subjected to constant, regular, and definite laws. It shows, in fact, a mineralogical evolution which has determined the order, the composition, and the succession of the crenitic masses of the terrestrial crust, as well as the composition of the plutonic masses of the various geological periods. In the study of the successive groups of crenitic rocks we must take into account the intervention in the crenitic process alike of the soluble and the insoluble products of the aërial decomposition both of more ancient crenitic rocks and of plutonic masses, as well as the effects, both direct and indirect, of the products of organized beings. It results from the influence of all these secondary agencies which have intervened in the course of the crenitic process, that the fundamental granite, as the most ancient crenitic rock, presents characters of uniformity and of universality which do not reappear in the less ancient crenitic terranes. These, in fact, already begin to show indications of a passage to the new order of things, and were thus, in the language of the Wernerian school, called Transition rocks.

As a farther result of this mineralogical evolution in the history of the crenitic rocks, we find that certain aluminiferous silicates rarely met with at a given period, at length become more abundant and finally predominate. For this reason it follows that in the mineral kingdom, as in the organic kingdoms, generalizations which have for their object chronological classifications, should be founded upon the character of a group taken in its integrity, and not upon the characters of exceptional species. For the rest, it is to be remarked that non-aluminiferous species, such as the protoxyd-silicates, quartz, carbonate of lime, and oxides of iron are found, with small variations, in the crenitic masses, whether indigenous or endogenous, alike of earlier and of later periods.

It is evident that the operations of solution and of aqueous deposition, as well as those of decomposition and sub-aërial decay, went on in the Primary and Transition periods under geographical conditions which did not differ greatly from those of the Secondary and Tertiary periods. The marks of erosion, of contemporaneous movements, and of deposition in discordant stratification are met with at different horizons in the indigenous terranes of the Primary as well as in those of the Secondary ages; offering in both cases local and accidental interruptions of the normal order of mineralogical development.

§ 7. The various granitic, quartzose, and calcareous veinstones, including metalliferous lodes, not less than the veins and geodes of zeolitic minerals, are examples of endogenous masses formed by the crenitic process. The production of zeolites and of other silicates by the action of thermal waters, and the formation of zeolitic species in the deep-sea ooze, are examples of the same crenitic action continued to our own time. As is shown by the studies of the action of our modern thermal springs, the surrounding solid matters co-operate with those in solution in the production of new mineral species. We must not overlook the part which is often played by infiltrating waters in producing local transformations in sediments, thereby giving rise to the production of crystalline species in the midst of detrital rocks. Pressure alone appears in certain cases to produce similar results, all of which cases are often insisted upon in support of the application of the metamorphic and metasomatic hypotheses to the origin of the primary rocks.

[1] Besides the removal of all the silica and alumina found in the crenitic rocks must be added the diminution of porosity in the protoplasmic mass and the probable formation of more condensed species than those originally contained therein.

The granitic veins, composed essentially of orthoclase and quartz, which are found not only among gneisses and mica-schists, but among basic plutonic rocks alike of Palæozoic and of Mesozoic age,[1] help us to understand the conditions which in times of greater crenitic activity gave rise to the production of the gneisses and the fundamental granite, both of which, according to our hypothesis, are essentially neptunian and crenitic in their origin. These same indigenous and endogenous crenitic rocks have furnished the greater part of the materials for the Secondary rocks. We have already indicated concisely, in § 4, our explanation of the origin of the true plutonic rocks, as the result of modifications which have taken place in the midst of the protoplasmic mass.

§ 8. We must not lose sight of the important part played by water in plutonic and volcanic phenomena, nor the fact that it can exist under strong pressure, at high temperatures, in combination with silicated rocks. From this union there result hydrated compounds, which are more fusible than the anhydrous rocks, and which are decomposed in the transformations that take place during the cooling, with diminution of pressure, which accompanies the eruption of these materials. The water thus set at liberty may be disengaged in the form of vapour, and with it certain other volatile matters which are met with in volcanic emanations. In other cases, however, under a high pressure still maintained, and at a temperature above the critical point of vaporization, the water may be liberated in the state of a dense polymeric vapour, holding in solution, in accordance with late observations, mineral matters, which, through cooling, are at length deposited either from the vapour itself or from the liquid resulting from its condensation, in the form of crystalline species. Superheated aqueous vapours may thus play a part closely akin to that of thermal waters, and one which must be regarded as itself belonging to the crenitic process.

The greater part of the questions here noticed have already been discussed in detail by the author in his volume entitled "Mineral Physiology and Physiography" (Boston, 1886). especially in the three chapters on the Origin, the Genetic History, and the Decay of Crystalline Rocks (pp. 68–277.)

II.

§ 9. In another chapter of the volume just mentioned the author treats of the History of Pre-Cambrian Rocks (pp. 402–25), and endeavours to resume in a few pages the results of his attempts through a period of forty years to arrive at a subdivision and a nomenclature of these terranes, which comprise both the Primary and the Transition systems of Werner. It must suffice for the present to indicate in a succinct manner the conclusions already reached.

I. Laurentian.—Under this name, proposed and adopted by the author in 1854, is included the ancient gneissic terrane met with in the Laurentide and the Adirondack Mountains, as well as in parts of the great Atlantic belt, and in the Rocky Mountains in central North America. To this same series the author has also referred the similar gneisses of Great Britain and of Scandinavia, as well as the ancient or central gneiss of the Alps. Beginning with our first studies in Canada in 1847, we indicated the existence in this ancient gneissic system of two subdivisions, the lower being described as consisting of granitoid gneiss (to be confounded with the fundamental granite), to which succeeds (in discordant stratification) another gneissic series, also granitoid, and frequently hornblendic, with which are intercalated quartzites and crystalline limestones, often with serpentine. These two subdivisions, which we may provisionally call Lower Laurentian and Upper Laurentian, have been described respectively as the Ottawa gneiss and the Grenville series. To prevent any misconception, it should be noted that the name of Upper Laurentian was for a time given by Logan to the terrane subsequently designated Labradorian, and afterwards Norian. It is therefore by a mistake that some have wished to retain as the designation of the upper division of the Laurentian terrane, the name of Middle Laurentian.

[1] We have elsewhere described the granitic veins inclosed in the diabases which themselves traverse the Ordovician limestones of Montreal in Canada. These veins, having sometimes a thickness of three decimetres, are coarsely crystalline and drusy, and. besides quartz and orthoclase, contain, as accidental minerals, sodalite, nephelite, cancrinite, amphibole, acmite. biotite, and magnetite. Veins composed essentially of pink orthoclase and quartz, often accompanied by zeolitic minerals, are found in similar conditions inclosed in the diabases which are contemporaneous with the Mesozoic sandstones of Hoboken, near New York. In both cases the endogenous and crenitic origin of the granitic veins does not admit of any doubt. See for details the author's "Mineral Physiology and Physiography" (Boston, 1886), pp. 121–37.

II. NORIAN.—The terrane thus designated by the author in 1870 is composed in great part of those straliform rocks having a base of anorthic feldspars, to which has been given the name of norite. This terrane, however, includes intercalated strata of gneiss, of quartzite, and of crystalline limestone, all of which resemble closely those of the Upper Laurentian. These norite rocks, which are sometimes called gabbros, are not to be confounded with the very distinct gabbros of the Huronian terrane, nor yet with certain plutonic rocks having with them certain mineralogical resemblances. The facies of the norites serves to distinguish them.

III. ARVONIAN.—This terrane is composed in great part of petrosiliceous rocks, which pass into quartziferous porphyries. With them, however, are intercalated certain hornblendic rocks, sericitic schists, quartzites, oxides of iron, and, more rarely, crystalline limestone. This terrane, indicated for the first time as distinct by Dr. Henry Hicks, in Wales, in 1878, and named by him, is regarded by Mr. Charles Hitchcock as constituting in North America the lower portion of the Huronian.

IV. HURONIAN.—This name was given by the author in 1855 to a terrane already recognized in North America, where it rests in discordant stratification either upon the Laurentian gneiss or upon the Arvonian petrosilex. It includes, besides quartzose, epidotic, chloritic, and calcareous schists, masses of serpentine, and of lherzolite, together with euphotides, which represent herein the norites of the Norian terrane, often confounded with them under the common name of gabbro. This Huronian terrane is greatly developed in the Alps, where it constitutes the series of the greenstones or *pietre verdi*.

V. MONTALBAN.—The studies of von Hauer in the Eastern Alps, published in 1868, and those of Gerlach on the Western Alps, published in the year following, agree in recognizing in these regions two gneissic terranes—namely, an older or ancient central gneiss, and a younger or recent gneiss; this last, which is petrographically very distinct from the old gneiss, being accompanied by micaceous and hornblendic schists. The studies of Gastaldi, published in 1871, and those of Neri, in 1874, while confirming the results of von Hauer and of Gerlach, furnish us with further details respecting these terranes and their lithological characters. It should here be remarked that all of these observers appear to agree in placing the horizon of the *pietre verdi* (Huronian) between the ancient gneiss (Laurentian) and the recent gneiss.

Before becoming acquainted with the first results of these observers, the writer, from his own studies in North America, was led to precisely similar conclusions, and in 1870 announced the existence of a series of younger gneisses very distinct from the old Laurentian gneisses, and accompanied by crystalline limestones and by micaceous and hornblendic schists. To this younger terrane, on account of its great development in the White Mountains of New Hampshire, he gave in 1871 the name of Montalban. This series appears to be identical with the younger gneiss of the Alps; the so-called Hercynian gneisses and mica-schists of Bavaria; the granulites, with dichroite-gneiss, mica-schists, and lherzolite of the Erzgebirge in Saxony; and similar rocks in the Scottish Highlands. The Montalban terrane in North America contains not only crystalline limestones, but beds of lherzolite and of serpentine, resembling in this respect the Huronian and the Laurentian. It is in this series, in North America at least, that are found the chief part of the veins or endogenous masses of granite, which carry beryl, tourmaline, and the ores of tin, of uranium, of tantalum, and of niobium.

Gastaldi, in an essay published in 1874, declares that "the *pietre verdi* properly so called" is found between "the ancient porphyroid and fundamental gneiss" and "the recent gneiss, which latter is finer-grained and more quartzose than the other." This younger gneiss he also describes as a gneissic mica-schist, and as a very micaceous gneiss passing into mica-schist, and often hornblendic; the two gneissic series being, according to him, easily distinguished the one from the other. To these two divisions, superior to the ancient gneiss—that is to say, the true *pietre verdi* and the younger gneiss—Gastaldi adds a third division, still more recent. This highest division contains considerable masses of strata called by him argillaceous schists, and otherwise lustrous, talcose, micaceous, and sericitic schists. Associated with these are also found quartzites, statuary and cipolin marbles, with dolomite, karstenite, and sometimes hornblendic rocks and serpentines, the presence of which in this division, and also among the recent gneisses, as well as "in the *pietre verdi* proper," was regarded by Gastaldi as justifying the name of "the *pietre verdi* zone," often given by him to the whole of this tripie group of crystalline schists, which he recognized as younger than the central gneiss.[1]

VI. TACONIAN.—This third division, to which Gastaldi did not give a distinctive name, has, as is well known, a very interesting history in Italian geology. A terrane having the same horizon and the same mineralogical characters is found developed on a grand scale in North America, where it includes quartzites, often schistose, and sometimes flexible and elastic, with crystalline limestones yielding both statuary and cipolin marbles. It also contains deposits of magnetite and of hematite, as well as important masses of limonite, which is epigenic in some cases of pyrites, and in others of chalybite, two species which form, by themselves, large masses in the undecayed strata. This same terrane contains, moreover, roofing-slates, as well as lustrous unctuous schists, ordinarily holding damourite, sericite, or pyrophyllite, but including, occasionally, chlorite, steatite, and hornblendic rocks with serpentine and ophicalcite. We also find among these schists, which are met with at several horizons in the terrane, layers which are visibly feldspathic, with others of ill-defined character, which, however, are converted into kaolin by sub-aërial decay. These same schists furnish remarkable crystals of rutile, and also tourmaline, cyanite, staurolite, garnet, and pyroxene. This terrane, which, moreover, appears to be diamond-bearing, was described in 1859 by the late Oscar Lieber, under the name of the Itacolumitic group. Eaton already, in 1832, had placed the quartzites and the limestones, which form the lower members of this group, in the Primitive division. The argillites in the upper part of the group were regarded as the inferior member of his Transition division, and were, according to him, overlain unconformably by the fossiliferous graywacke (First Graywacke), made the upper member of this same Transition division. In 1842, Ebenezer Emmons included in what he then named the Taconic system the whole of this crystalline series, to which he added the graywacke; but in 1844 he separated this latter, in which he had meanwhile found a trilobitic fauna, and gave it the name of Upper Taconic; the inferior and crystalline portions being the Lower Taconic. Many years of study have shown me that this upper division is entirely independent of the Lower Taconic, with which the fossiliferous graywacke series is found in contact only in certain localities, while in many others it rests directly upon more ancient crystalline terranes. Seeing, morever, that the Lower Taconic is found without this graywacke, in a great number of localities, from the Gulf of St. Lawrence as far as Alabama to the south, and as far as Lake Superior to the west; and recognizing also the fact that the Upper Taconic is really a part of the Cambrian (as was avowed by Emmons himself in 1860), the author proposed in 1878 to limit the use of the term Taconic to the crystalline infra-Cambrian series which forms the Lower Taconic of Emmons and the Itacolumitic group of Lieber, and to call it the Taconian terrane.

The history of the various attempts made by the partisans of the metamorphic school to establish a more recent origin for the Taconian is a curious one. Various American geologists, adopting for the most part stratigraphical arguments, have successively referred it to the Cambrian, Ordovician, Silurian, Carboniferous, and Triassic horizons. It is, however, to be noted that these same geologists have also maintained the Palæozoic age of the greater part of the other crystalline terranes of North America, comprising the Montalban, the Huronian, the Arvonian, and a part of the Laurentian itself. The want of any conception of the principle of mineralogical development in the history of the crystalline schists, conjoined with the difficulties arising from the stratigraphical complications met with at many points along the eastern border of the great North American Palæozoic basin, has helped to confirm the belief of many American geologists in the hypotheses of the metamorphic and metasomatic schools.[2]

§ 10. The mineralogical resemblances which exist between the various crystalline terranes above mentioned are easily recognized.

[1] This question is discussed at length by the writer ("Mineral Physiology and Physiography," pp. 457–96) in a study of the geology of the Alps and the Apennines, and of the serpentines of Italy. See also his paper on "Gastaldi and Italian Geology," containing a hitherto unpublished letter from Gastaldi, in the *Geological Magazine* for December 1887.

[2] The reader who wishes to follow this question will find it discussed with much detail in the volume already cited "Mineral Physiology and Physiography" (pp. 517–686) under the title of "The Taconic Question in Geology." It is also treated, with some new facts, in the *American Naturalist* for February, March, and April, 1887, in an article entitled "The Taconic Question Restated."

The type of rocks characterized by orthoclase, appearing in the fundamental granite and the granitoid gneisses of the Laurentian, is again found in the quartziferous porphyries of the Arvonian, in the Montalban gneisses, and, though less distinctly, in the feldspathic rocks of the Taconian. The non-magnesian micas, rare in the fundamental granite and the Laurentian gneisses, appear abundantly in the Montalban gneisses and mica-schists, as well as in the lustrous schists which are found in the Huronian and the Taconian, and which predominate in the latter. It is further to be remarked that the simple silicates of alumina, such as andalusite, cyanite, fibrolite, and pyrophyllite, as yet unknown in the more ancient terranes, are abundant in the Montalban, and are also found in the Taconian. At the same time, crystalline limestones, oxides of iron, and calcareous and magnesian silicates, are met with in every terrane above the fundamental granite.

The chemical and mineralogical differences between these various terranes are more remarkable than the resemblances, a fact which, however, has not prevented some observers from confounding the younger with the older gneisses. Again, the resemblances between the Huronian and Taconian terranes led the late Prof. Kerr, in North Carolina to refer the latter terrane to the Huronian. Moreover, in the vicinity of the Lakes Superior and Huron, where we find alike Laurentian, Norian, Huronian, Montalban, and Taconian, the outcrops of this last were confounded with the Huronian by Murray and by other observers. In 1873, however, the author, distinguishing between the two, gave to the Taconian in this region the provisional name of the Animikie series. It was not until later that he recognized the fact that this series, which is here found in certain localities resting unconformably upon the Huronian, is no other than the Taconian. Emmons, on the contrary, who had long known the existence in this region of what he called the Lower Taconic, believed that the terrane to which the author gave, in 1855, the name of Huronian, was identical with this same Lower Taconic or Taconian. The differences between these two terranes in the basin of Lake Superior, first noted by Logan and later by the author, are clearly brought out by the recent studies of Rominger.

Upon all these different terranes, including the Taconian, there rests in discordant stratification in this region a vast series of sandstones and conglomerates, with contemporary basic plutonic rocks, the whole remarkable by the presence of metallic copper. This series, which had been alternately confounded with the Huronian and the Taconian on the one hand, and with the trilobitic sandstones of the Cambrian on the other, was for the first time separated by the author in 1873, under the name of the Keweenaw group, a term changed by him in 1876 to that of the Keweenian terrane. It still remains to be decided whether this series, upon which rest unconformably these same trilobitic sandstones, should form a part of the Cambrian, or should constitute a distinct terrane between the Taconian and the Cambrian.

§ 11. In submitting to his colleagues of the International Geological Congress this summary of his conclusions, based on over forty years of study, the author takes the liberty to state that the notions here advanced as to the origin, the chemical and mineralogical history, the subdivision, and the nomenclature of crystalline rocks, are for the most part the generalizations of a single observer. He now offers them as a first attempt at a classification of the indigenous rocks, and at the same time as an exposition of his crenitic hypothesis, and of the mineralogical evolution of the globe, which he conceives to have determined the succession and the chemical nature of the masses which he has named crenitic, as well as those of plutonic masses. He feels at the same time that his work is far from complete, and that to others must now be left the task of correcting and finishing it.

As a large part of these results, so far as regards geognostic classification, appeared for the first time in the Reports of the Geological Survey of Canada, the author may be permitted to say, in closing, that the first publications made by that Geological Survey on the crystalline rocks of Canada—that is to say, the reports of progress for the years 1845 and 1846, were prepared by him, and published in 1847, from the notes and the collections made by Logan and by Murray in the two years previous. Moreover, all the statements relating to the mineralogy, the lithology, or the chemical composition of the rocks of Canada, which are found in the official reports from 1847 to 1872, when the author resigned his position as a member of the Geological Survey of Canada, were written by him or under his personal direction.

T. STERRY HUNT.

SOME QUESTIONS CONNECTED WITH THE PROBLEM PRESENTED BY THE CRYSTALLINE SCHISTS, TOGETHER WITH CONTRIBUTIONS TO THEIR SOLUTION FROM THE PALÆOZOIC FORMATIONS.[1]

THE question of the "crystalline schists" still presents so many unsolved difficulties, and the views of contemporaneous fellow-workers diverge herein so widely, that an attempt at unanimous agreement on the points at issue must at present be regarded as premature. This assuredly does not prevent our taking counsel together, interchanging observations, and endeavouring to gain solid ground, whence a future solution can be aimed at. Each geologist will approach such a consultation in a way differing in accordance with his own experience.

I can only contribute experience gained by the study of the metamorphic crystalline schists, belonging to the *Palæozoic* formations, that have been proved to have resulted from the action of contact or dynamic metamorphism on eruptive or stratified rocks, the latter including the tuffs. The *direct* application of this experience to *all* Archæan crystalline schists appears to me premature—*i.e.* rather a *thema probandum* than *probatum*. Doubtless there are cases—as, for instance, in the so-called flasergabbros or zobtenites, which, apparently, must be regarded as quite analogous to the alteration of the diabases in the Palæozoic formations. Indeed, the same essential features which Lehmann has described in the development of the Saxon "flasergabbros" have been demonstrated by Teall in the Lizard gabbros, G. H. Williams in the Baltimore gabbros, and Hans H. Reusch in Norway. But Hans H. Reusch also mentions *bedded gabbros*[2] as well as eruptive flasergabbros, differing thus from Lehmann; while Credner and Roth appear by no means willing to concede all that is contained in Lehmann's book. This fundamental difference must, however, be noticed: Lehmann holds the Archæan schists half for metamorphosed sediments, half for interbedded or injected eruptive rocks; and although I cannot agree with or follow Lehmann in every detail (and, above all, lay more stress upon the altered tuffs), still on the whole I can but support him in this view. Roth, on the other hand, holds all the Archæan crystalline schists—limestones, quartzite, gneiss, mica-schist, amphibolite, &c.—for *schistose, plutonic* (only in form not eruptive) rocks (*Erstarrungskruste*); finally, Credner holds the majority of the crystalline schists, including granite-gneiss and flaser-gabbro, for the *normal stratified* sediments of a primæval ocean, their crystalline nature being essentially not due to metamorphism.

I have dwelt thus at length on this point in order to demonstrate that there exist numerous controversies even on those questions that admit of solution by reason of the *most undoubted pseudomorphic changes* (hornblende after diallage, hypersthene, augite; zoisite, epidote, actinolite, quartz, albite after lime-soda feldspar), and by reason of the presence of the *original eruptive structure*.

My stand-point is identical with that expressed by Carl Friedrich Naumann in the following words: *My task above all else is to study the metamorphism, with respect both to substance and to structure, of the fossiliferous sediments and the eruptive rocks, together with the tuffs intercalated therein.* Much has already been done, especially with respect to contact-metamorphism, which is more sharply defined than regional or dynamic metamorphism. There remains, however, much to answer, especially as the primary structures of original schistose eruptive rocks and the structure and substance of certain very common sedimentary rocks (as, for instance, the greywackes, the so-called greywacke-schists, or the majority of the tuffs) are still too little known to afford a firm basis for the study of metamorphic processes.

Still the detailed solution of the following question would be of no little value for the study of the Archæan schists:—

(1) What material agreement or difference exists between the

[1] "Einige Fragen zur Lösung des Problems der krystallinischen Schiefer, nebst Beiträgen zu deren Beantwortung aus dem Paläozoicum," von Prof. Dr. K. A. Lossen. "Etudes sur les Schistes cristallins," 1888. Published by the International Geological Congress in London, 1888. (Translated from the German by Dr. F. H. Hatch.)

[2] Giving a somewhat wide meaning to the word "gabbro"; he now says, "dioritic rock," "altered gabbro and diabase." In the Hartz the interesting gabbro-district of Hartzburg presents, among numerous other varieties, some which show layers alternately richer in plagioclase and diallage (bronzite) or present *flaser*-structure with biotite, and possess thus a *bedded-like* but not a true *bedded* parallel structure. These rocks are true eruptive gabbros.

results of metamorphism due to the contact of granite with fossiliferous sediments and the eruptive rocks intercalated therein, on the one hand, and the Archæan schists on the other?

For such a comparison useful data are furnished by the *Hartz*. These mountains, consisting of fossiliferous sediments and the most diversified eruptive rocks, already plicated at the Coal-measure period, represent a fairly average section of the earth's crust, *i.e.* although there is no axis of crystalline schists, the strata, together with diabases, keratophyres, and the accompanying tuffs, are considerably depressed between highly elevated plutonic rocks (granite, gabbro, &c.).

The *contact-zones around the gabbro and granite* present the following authigenic *minerals*: quartz, orthoclase, albite, plagioclase, biotite, muscovite, hornblende, actinolite, augite, bronzite, chlorite, epidote, garnet, vesuvian, tourmaline, axinite, wollastonite, cordierite, sphene, spinel, andalusite, rutile, magnetite, hematite, titaniferous iron ore, magnetic pyrites (pyrrothine), and other sulphur ores, calcite, fluorite, apatite; and continued investigations will easily add others to the list, as, for instance, anatase, zoisite, lithionite, lepidolite, corundum, sillimanite, cyanite, graphite—indeed, the four last-mentioned minerals have already been detected in certain mineral aggregations in post-granitic dykes of the Hartz, that probably are to be referred to metamorphic influence. But not only *do these minerals show great resemblance to those which are most frequently present in Archæan crystalline schists; their combination to definite mineral aggregates and rocks also makes the analogy even more complete.* In the *normal gneisses*, which are derived, with great diversity of structure, from the *culm-greywackes and the greywacke schists of the Oberhartz*, in contact with granite and gabbro, are intercalated *cordierite- and garnet-gneisses and augite- (or bronzite-) bearing gneisses*, which are produced by the alteration of schistose and calcareous sediments. *Saccharoidal quartzites* are clearly produced by the recrystallization of *Carboniferous* or *Devonian lydites* (*Kieselschiefer*); and it is very difficult to distinguish these from rocks produced by the contact-metamorphism of nearly pure quartz-sandstone (*Quarzitsandsteine*). *Hornstones* (*cornéenne*), which contain garnet, amphibole, augite (or bronzite), schorl, andalusite, apatite, as well as orthoclase and plagioclase, are found replacing *mica-schists* and phyllites. The thin *limestone-seams in the Lower Devonian* (*Hercynian*), Upper Devonian, and the Culm-measures, are partly metamorphosed to compact or phanero-crystalline "*lime-silicate-hornstones*," containing garnet or other allied silicates—vesuvian, epidote, malacolite, cordierite, amphibole, sphene, &c., in places also fluorite or axinite, and corresponding to the garnet-rocks, epidote-rocks, pyroxenites, ecklogites, &c., of the Archæan formation.

In part, however, they have undergone *marmorosis*, while being impregnated with garnet or other silicates and locally with ores; even anthraconite is not altogether absent from these marbles. Amphibolites are in part also derived from *calcareous sediments*; those, however, that contain felspar (plagioclase) in any essential quantity can be demonstrated to result from the *contact-metamorphism of pre-granitic, Devonian, and Carboniferous diabases that have been plicated and metamorphosed in common with the strata.* Further, there are, in the granite and gabbro contact-zones, *alteration-products of the diabase* that are *rich in biotite*; and other pre-granitic eruptive masses, such as the *augite-keratophyres* and the *augite-orthophyres*, show a great abundance of biotite, which is associated with a recrystallization of the orthoclase and of a part of the augite. This biotite is certainly developed at the expense of chlorite derived from augite or primary hornblende.

Schistose rocks with more abundant biotite, that are locally present among the more dominant massive rocks, bear the strongest resemblance to garnetiferous *mica-schists*. In the *porphyroids of the Hartz*, which occur both within and without the contact-zones, we mainly find *sericitic muscovite*; beyond the contact-zone it occurs in such abundance as to produce very schistose sericite rocks, which, on the other hand, are here also derived directly from the porphyritic massive rocks. These porphyroids I regard, from my present stand-point, as the metamorphosed pre-granitic tuffs of quartz-keratophyres and quartz-porphyries. To these tuffs are perhaps related certain *hornstones, very rich in orthoclase*, which occur in the granite contact-zone with Devonian and Carboniferous siliceous schists (equivalents of Adinole?).

Other questions are:—

(2) What differences exist in the *order of crystallization* of the minerals which compose granites, quartz-diorites, gabbros, diabases, in short holo- and phanero-crystalline eruptive rocks, and that of the secondary minerals produced in the contact-metamorphism of these eruptive rocks?

This question must be the more carefully answered, as, in spite of the rich material so excellently collected and cleverly arranged for the use of science by H. Rosenbusch, the order of crystallization of the eruptive rocks is not yet firmly established. A certain degree of regularity is undeniable; but, on the one hand, the chemical law is, as Lagorio has demonstrated, more intricate than that formulated by Rosenbusch; and on the other, the order varies quite unaccountably with alterations in the physical conditions of consolidation (compare granite and pegmatite).

(3) Is the ophitic (diabase-) structure under all circumstances the structure of an eruptive rock, or are there undoubted sedimentary rocks possessing a similar structure?

(4) It has been proved that graphic granite, as micro- and macro-pegmatite, forms an integral part of true eruptive rocks, especially of granite and its porphyritic modification. Since graphic granite is very common among the gneisses, the question arises whether it is to be regarded as a true eruptive rock, or whether such occurrences can be proved to have been produced by thermal action, or even lateral secretion, in the sense of a partial solution of the neighbouring rocks.

Even if it be admitted that all minerals can be produced, by a suitable variation of the conditions, either by consolidation, by separation from aqueous solutions, or by sublimation, still it does not follow, to my mind, that all the structures that combine minerals to regular aggregates, can be produced in like manner in these three modes of formation. It seems to me that such structures—as, for instance, the ophitic (diabasic) or the pegmatitic (to say nothing of the structures which are developed in rocks containing glass or other base)—that have been demonstrated to be *characteristic of rocks of undoubted eruptive origin, must rather be regarded as indicating an origin by consolidation from a magmatic condition, so long as contrary proofs are not forthcoming.* No one, to my knowledge, has ever maintained that the ophitic or diabasic structure can be of sedimentary origin; but gabbros have been claimed—wrongly, as I believe—as sediments, in spite of the close relation of their structure to that of the diabases.

As regards *graphic granite* (or *macro-pegmatite*), the case is somewhat different.

The frequent occurrence of such masses in gneiss has created the notion that they are integral components of the *sedimentary* gneisses. And this view is maintained, although a considerable portion of these pegmatitic masses can be clearly seen filling vein-like cavities, while another part make up lenticular patches that follow, more or less, the dip and strike of the schists. The occurrence of simple aggregates of quartz and feldspar, that are of *thermal* origin, must, then, in accordance with one's experience of regional and contact-metamorphism, be unconditionally conceded; while the absence of such aggregates in the greywackes appears to me to absolutely disprove a development by lateral secretions. It is therefore not inconceivable that the pegmatitic aggregates represent, so to speak, the quintessence of the gneiss, exuded into primary cracks. At the same time, great caution is to be recommended; for, since the introduction of the microscope, *micropegmatite* has, little by little, been recognized as an essential constituent of numerous acid and basic (with SiO_2 per cent. as low as 48) rocks. The veins of graphic granite in the Hartzburg gabbro have been held by some for segregation-veins. They are, however, demonstrably apophyses of the eruptive granite; indeed, the principal mass of granite in the Brocken *massif* is, in the main, micropegmatitic. The *banded structure, with bilateral symmetry*, of many pegmatites, which has been compared to that of many mineral veins, is no proof of their non-eruptive nature. The augites, felspars and other minerals of lavas present banded structures with variable chemical composition: banded structure with a chemical composition varying from that of diabase to granite-porphyry, is shown by compound eruptive dykes, as has lately been well shown by Bücking, in the Thüringerwald ("Jahrb. d. kgl. preuss. Geol. Landesanst. f. 1887," p. 110, *et seq.*). Even the *drusy character and the richness in minerals presented by the central portion of many pegmatite-dykes* finds its analogy in the external shells of true eruptive granites, which may, however, be complicated by the influence of thermal actions, accompanying, or subsequent to, eruption. *Giant spherulites*, of a decimetre diameter, *composed of macropegmatite, enveloping a porphyritic Carlsbad twin of potash-feldspar* (orthoclase or microcline), that occur in the granite of the Riesengebirge, repeat, on the large scale, the microscopic characters of the micropegmatite of certain quartz- and granite porphyries (the granophyre of Rosenbusch). *All these phenomena compel the assumption that at least a part of the pegmatites are of indubitably eruptive origin*, and arouse in us the question whether this structure is not to be brought into connection with the origin of the gneisses.

(5) What are the differences between the *primary structures* (*due to consolidation*) of the plutonic and volcanic rocks and the *structures* of (*a*) the crystalline sediments, (*b*) the metamorphic rocks in contact with granite, (*c*) the crystalline schists?

(6) What *reliable* characters have we, to distinguish crystalline grains developed *in situ* from clastic grains, in cases where they occur, side by side, in one and the same rock?

The answer to this question has already frequently been attempted, among others in the most praiseworthy manner by A. Wichmann. It requires, however, a fresh solution based on the latest experiences. The safest test of the authigenic, non-clastic nature of a grain is doubtless the presence in it of enclosures of minerals that are also present in the rock as authigenic constituents. External form and internal molecular relations, in consequence of pressure-phenomena, can, however, be very misleading. Hard minerals, especially, occur in clastic sand in very sharp crystals (quartz, tourmaline, zircon, &c.).

(7) Are the views of those authors justifiable, who conceive certain gneisses or porphyroid crystalline schists to have been produced by the injection of a granitic magma, *in discontinuo*, between the schists (*Schiefer*)?

(8) If the views expressed in the preceding question are justifiable, how are the gneisses and porphyroids, produced by the addition of granite *in discontinuo* to slaty sediments, to be distinguished (*a*) from true eruptive granite or its porphyritic modification, both having, under the influence of pressure, undergone a "phyllitic" modification; (*b*) from slaty sediments in which aggregates or crystals of silicates have been deposited from water (quartz and feldspar)?

(9) What differences can be established in mineral composition and structure between a true eruptive granite and an indubitably stratified (not simply jointed or cleaved) so-called "*Lagergranit*" or granite-gneiss?

An *amalgamation of eruptive granite* with *the mineral aggregates of the rocks in contact* has, according to my experience taken place in some cases; but I have not yet observed an undoubted discontinuity in such granitic material. It is much to be desired that the French geologists (for instance, Michel-Lévy and Charles Barrois), who defend the views formulated in Questions 7 and 8, would enlighten us by good drawings of macro- or microscopic sections, as to how far in this difficult question *an incontestable separation of injected eruptive granite from metamorphic gneiss* is possible. This would, without doubt, facilitate the solution of Question 9. Unanimity on this point will scarcely be obtained without a careful structural diagnosis, which, of course, must be supported by serviceable material, self-collected in the field.

(10) Are there any *absolute* material and structural differences between metamorphic rocks of the granite contact-zone (hornstones, cornéenne, &c., cp. Question 1) and rocks affected by regional or dynamic (*Dislocations*-) metamorphism? or are such differences only *relative*, and what are they?

The exact solution of this question requires, above all, the assumption that only such occurrences shall be submitted to consideration that are unmistakably connected with *visible* eruptive rocks. It should also not be forgotten that rocks which have originally undergone contact-metamorphism have, in some cases, *subsequently* lost their peculiar characteristics in consequence of the influence of regional metamorphism. With this qualification I am personally inclined to concede only a *relative* and not absolute differences. I am guided in this, not only by my experience in the Hartz, which has made me acquainted with the remarkable variation of the metamorphic rocks in contact with granite, according as they occur just outside the contact-zone or in its outer, middle, or inner division; or again according as they belong to the unpenetrated but eroded mantle of the eruptive cores, or to masses, of greater or smaller extent, that have sunk deep in between the eruptive masses and have been covered up by them. The rocks occurring thus differently vary between a phyllitic clay-slate and gneiss, while the main mass of the slate- and grauwacke-hornstones present little resemblance to the crystalline schists. In the classic region of the Erzgebirge, however, there occur, according to the careful investigation of our Saxon colleagues, *compact hornstone-like or even conglomeratic greywacke gneisses* (the mica-trap of older writers) that present this analogy in a complete degree. The same analogy is presented by Gosselet's Lower Devonian "*cornéite*" (to be distinguished from *cornéenne*, the product of contact-metamorphism) from the regionally metamorphic Ardennes of Belgium. Again, *the Lower Devonian fossiliferous sediments* of the Ardennes, *containing garnets, hornblende, and graphite*, that are so well known through A. Renard's admirable descriptions and drawings, remind one of hornstone, although no contact with eruptive rocks has been observed affecting either them or the *Cambrian garnetiferous "Wetzschiefer"* of Vielsalme. The association of such hornstone-like rocks with those of the usual phyllitic type of regional metamorphism recalls the occurrence of lime-silicate-hornstones in the outermost zone (beyond the zone of the "Knotenschiefer" around the granite of the Rammberg. Whatever explanation of these phenomena may be given—Gosselet is decidedly in favour of dynamic metamorphism as opposed to a latent contact-metamorphism—at least this is evident, that important contributions to the question, here formulated, can be furnished by the Ardennes.

ON THE CLASSIFICATION OF THE CRYSTALLINE SCHISTS.[1]

THE most important constituent of the earth's crust—the crystalline schists—has remained, with respect to their field-relations and their origin, the most shrouded in darkness. The difficulties that bar the way are quite exceptional. We have frequently to deal with rocks that have undergone subsequent alteration, without being able to determine their original constitution, and without being able to explain the nature of the change. We have, as it were, to deal with an equation with two unknowns—we cannot solve it.

At the present time we meet with a number of attempts to classify the crystalline schists, mainly according to petrological characters, in stratigraphical groups. I regard these attempts as premature, for this reason: microscopists are unfortunately very behindhand in the exact investigation of the crystalline schists, and of the half-clastic, half-crystalline sediments. The purpose of these lines is to direct attention to another difficulty which has not yet received sufficient consideration, but which bars the way to every attempt of that kind—namely, *the mechanical metamorphism during mountain-formation.*

That, by the plication of the Alps, the constitution of the rocks has been completely changed, is most directly proved by an examination of the sedimentary rocks; because the latter can be also studied in an unaltered condition in adjacent localities. The commonest changes met with here in connection with folding are:—

Deformation of fossils, pebbles, or crystals (compression in one direction, extension in another)

Cleavage (*Transversalschieferung*).

Cleavage with linear extension.

Puckering.

Internal formation of breccias and cementing of the same by secretions.

Internal formation of innumerable slickensides, so as to change the whole structure.

Scaly structure, produced by the compression of oolitic structure.

Alteration of hematite and limonite into magnetite, in connection with cleavage.

Marmorosis of the limestones.

Formation of confusedly "kneaded" structures (*Knetstructuren*).

Development of new minerals (garnet, staurolite, mica) in places that have undergone crushing.

Now, sedimentary rocks, metamorphosed in the above way, are frequently found in extremely narrow synclinal zones, nipped in between rocks belonging to the crystalline schists. The

[1] "Zur Klassification der krystallinischen Schiefer," von Prof. Dr. Albert Heim. "Études sur les Schistes Cristallins." Published by the International Geological Congress in London, 1888. (Translated from the German by Dr. F. H. Hatch.)

Alpine zones, which consist mainly of crystalline schists, are termed *central massifs*. Such intercalations of mechanically metamorphosed sediments with the crystalline schists are very frequently to be observed at the ends of the strike of the *central massifs*, and between the *central massifs*; they are not rare even in the interior of the *central massifs*. The crystalline schists and metamorphosed sediments not only present the same stratigraphical position, but also similar characters in other respects. The cleavage of the sedimentary rocks may be continued in the same direction into the crystalline schists; and similar contortions may traverse both: in the latter, as in the former, a marked linear extension in the same or but slightly deviating direction may be present: calcareous patches in the crystalline schists are crystalline and granular, and contain layers of mica-scales which have undergone extension, precisely as in the neighbouring Jurassic limestones, &c., &c. From these facts we see that in these crystalline schists we have not to deal with rocks of original constitution, but that both these rocks and the sediments have undergone similar mechanical metamorphism. The only difficulty in dealing with the schists is contained in the fact that we are never in a position to describe the original appearance of the rock before it underwent the mechanical metamorphism.

Now it is in the crystalline schists that the plications of the earth's crust are most potently developed. The isoclinal and fan-shaped folds, the wedging and "kneading together" at the contact with the sediments—in short, all these high forms of dislocation, which are the earliest to modify the inner structure of rocks, are to be found in the crystalline zones of the Alps. They are most highly developed in the northern series of the *central massifs* (Mont Blanc, Aiguille Range, Finsteraar-*massif*, Gotthard-*massif*, Silvretta-*massif*, &c.).

At first sight it appears as if the crystalline schists and the true sediments, in the Alps, were separated by a constant unconformity; but frequently even recent sediments are found folded in, parallel with the crystalline schists. Again the sediments often take the position of a *central massif*; indeed, it seems as if a great part of several of the *central massifs* consisted of Palæozoic sediments. On the other hand, in the southern *central massifs* of the Central Alps, we see the crystalline schists lying in all respects like the sediments.

Those who have worked in these parts of the Alps will have remarked how often the mechanical crushing undergone by the rocks obliterates the limits of stratigraphical and petrographical characters, and how many rocks have become confused thereby in their development (*Ausbildungsweise*). Such changes can sometimes be directly proved to be the result of local crushing; sometimes, however, they are regional, and then passages into the unaltered rock are difficult to trace. All degrees of change by earth-movements are to be found, from a slight alteration of the structure up to complete metamorphism. In hundreds of places one does not know whether one has to deal with the residual traces of original bedding or with a cleavage (*Transversalschieferung*, *Quetschungsschieferung*) that has completely obliterated the original structures. In many cases it is impossible to distinguish between a schistose structure (*Schieferung*), superinduced by earth-movements, and one that is original. Schistose structures which cross one another are by no means rare. Whether the more pronounced or the less definite one is then the original is often not to be decided. Even an exact microscopical examination will often not suffice to distinguish between structures resulting from crushing and lateral deformation, and the fluxion-structure of an eruptive rock. It is certain that a structural modification by earth-movements has everywhere taken place where linear extension abounds. The latter is never original. In such crystalline schists with linear-parallel structure there are often elongated, ragged mica-scales. The linear extension can go as far as the development of rod-like separation (*stenglige Absonderung*).

Are there any rocks left in the *central massifs* of the Alps which have undergone no change in structure during the orogenetic processes?

The metamorphism can penetrate still deeper.

Enormous zones, for instance, in the interior of the Finsteraar-*massif*, that were formerly held to be true crystalline schists, prove to be originally clastic rocks of the Carboniferous period that have been squeezed into schists, and pervaded by secondary mica. Conglomeratic rocks of the Verrucano group, and clay-slates, nipped into the *central massif*, have become crystalline, schistose, and even gneissose. They can scarcely be distinguished, in the field and in the hand-specimen, from crushed gneisses pervaded by sericite. Granites can be proved, locally and perhaps also regionally, to have been compressed into gneisses. Gneisses, having a different position relatively to the pressure, have locally become granitoid. Massive eruptive felsite-porphyries have become felsite-schists. Mica-schists have been dragged out; their quartz grains ground down; and the whole converted into a rock that one would be inclined to describe as a sandy clay-slate. Even Liassic slates with fossils have been converted into garnetiferous mica-schists, staurolite-schists, &c. The boundary between the old crystalline schists and real sediments in the Alps has, by such processes of dynamic metamorphism, been obliterated, and the proper character of the rock so altered as to render recognition impossible. When we see, in true sediments, new minerals developed by the progress of the mechanical metamorphism (magnetite in the crushed Oolitic ironstone of the Windgälle, garnet in the Belemnite-slates of Scopi), the question arises, for the crystalline schists of this and neighbouring regions—Which minerals are original, and which have been produced subsequently, by orogenetic processes?

We arrive at this conclusion:—*The constitution of the crystalline schists in the Alps has been much changed by the orogenetic process* (dynamic metamorphism). *Original material and material mechanically produced at a later period, are often not to be separated from one another.*

Besides these, the Alps present other difficulties that stand in the way of the recognition of a stratigraphical grouping of the crystalline schists. The field-relations are frequently so intricate, that often it is very difficult to decide what originally lay under and what above; and whether the enormous thickness, for instance, of many gneiss-complexes, is real, or merely produced by repetitions of the folding, the folds being concealed by cleavage.

It follows that, if, on the basis of petrographical relations, a general stratigraphy of the crystalline schists is to be attempted, *this must never take place as the result of observations made in plicated regions of the earth's crust: districts must rather be chosen which are not influenced by disturbances of the Alpine character.* In the question of the stratigraphy of true crystalline schists, the Alpine geologist is not in the position to furnish material of essential value; he must rather wait for the results of the workers in other regions, in order to be able to apply them to his own district. The dislocations of fractured regions have, in the main, left unaltered the constitution of the rocks. There, then, the crystalline schists can be studied in their unaltered condition. There also they lie in flatter and more regular bedding; and a stratigraphical sequence is sooner to be found than in the Alps.

ON THE ORIGIN OF THE PRIMITIVE CRYSTALLINE ROCKS.[1]

IN this paper the author briefly summarizes the ideas prevailing on the origin of the crystalline schists, and throws a doubt on the current opinion that the primitive rocks have been formed by the *direct* crystallization of their constituents. He divides his treatise into two parts: (1) stratigraphical considerations; (2) the mode of association of the component minerals.

(1) *Stratigraphical Considerations.*—The primitive crystalline rocks form the fundamental floor upon which lie the earlier detrital deposits, their schistosity being often parallel to the stratification of the latter.

Although composed mainly of acid gneisses, the primitive rocks present countless variations in chemical and mineralogical composition; they include very basic representatives, such as the amphibolites, pyroxenites, peridotites, cipolines, and dolomites, &c. These intercalations are always parallel to the schistosity: they form elongated lenticular patches, of which the greater axis is in the direction of the general banding.

At the same time, their relative homogeneity in composition is shown by comparison of sequences established, not only in Europe, but also in the United States and the rest of the world. Acid gneisses predominate at the base; then come frequent intercalations of mica-schists and leptynites, with which are

[1] "Sur l'Origine des Terrains Cristallins Primitifs," by M. A. Michel-Lévy, Bull. Soc. Géol. France. 3e série, t. xvi. p. 102, 1888. Published by the International Geological Congress in London, 1888. (Abstracted from the French by Dr. F. H. Hatch.)

associated amphibolites and cipolines. Above this first division chloritic and sericitic mica-schists are developed, alternating occasionally with amphibolitic layers. This second stage is succeeded by a series which also comprises hornblendic and augitic (*cornes vertes*) schists, but includes, further, the first detrital deposits. At every horizon there is a gradual passage from the one stage to the other. The first detrital deposits alternate with sericitic and chloritic schists; and even as far up as in the Cambrian, large bands of felspathic schists, which can scarcely be distinguished from the more ancient gneisses, are developed in connection with the intrusion of granite.

The primitive rocks are, as first pointed out by the author, injected and penetrated by ancient eruptive rocks. This phenomenon is al o to be observed in the earlier detrital schists.

Rolled pebbles and fragments of gneiss, mica-schist, &c., have been repeatedly found in the granitic and granulitic gneisses of various localities. The author's own observations lead him to compare these phenomena with those in which rounded balls have been inclosed in a truly eruptive granite. In numerous cases, in which fragments of gneisses have been enclosed in other gneisses, he has always been able to prove that the enclosing rock is much more felspathic than the inclosed fragments.

These facts cannot, therefore, be advanced in support of the detrital origin of true gneisses.

(2) *Mode of Association of the Component Minerals.*—The mineralogical composition of the gneisses and of the schistose basic rocks associated with them, is nearly identical with that of the granular eruptive rocks; and all the types of the older eruptive rocks have their representatives in the schistose series.

A great analogy therefore exists between the natural forces instrumental in the production of the two series.

Speaking generally, the older eruptive rocks are rigorously homogeneous over vast areas: fragments of these rocks are everywhere comparable to one another. This homogeneity is reproduced in the schistose series; but it is, so to speak, periodic, and one must first know the orientation before comparing fragments taken from a distance.

The structure of the gneisses presents a series of successive crystallizations, accompanied by mechanical phenomena and a cementing of the dislocated components. The author, while seeing in these phenomena the traces of a series of metamorphic actions, followed by the injection of foreign material, does not wish to deny the additional intervention of secondary mechanical actions. But, whatever theoretic explanation be adopted, the facts are well established, and irreconcilable with the assumption of a preliminary mixing of the magma of the schistose rocks, and therefore with the hypothesis of a primordial origin.

The author then proceeds to demonstrate at some length that the intimate structure of the gneisses is identical with that of sedimentary schists modified by contact metamorphism, and finally injected by eruptive rocks.

Microscopic studies have disclosed the minute liquid inclusions contained by the quartz of the gneisses. Zirkel and Kalkowsky have made the interesting observation that the streams of inclusions are restricted to the central portions of the quartz-grains and are not prolonged to the periphery; and De Lapparent adduces this fact as a proof that the grains have not been derived from a pre-existing rock. But this argument is overthrown by the fact that the quartz-grains in the Cambrian micaceous schists, which are of indisputably detrital origin, present exactly the same phenomenon. It admits, moreover, of a very simple explanation. These quartz-grains, of clastic origin, have undergone subsequent enlargement by the assimilation of secondary quartz, which tends also to give them an exterior crystalline form. This secondary quartz is poor in liquid inclusions, and encloses scales of black mica and other minerals.

General Considerations and Hypotheses on the Origin of the Primitive Rocks.—Among the hypotheses advanced to explain the origin of gneiss, the author discusses the two that have found the most general acceptance. The first, which is now somewhat abandoned but has the merit of perfect clearness, makes the gneisses the result of a kind of conflict between water and the primary molten magma of the earth. The other explanation, which is more vague, accords to the gneisses a sedimentary origin. They are the deposits of a kind of supersaturated sea, which precipitated on to its floor the successive crystalline bands which characterize the gneisses. Note that this hypothesis presupposes a floor—an unknown substratum.

(1) Geologists originally supposed that the first substratum was formed by the granites which are found cropping out over such vast areas. Detailed studies have shown, however, that the granites are younger than the gneisses which they traverse, inject, and displace. Even the most ancient among them are at least younger than the first detrital schists.

It is therefore to the gneisses, distinctly banded and alternating in their lower beds with mica-schists, that this mixed origin—this *rôle d'écumes primordiales*—must be attributed.

Has this substratum of the terrestrial crust ever been seen in the most disturbed regions?

Cordier supposed that terrestrial refrigeration was constantly increasing, in the downward direction, the thickness of the first solid crust. If we could descend through the earth's crust, we should pass successively through rocks of increasing basicity until we should find, enveloping the still incandescent nucleus of impure iron, a rock analogous to lherzolite.

A serious objection to this is the fact that a descending order of basicity is not borne out by the stratigraphical relations of the gneisses. Lherzolite is found erupted through the primitive rocks; and the basic peridotites are intercalated moderately high up in the gneissic series.

From the purely speculative point of view it is improbable that the first products of consolidation did not receive a thorough mixing, rendering the rock homogeneous, and preventing the formation of those numerous micaceous membranes so characteristic of the primitive rocks. If these first products were acid, as there is reason to suppose, the first substratum must have constituted a massive and homogeneous granite. It is on a floor of this kind that the precipitation of the atmospheric waters must have prepared the elements of the first detrital rocks—the first arkoses.

(2) The second explanation—the successive crystallization of bands of gneiss from the waters of a universal sea—encounters similar difficulties. It appears to the author irreconcilable with the structure of the gneissic rocks. The continuous membranes of mica, and the almost vein-like appearance of the quartz and felspar, do not accord with the notion of concretionary deposits that this hypothesis requires, supposing the supersaturated liquid to have been in a state of perfect tranquillity. If, on the other hand, we suppose that there existed local agitations due to the unequal distribution of high temperatures, the remarkable periodic homogeneity of the gneisses becomes inexplicable.

From a consideration of these facts and hypotheses, the author arrives at the conclusion that the veritable and primary substratum of the terrestrial crust is not visible; that this substratum has undergone much alteration; finally, that the so-called primitive rocks are a complex of eruptive rocks, later than the gneisses, and of rocks which are really detrital, but which have undergone excessive metamorphism.

The eruptive rocks, by which the primitive rocks have been injected, are later than the beginning of the Cambrian. They were produced in extraordinary abundance in the later portion of this period: granites, diabases, diorites, norites, and lherzolites.

In discussing the primary causes of the eruption of these rocks, the author mentions that Lehmann and others of the German school, are inclined to seek them in the partial transmutation into heat of the mechanical work performed during the intense periods of contortion undergone by the earth's crust. The author himself refers them to manifestations of the internal heat of the globe, the great earth-movements having simply effected the ascension and injection of the eruptive magmas.

NOTES.

By the death of Mr. Jameson on the Upper Congo, science has lost a most promising young naturalist. The collections made by him some years ago in Borneo were never described, but we believe that in that island Mr. Jameson met with many species of birds since obtained by other travellers. His expedition to Mashoona Land resulted in the discovery of some interesting new species of birds, and an elaborate paper was written on his collection by Captain Shelley in the *Ibis* for 1882. A small number of birds has also been sent by him from the Aruwimi River to his friend Mr. Bowdler Sharpe, who has been waiting for further collections before writing an account of them. We do not know whether any further consignments ar on their way from the district where Mr. Jameson was stationed for many months with the late Major Barttelot. He described the country as a disappointing locality for the collector, the few birds obtained by him being merely the ordinary Congo species.

WE regret also to have to record the death of Mr. T. H. Potts, a well-known New Zealand ornithologist. Mr. Potts's name has been connected with the natural history of New Zealand for a number of years, and his observations on the nesting and life-history of the birds of his native country are among the most interesting contributions to the Transactions of the New Zealand Institute.

WE have received a communication from Herr Gamél, of Copenhagen, the equipper of the Norwegian Expedition to Greenland, in which he informs us that if the undertaking has been successfully accomplished the members of the Expedition should be on board the sailing-ship *Peru*, which was to leave Disco Bay on September 16, and is due in Copenhagen in the middle of October. If not on board this vessel, the Expedition will have to remain in Greenland until next spring, as this is the last ship leaving, and no news will be obtainable from Greenland till then.

WE learn from the *Scotsman* that the fishery cruiser H.M.S. *Jackal* lately left Granton on a scientific expedition, which will include a cruise of several weeks in the North Sea and a visit to the Baltic. The chief object in view is to collect data likely to throw more light on various questions which, when solved, will admit of a better understanding of the movements of the edible fishes and of the myriads of minute organisms on which they feed. The Expedition is under the direction of Dr. John Gibson, of the University of Edinburgh Chemical Laboratory, who is accompanied by Dr. Hunter Stewart and Mr. Maitland Gibson, also from the University of Edinburgh.

MANY students of science will regret to learn that the *Naturforscher* has ceased to appear. The last number is dated September 23.

PREPARATIONS have been made for effecting the proposed connection between the Observatory of Paris and Greenwich. It is expected that this will lead in the end to the acceptance of the Greenwich meridian by French astronomers.

THE General Omnibus Company in Paris has introduced into its service the electricity supplied by the Electric Storage Company. The carriages run from the Arc de Triomphe to Courbevoie, a distance of about two miles. Each of the two fore-wheels is put into rotation by a separate dynamo, over which the driver exerts control. The velocity is somewhat greater than that obtained with horses.

THREE new sulpho-chlorides of mercury have been isolated by Drs. Poleck and Goercki, of Breslau. Every student of chemical analysis is familiar with the peculiar changes of colour which occur when a solution of mercuric chloride is precipitated by sulphuretted hydrogen gas; how that the precipitate at first is perfectly white, shortly passes to a yellow, and then rapidly darkens, becoming orange, brownish-red, and finally, when excess of the gas has been led through the solution, perfectly black. The white compound first formed was shown so long ago as 1828 by Rose to consist of a sulpho-chloride of the composition $2HgS . HgCl_2$; but the further changes appear never to have been hitherto thoroughly investigated. The Breslau chemists, after fully confirming the composition of the white substance, now show that the darkening is due to the formation of successive higher sulpho-chlorides, $3HgS . HgCl_2$, $4HgS . HgCl_2$, $5HgS . HgCl_2$; the final product being, of course, the sulphide of mercury, HgS, itself. This has long been supposed to be the case, and it is very satisfactory to have these various sulphochlorides at last isolated. It may readily be seen, however, that by simply passing the current of sulphuretted hydrogen until the precipitate became of any particular tint, one would never be able to isolate these higher compounds, the mixture becoming more complicated every minute. The method adopted, after many fruitless attempts, consisted in completely precipitating in various experiments quantities of mercuric chloride corresponding to three, four, and five molecules respectively; the precipitates were in each case transferred to a flask fitted with inverted condenser, and digested for some time with a fresh quantity of the chloride corresponding to another molecule. The first product, $3HgS . HgCl_2$, possessed a brownish colour, and the two higher ones more and more nearly approximated to the black of the pure sulphide of mercury. In each case the filtrate was found to be free from quicksilver and chlorine, proving that the extra molecule of the chloride had in each case combined, and analysis showed that the precipitates really possessed the compositions above indicated. These sulpho-chlorides, moreover, are very stable; they are almost perfectly insoluble in water, and may be digested with water in sealed tubes at 200° C. without undergoing any change. They are also insoluble in both hydrochloric and nitric acids, but dissolve in the mixture of the two known as aqua regia. They were finally shown to be distinct chemical compounds, and no mere mechanical mixtures of sulphide and chloride, by the peculiar action of potassium iodide upon them. It may therefore be considered that the question of the action of sulphuretted hydrogen upon mercuric chloride has now been definitely settled.

PROF. H. A. HAZEN, of the Signal Service, Washington, has compiled a "Hand-book of Meteorological Tables," containing in a convenient form all the reductions needed for current work, omitting those not now generally used, such as Reaumur temperatures, &c. Several of the tables are new, or re-computed in their present form after some years' experience of the author in their use. The table for reduction of barometrical observations to sea-level has been extended to 8000 feet. Among the useful additions we may mention formulæ and tables for the determination of mean wind direction, and for the conversion of wind velocities from miles per hour to metres per second, and *vice versâ*. The latest determination of the metre is used in all linear tables.

ON the night of September 5 a brilliant meteor was seen at Bolmen, in Småland, in Sweden. It first went in a straight line from east to west, when it suddenly altered its course, falling to the earth with a dull report. Its colour was bluish-white.

SNOW and frost are reported from several parts of Sweden, whilst flocks of birds have been seen migrating southwards.

THE preservation of the eider on the south coast of Sweden has had the most beneficial results, considerable flocks of these birds being now often seen.

TWO runic stones have been discovered at Sorunda, in Sweden.

THE Swedish Consul at Eskefjord, in Iceland, writing at the end of August, states that although the fjord was free from ice there were still large masses of drift-ice along the east and north coasts, which were practically unapproachable for vessels. There was also much drift-ice in Denmark Sound. The cod and herring fisheries had been good.

A NORWEGIAN naturalist, Herr L. Ucherman, draws attention to the peculiarly green waters of certain rivers in Norway, emanating from those snow-fields which never melt, and describes the colour as due to certain green Algæ on old snow. In support of this he mentions that, when walking across old snow in the highest parts of Norway this summer, he noticed that footprints a sumed a greenish hue, which was not the case with new snow. It has generally been assumed that the snow Algæ, so well known in higher latitudes, did not as a rule flourish on snow in Norway.

THE Society for Promoting Christian Knowledge will publish shortly a "Star Atlas," containing maps of all stars from 1 to 6·5 mag. between the North Pole and 34° south declination, and of all nebulæ and star clusters in the same region which are visible in telescopes of moderate powers. The explanatory text, by Dr. Hermann J. Klein, has been translated and adapted for English readers by Mr. Edmund McClure.

MESSRS. CROSBY LOCKWOOD AND SON will publish during the ensuing season the following works bearing on science:—"The Metallurgy of Gold," a practical treatise on the metallurgical treatment of gold-bearing ores, including the processes of concentration and chlorination, and the assaying and refining of gold, by M. Eissler, formerly Assistant Assayer of the United States Mint, San Francisco; with 90 illustrations. "Practical Surveying," a text-book for students preparing for examinations or the colonies, by George W. Usill, A.M.I.C.E.; with upwards of 330 illustrations. "Tables, Memoranda, and Calculated Results for Farmers, Agricultural Students, Graziers, Surveyors, Land Agents, Auctioneers, &c.," with a new system of farm book-keeping, selected and arranged by Sidney Francis; waistcoat pocket size. Also the following new volumes in Lockwood's series of "Handy-books for Handicrafts":—"The Model Engineer's Handy-book," a practical manual, embracing information on the tools, materials, appliances, and processes employed in constructing model steam-engines, by P. N. Hasluck; with about one hundred illustrations and working drawings (in the press). "The Clock Jobber's Handy-book," a practical manual, embracing information on the tools, materials, appliances, and processes employed in cleaning, adjusting, and repairing clocks, by P. N. Hasluck; with about one hundred illustrations. "The Cabinet Worker's Handy-book," a practical manual embracing information on the tools, materials, appliances, and processes employed in cabinet work, by P. N. Hasluck; with about one hundred illustrations.

IN an interesting paper presenting a concise history of the acclimatization of the Salmonidæ in Tasmania, Mr. P. S. Seager claims that success has been secured in the thorough and unquestioned establishment of salmon trout and brown trout, both of which species are now abundant in Tasmania. The establishment of the true salmon is still to some extent a matter of uncertainty. "It must, however, be borne in mind," says Mr. Seager, "that more than one specimen submitted for scientific examination to Dr. Günther and others have been pronounced *S. salar*, and that Sir Thomas Brady has publicly stated his belief that specimens shown to him are of the same species. In speaking of them commercially, Sir Thomas states that such specimens in a salmon-producing country would be accepted as salmon without a doubt." This being so, Mr. Seager is of opinion that the establishment of *S. salar* in Tasmania may almost be regarded as an accomplished fact.

ADVICES from the Philippine Islands, *viâ* Hong Kong and Yokohama, received at Queenstown from New York on Saturday morning last, state that over 300 lives were lost in those islands by the eruption of an old volcano, named Mayon, at the latter end of July. Several hundreds of houses were also destroyed by the lava and ashes, and the natives were in a state of panic. Volcanoes in the islands of the Bissayar group were also in a violent state of eruption, and it is thought there has been a terrible loss of life.

THE Artisans' Classes at the Royal Victoria Hall will reopen on Monday, October 1. Among the subjects taught will be arithmetic, physiology, physiography, shorthand, chemistry astronomy, mechanics, machine drawing, and electricity. Many of the classes are in connection with the Science and Art Department.

THE additions to the Zoological Society's Gardens during the past week include two Vulpine Phalangers (*Phalangista vulpina* ♀♀) from Australia, presented by Mr. J. M. Kirby; a Suricate (*Suricata tetradactyla*) from South Africa, presented by Lieut. Lionel de Latour Wells, R.N.; a Common Teal (*Querquedula crecca* ♀), British, presented by Mr. Bergman; an European Pond Tortoise (*Emys europæa*), European, presented by Master William Reed; a Robben Island Snake (*Coronella phocarum*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; an Ourang-outang (*Simia satyrus* ♀) from Borneo, a Ruffed Lemur (*Lemur varius*) from Madagascar, a Larger Hill Mynah (*Gracula intermedia*) from India, two —— Tree Ducks (*Dendrocygna* ——) from the Celebes? deposited; a —— Capuchin (*Cebus* —— ♀) from Brazil, two Brush-tailed Kangaroos (*Petrogale penicillata* ♂♀) from Australia, purchased; a Chinese Goose (*Anser cygnoides* ♀) from China, received in exchange.

OUR ASTRONOMICAL COLUMN.

COMET 1888 *c* (BARNARD).—The comet discovered by Barnard on September 2 is increasing in brightness, but is still a faint object. M. Bigourdan describes it on September 5 as showing a round nebulosity from 1′ to 1′·5 in diameter, with a fairly stellar nucleus, of magnitude $11\frac{1}{2}$ or 12. The nebulosity was not quite symmetrical with regard to the nucleus, but was most developed in the direction of position-angle 20°. The following elements are by Dr. A. Berberich from observations made at Strassburg, September 4 and 8, and Dresden, September 13 (*Astr. Nach.*, No. 2858):—

T = 1889 January 29·0959, Berlin M.T.

$$\left.\begin{aligned} \omega &= 341^\circ\ 43'\ 27''\cdot 9 \\ \Omega &= 358\ \ 6\ 20\cdot 8 \\ \iota &= 166\ 20\ 28\cdot 2 \end{aligned}\right\}\ \text{Mean Eq. } 1888\cdot 0.$$

$$\log q = 0\cdot 252291$$

Error of middle place (O – C). $\Delta\lambda = -2''$; $\Delta\beta = 0''$.

Ephemeris for Berlin Midnight.

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Sept. 30 ...	6 40 1 ...	8 37·7 N. ...	0·3694 ...	0·3326 ...	2·27
Oct. 2 ...	6 37 54 ...	8 23·4			
4 ...	6 35 33 ...	8 8·4	... 0·3637 ...	0·3081 ...	2·59
6 ...	6 32 56 ...	7 52·6			
8 ...	6 30 2 ...	7 35·9	... 0·3580 ...	0·2822 ...	3·00
10 ...	6 26 48 ..	7 18·2			
12 ...	6 23 14 ...	6 59·5 N. .	0·3523 ...	0·2550 ...	3·51

The brightness on September 2 has been taken as unity.

Prof. Krueger has deduced very similar elements to the above, using an observation made at Hamburg on September 13 instead of that made at Dresden.

COMETS BROOKS AND FAYE.—The following ephemerides for these two comets are in continuation of those given in NATURE for September 20 (p. 503), and are by Dr. H. Kreutz:—

1888.	Comet 1888 *c* (Brooks). R.A. h. m. s.	Decl. ° ′	Comet 1888 *d* (Faye). R.A. h. m. s.	Decl. ° ′
Sept. 30 ...	15 30 41 ...	14 47·4 N.	7 6 24 ...	14 3 N.
Oct. 2 ...	15 37 15 ...	13 28·2	7 10 19 ...	13 41
4 ...	15 43 35 ...	12 11·6	7 14 8 ...	13 19
6 ...	15 49 42 ...	10 57·6	7 17 51 ...	12 56
8 ..	15 55 35 ...	9 46·3	7 21 29 ...	12 33
10 ...	16 1 16 ...	8 37·7	7 25 0 ...	12 10
12 ...	16 6 46 ...	7 31·7	7 28 25 ...	11 47
14 ...	16 12 6 ...	6 28·2	7 31 43 ...	11 23
16 ...	16 17 18 ...	5 27·1 N.	7 34 55 ...	10 59 N.

Comet Brooks is slowly decreasing in brightness, but Comet Faye is brightening.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 SEPTEMBER 30—OCTOBER 6.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on September 30

Sun rises, 6h. 1m. ; souths, 11h. 49m. 46·2s. ; sets. 17h. 38m. : right asc. on meridian, 12h. 28·4m. ; decl. 3° 4′ S. Sidereal Time at Sunset, 18h. 18m.

Moon (New on October 5, 15h.) rises, 23h. 22m.* ; souths, 7h. 30m. ; sets, 15h. 32m. : right asc. on meridian, 8h. 8·0m. ; decl. 20° 22′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	8 25	13 16	18 7	13 55·2	14 2 S.
Venus.....	8 5	13 12	18 19	13 50·4	10 55 S.
Mars......	12 20	16 9	19 58	16 47·9	24 1 S.
Jupiter....	11 11	15 25	19 39	16 4·1	20 13 S.
Saturn....	1 10	8 41	16 12	9 19·2	16 28 N.
Uranus...	6 53	12 25	17 57	13 3·6	6 7 S.
Neptune..	19 37*	3 24	11 11	4 1·7	18 56 N.

* Indicates that the rising is that of the preceding evening.

Occultation of Planet and Star by the Moon (visible at Greenwich).

Oct.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
1	Saturn	—	15 59	16 49	110 288
3	*l* Leonis	5	3 0	3 52	55 199

Oct.	h.	
1	15	Saturn in conjunction with and 0° 55′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Oct. 1,	4 13	*m*
			,, 6,	3 52	*m*
S Arietis	1 58·6	11 59 N.	,, 4,		*M*
Algol	3 0·9	40 31 N.	,, 2,	4 20	*m*
			,, 5,	1 8	*m*
R Persei	3 22·9	35 17 N.	Sept. 30,		*M*
ζ Geminorum	6 57·5	20 44 N.	Oct. 4,	4 0	*M*
R Cancri	8 10·4	12 4 N.	,, 2,		*M*
S Ursæ Majoris	12 39·1	61 42 N.	,, 1,		*m*
U Ophiuchi	17 10·9	1 20 N.	Sept. 30,	21 20	*m*
			Oct. 5,	22 6	*m*
W Sagittarii	17 57·9	29 35 S.	Sept. 30,	19 0	*m*
β Lyræ	18 46·0	33 14 N.	Oct. 3,	19 0	m_2
R Lyræ	18 51·9	43 48 N.	,, 3,		*m*
S Sagittæ	19 50·9	16 20 N.	Sept. 30,	21 0	*M*
X Cygni	20 39·0	35 11 N.	Oct. 3,	5 0	*M*
T Vulpeculæ	20 46·7	27 50 N.	,, 3,	5 0	*M*
Y Cygni	20 47·6	34 14 N.	,, 2,	3 0	*m*
			,, 5,	3 0	*m*
δ Cephei	22 25·0	57 51 N.	Sept. 30,	21 0	*m*

M signifies maximum ; *m* minimum ; m_2 secondary minimum.

Meteor-Showers.

	R.A.	Decl.	
Near η Auriga	75°	41° N.	Swift ; streaks. October 2.
	225	52 N.	Bright ; slow. October 2.

GEOGRAPHICAL NOTES.

LIEUT. WISSMANN, who is to command the German Emin Pasha Expedition, has already done much excellent work in Africa, for which he received one of the medals of the Royal Geographical Society a few months ago. In his hands the interests of science are sure to be attended to. The Expedition will consist of two contingents, which will proceed through German East Africa by the south shore of Victoria Nyanza to the region between that lake and the Albert Nyanza. That the Expedition is sure to meet with difficulties is evident from the telegrams which are almost daily appearing from Berlin and from Zanzibar. The whole coast region is rising against the Germans, and it is to be feared that Lieut. Wissmann will have to proceed through a practically hostile country all the way to Wadelai. It is a pity that in the matter of Emin Pasha, which interests all Europe, Germany and England could not work hand in hand.

THE new American Geographical Society recently founded at Washington, and including the most eminent geologists and geographers of the United States, has already held several meetings, and begun work in earnest. It has been resolved that the Society will undertake the task of bringing out a new physical atlas of the United States, and for this purpose it has appointed a committee of specialists to proceed with the undertaking.

IT is to be regretted that Dr. Meyer's Expedition to Kilimanjaro has met with opposition in traversing Usambara, and has been compelled to return to the coast. Dr. Meyer, who was accompanied by Lenz's former companion, Dr. Baumann, intended to make a thorough survey of the whole region around Kilimanjaro, which, it will be remembered, he recently scaled to within a few hundred feet of the summit. The Chief Semboja, who is reported to have attacked the Expedition, has hitherto been on friendly terms with the whites. He is a great friend of the missionaries of the Church Missionary Society, and Mr. H. H. Johnston, in his book on Kilimanjaro, speaks in highly favourable terms of him, and was indeed indebted to him for many friendly services. It is to be feared, therefore, that the Germans have shown some want of tact in dealing with the Usambara people. It is to be hoped that Dr. Meyer may be able to resume his journey, and carry out the objects of his expedition.

THE last number of the *Izvestia* of the Russian Geographical Society will be welcome to geographers, as it contains a chapter from the work of Przevalsky, now in print, about his fourth journey to Central Asia. All discoveries made during that journey are summed up in this chapter, and the relations of the mountain ridges, mapped by the Russian traveller to other hilly tracts, formerly known, or explored by Mr. Carey, are shown. We hope that this chapter, the chief one of the whole work, will soon be translated into English. After giving a general sketch of the Kuen-lun Mountains, M. Przevalsky describes his journey along the Zaisan-saitu River, the ridges of Tsaidam, "Columbus" and "Moscow," the Lake Unfreezing, Przevalsky's ridge, and the "Windy Valley," which offers an advantageous route to China ; as also the return journey, the excursion to the Khatyn-zan River, the passage across the Altyn-tagh, and the return to Lake Lob-nor. The forty pages covered by the article are a rich mine of geographical information. The same number of the *Izvestia* contains an abstract from A. D. Carey's "Journey to East Turkistan," with a map.

THE remarkable facts communicated by M. Yadrintseff as to the drying up of lakes in Siberia have induced the Russian Geographical Society to take decisive steps for the exploration of the lakes of the Empire. A great number of copies of an instruction by Dr. Forel, of Lausanne, have been sent out to correspondents of the Society, as also a programme for collecting data on the subject, and it is hoped that in a year or two most valuable data will thus be gathered.

IN the last number of *Petermann's Mitteilungen*, Herr J. Menges raises once more the question of the possibility of utilizing the African elephant. Herr Menges points out that there is strong evidence that the elephant was made use of in ancient times in Africa, and asserts that no serious attempt has been made in modern times to subdue it to the uses of humanity. He maintains that it is quite as docile as the Indian elephant, and much stronger, and that if it could be really tamed and trained to work, it would be of immense utility in the opening up of Africa. But, unless some protection is accorded to the African elephant, Herr Menges believes that by the end of next century it will be quite extinct. We are therefore glad to notice that the British East African Company will take special means for the protection of the animal, and they might very well make some attempt to prove whether or not it is capable of being tamed.

NOTES ON METEORITES.[1]

III.

Identity of Origin of Meteorites, Luminous Meteors, and Falling Stars.

It is very fortunate for science that many of the meteorites so carefully preserved in our museums *have been seen to fall.* This being so we possess full accounts of the accompanying phenomena and effects.

These comprise the most vivid luminosity; visible and audible explosions, in some cases heard over thousands of square miles of country, and at times a long train in the sky indicating the meteor path, which sometimes remains visible for hours.

Now precisely similar effects have been noted when nothing has reached the earth's surface; and in the thousands of records of the phenomena presented by luminous meteors, fire-balls, bolides, or shooting or falling stars as they have been variously called, we have the links which connect in the most complete manner the falls of actual irons and stones from heaven with the tiniest trail of a shooting or falling star, *une étoile qui file, qui file, et disparait.*

The heavy masses fall by virtue of their substance resisting the friction of the air, the grains are at once burnt up and fill the upper regions of the earth's atmosphere with meteoric dust.

As we have seen, the weights of meteorites which have actually fallen vary between many tons and a few ounces, the latter being, in all probability, fragments shattered by the explosion. In the case of some shooting-stars, the actual weight involved has been estimated by Prof. Herschel as low as *two grains,* not one out of twenty estimated by him exceeding a pound.

It may appear impossible that such atoms should produce the brilliant effects observed, but Prof. Herschel has calculated that a single grain moving at the rate of 30 miles a second represents a dynamical energy of 55,675 foot-pounds. This energy is converted by the resistance of our grosser air into heat, as the motion of a projectile is converted into heat by its impact on the target;[2] and hence the combustion of the matter of the meteorite, and perhaps even the incandescence of the air through which it rushes with such lightning velocity. This luminosity commences often at a height of 80 miles, and sometimes even higher, in regions where the atmosphere must be excessively rare.

Could these little bodies pierce our envelope as readily as do their larger cousins, the meteoric stones and meteoric irons, we should certainly have the advantage of placing them in our museums; but, on the other hand, the bombardment—the *feu-de ciel*—might be one to which the *feu-d'enfer* of all terrestrial artillery would be, in the gross total of results, as mere child's play.

But the identity of such phenomena as these is by no means the only line of evidence demonstrating the connection now in question.

Proof from the Chemistry of Fire-balls.

The spectral appearances observed with meteors, fire-balls, and shooting-stars, which explode and produce luminous effects, are entirely in harmony with those observations on the spectra of meteorites to which I have referred.

The observations, so far as they have gone, have given decided indications of magnesium, sodium, lithium, potassium, and of the carbon flutings seen in comets.

Prof. Herschel and Herr Konkoly have both noticed that in the generality of cases the lines of magnesium (one of the constituents of the olivine) show themselves first in the ordinary meteor or falling star, and the beautiful green light which is so often associated with these falling bodies is due to the incandescence of the vapour of magnesium.

The following quotations from Konkoly and Prof. Herschel are among the authorities which may be cited for the above statement:—

"On August 12, 13, and 14 I observed a number of meteors with the spectroscope; amongst others, on the 12th, a yellow fire-ball with a fine train, which came directly from the Perseid radiant. In the head of this meteor the lines of lithium were clearly seen by the side of the sodium line. On August 13, at 10h. 46m. 10s., I observed in the north-east a magnificent fire-ball of emerald-green colour, as bright as Jupiter, with a very slow motion. The nucleus at the first moment only showed a very bright continuous spectrum with the sodium line; but a second after I perceived the magnesium line, and I think I am not mistaken in saying those of copper also. Besides that, the spectrum showed two very faint red lines."[1]

"A few of the green 'Leonid' streaks were noticed in November (1886) to be, to all appearances, monochromatic, or quite undispersed by vision through the refracting prisms; from which we may at least very probably infer (by later discoveries with the meteor-spectroscope) that the prominent green line of magnesium forms the principal constituent element of their greenish light."[2]

Again, later on in the same letter, Prof. Herschel mentions Konkoly's observation of the bright *b* line of magnesium, in addition to the yellow sodium line, in a meteor on July 26, 1873. I again quote from Prof. Herschel:—

"On the morning of October 13 in the same year, Herr von Konkoly again observed with Browning's meteor-spectroscope the long-enduring streak of a large fire-ball, which was visible to the north-east of O'Gyalla. It exhibited the yellow sodium line and the green line of magnesium very finely, besides other spectral lines in the red and green. Examining these latter lines closely with a star-spectroscope attached to an equatorial telescope, Herr von Konkoly succeeded in identifying them by direct comparison with the lines in an electric Geissler-tube of marsh-gas. They were visible in the star-spectroscope for eleven minutes, after which the sodium and magnesium lines still continued to be very brightly observable through the meteor-spectroscope."[3]

Another series of observations[4] gives continuous spectra for the nucleus, and two trains with sodium, and a third with sodium and a predominant green band, which was doubtless *b* of magnesium, the meteor itself being of emerald-green colour.

In cases where the temperature has been higher, the bright line spectrum of iron has been associated with the bright lines of magnesium in the spectrum of the falling star, so that the two substances which are among the chief constituents of stones and irons—precisely the two substances which we should expect to find—are actually those which have been observed.

The two lines which Konkoly supposes are probably due to copper will, I expect, be found to be iron lines when other observations are made of the spectra of meteors.

These spectral appearances are naturally associated with colours, and again we find that the colours of the trail, when meteorites have fallen, closely resemble those observed when no fall has been observed.

Green is a tolerably common colour, especially in slow-moving fire-balls about equal to Venus in lustre. These generally leave a short trail of red sparks.

About 10 per cent. of all shooting-stars show a distinct colour, the most usual being orange or red, the slowly-moving ones generally being red. The larger ones, or those with the longest trails, often turn from orange to bluish-white, like burning magnesium. Sometimes the change is very sudden and startling.[5]

A purple or mauve tint, like that given by copper, is sometimes seen.

Proof from the Aurora.

When we come to consider the number of meteorites which fall upon the earth daily we shall find that it is enormous; and this being so, if we can trace this dust in the air, or after it has fallen, or both, if chemical examination shows it to be identical with that of meteorites, we shall be supplied with another argument which can be used in support of the fact that the bodies which produce the dust are meteoric in their origin.

One must suppose that these meteors in their passage through the air break into numerous fragments, that incandescent particles of their constituents, including nickel, iron, manganese, and the various silicates of iron, are thrown off, and that these or the products of their combustion eventually fall to the surface as almost impalpable dust, among which must be magnetic oxide of iron more or less completely fused. The luminous trains of falling stars are probably due to the combustion of these innumerable particles, resembling the sparks which fly from a ribbon of iron burnt in oxygen, or the particles of the same metal

[1] Continued from p. 458.

[2] The particles of iron in a large projectile, after impact, which is accompanied by a flash of light, are usually brought to a dark blue colour, which would correspond to about 555° F., but the momentary heat imparted is certainly greater than this.

[1] Konkoly, *Observatory*, vol. iii. 157.
[2] Herschel, letter to Nature, vol. xxiv. p. 507.
[3] *Ibid.* See also *Astr. Nach.*, No. 2014.
[4] *Monthly Notices*, vol. xxxiii. p. 575.
[5] Corder, *Monthly Notices*, vol. xl. p. 33

thrown off when striking a flint. It is known that such particles in burning take a spherical form, and are surrounded by a layer of black magnetic oxide.

How are we to trace this dust in the air? It is well known that at times the air is electrically illuminated, not only by the flashes of lightning which pass along its lower levels, but by so-called "auroral" displays in its higher reaches.

It is now many years since the idea was first thrown out that the aurora was in some way connected with shoooting-stars.

M. Zenger has prepared a catalogue of auroræ observed from 1800 to 1877, in which he shows an apparent connection between the brightest displays and the appearance of large numbers of shooting-stars.

M. Denza noted the same connection on November 27, 1872, and remarked that he had noticed it before.

Admiral Wrangel, as quoted by Humboldt, observed that in the auroras so constantly seen on the Siberian coast, the passage of a meteor never failed to extend the luminosity to parts of the sky previously dark.[1]

It is clear that in such a case as this the spectroscope is the only chemical aid applicable, and it has long been recognized that the spectrum observed is *not* the spectrum of the constituents of the atmosphere, as we can study it in our laboratories.

The spectrum, however, strictly resembles that seen in the "glows," to which reference has been made; if the factors present in both cases are meteor dust, low pressure, and feeble electric currents, the resulting phenomena should not be dissimilar.

The results of recent inquiries certainly justify us, therefore, in concluding that the upper reaches of the atmosphere contain particles giving us the spectra of magnesium, manganese, iron, and carbon.

The natural origin is the dust of those bodies which are continually entering those regions, and hence the proof afforded by the spectroscopic observation of shooting-stars, that they are identical in chemical composition with meteorites, is strengthened by these auroral observations, while, on the other hand, the origin of the auroral spectrum is placed beyond all doubt.

Proof from the Fallen Dust.

It is universally recognized that the atmosphere holds in suspension an immense number of very minute particles of organic and inorganic origin. These must be either dust taken up by aërial currents from the ground—the result of volcanic action—or extra-terrestrial bodies. Many scientific men, among whom we may mention Ehrenberg, Daubrée, Reichenbach, Nordenskjöld, Tissandier, Murray, and Renard, have studied this problem. Dust collected in various places at different times has been examined with a view of determining whether its origin was meteoric. In many cases, in which chiefly definite iron chondroi have been observed, the evidence has seemed very strong in favour of the view.

It is at once obvious that the detection of such dust which falls on the general surface of the land is hopeless, and that that which is collected on snow in inhabited countries containing foundries and the like is doubtful.

But a considerable advance of this question has recently been made in studying the deep-sea deposits collected by the *Challenger* Expedition. Messrs. Murray and Renard,[1] in giving the results of their researches, point out that at the greatest

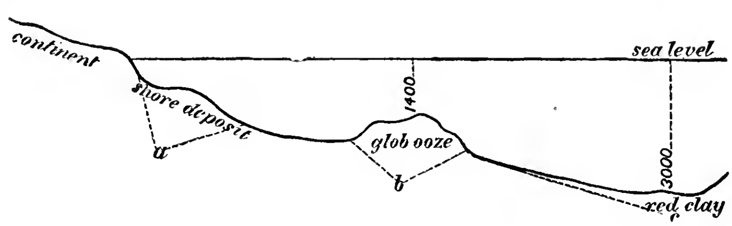


FIG. 3.—Section of ocean showing red clays at depths of 3000 fathoms (18,000 feet).

depths of the ocean furthest from land, the sea bottom is very different from that nearer the coast lines.

Under these necessary conditions of exceeding slow deposition and absence from ordinary sources of contamination, it is clear that the problem can be attacked under the best conditions.

We read:—"The considerable distance from land at which we find cosmic particles in greatest abundance in deep-sea deposits, eliminates at once objections which might be raised with respect to metallic particles found in the neighbourhood of inhabited countries. On the other hand, the form and character of the spherules of extra-terrestrial origin are essentially different from those collected near manufacturing centres. These magnetic spherules have never elongated necks or a cracked surface, like those derived from furnaces, with which we have carefully compared them. Neither are the magnetic spherules with a metallic centre comparable either in their form or structure to those particles of native iron which have been described in the eruptive rocks, especially in the basaltic rocks of the north of Ireland, of Iceland, &c."

Messrs. Murray and Renard then state on what they rely in support of their view that many of the particles thus obtained from great depths are of cosmic origin:—

"If we plunge a magnet into an oceanic deposit, especially a red clay from the central parts of the Pacific, we extract particles, some of which are magnetic from volcanic rocks, and to which vitreous matters are often attached; others again are quite isolated, and differ in most of their properties from the former. The latter are generally round, measuring hardly 0·2 mm., generally they are smaller, their surface is quite covered with a brilliant black coating, having all the properties of magnetic oxide of iron; often there may be noticed upon them cup-like depressions clearly marked. If we break down these spherules in an agate mortar, the brilliant black coating easily falls away, and reveals white or gray metallic malleable nuclei, which may be beaten out by the pestle into thin lamellæ. This metallic centre, when treated with an acidulated solution of sulphate of copper, immediately assumes a coppery coat, thus showing that it consists of native iron. But there are some malleable metallic nuclei extracted from the spherules which do not give this reaction, they do not take the copper coating. Chemical

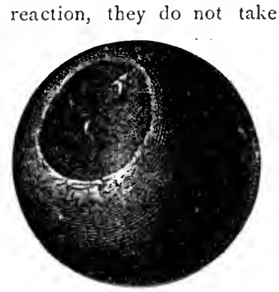


FIG. 4.

FIG. 5.

FIG. 4.—Black spherule with metallic nucleus (60 : 1). This spherule. covered with a coating of black shining magnetite, represents the most frequent shape. The depression here shown is often found at the surface of these spherules. From 2375 fathoms, South Pacific.

FIG. 5.—Black spherule with metallic nucleus (60 : 1). The black external coating of magnetic oxide has been broken away to show the metallic centre, represented by the clear part at the centre. From 3150 fathoms, Atlantic.

reaction shows that they contain cobalt and nickel; very probably they constitute an alloy of iron and these two metals, such as is often found in meteorites, and whose presence in large quantities hinders the production of the coppery coating on the iron. G. Rose has shown that this coating of black oxide of

[1] "Cosmos" (Otté), vol. i. p. 114.

[1] "On the Microscopic Characters of Volcanic Ashes and Cosmic Dust and their Distribution in Deep-sea Deposits," Proc. R.S.E., and NATURE, vol. xxix. p. 585.

iron is found on the periphery of meteorites of native iron, and its presence is readily understood when we admit their cosmic origin. Indeed these meteoric particles of native iron, in their transit through the air, must undergo combustion, and, like small portions of iron from a smith's anvil, be transformed either entirely or at the surface only into magnetic oxide, and in this latter case the nucleus is protected from further oxidation by the coating which thus covers it."

We are next shown that these metallic chondroi occur with stony chondroi, so that if the interpretation of a cosmic origin for the magnetic spherules with a metallic centre be not considered established in a manner absolutely beyond question, it almost becomes so when we take into account their association with the silicate spherules, never found in rocks of a terrestrial origin. These are thus described:—

"Among the fragments attracted by the magnet in deep-sea deposits we distinguish granules slightly larger than the spherules with the shining black coating above described. These are yellowish-brown, with a bronze-like lustre, and under the microscope it is noticed that the surface, instead of being quite smooth, is grooved by thin lamellæ. In size they never exceed a millimetre, generally they are about 0·5 millimetre in diameter; they are never perfect spheres, as in the case of the black spherules with a metallic centre; and sometimes a depression more or less marked is to be observed in the periphery. When examined by the microscope, we observe that the lamellæ which compose them are applied the one against the other, and have a radial eccentric disposition. It is the leafy radial structure (*radialblättrig*), like that of the *chondres* of bronzite, which predominates in our preparations. We have observed much less rarely the serial structure of the *chondres* with olivine, and indeed there is some doubt about the indications of this last type of structure. Fig. 6 shows the characters and texture of one of these spherules magnified twenty-five diameters."

FIG. 6.—Chondros. Spherule of bronzite (25 : 1) from 3500 fathoms in the Central South Pacific, showing many of the peculiarities belonging to *chondres* of bronzite or enstatite.

It is worthy of remark that, associated with these chondroi in the red muds at the greatest depths in the ocean, are found manganese nodules in enormous numbers. If a section be made of one of these, a number of concentric layers will be observed arranged around a central nucleus—the same as in a urinary calculus. When the peroxide of manganese is removed by strong hydrochloric acid, there remains a clayey skeleton which still more strongly resembles a urinary calculus, according to Mr. Murray.

This skeleton contains crystals of olivine, quartz, augite, magnetite, or any other materials which were contained in the clay from which the nodule was taken. In the process of its deposition around a nucleus, the peroxide of manganese has inclosed and incorporated in the nodule the clay and crystals and other materials in which the nucleus was embedded. The clayey skeleton thus varies with the clay or ooze in which it was formed. Those from a fine clay usually adhere well together; those from a globigerina ooze have an areolar appearance; those from a clay with many fine sandy particles usually fall to pieces. Mr. Murray attributes the origin of these nodules entirely to the decomposition of volcanic rocks:—

"Wherever we have pumice containing much magnetite, olivine, augite, or hornblende, and these apparently undergoing decomposition and alteration, or where we have evidence of great showers of volcanic ash, there we find the manganese in greatest abundance. This correspondence between the distribution of the manganese and volcanic *débris* appears to me very significant of the origin of the former. I regard the manganese, as we find it, as one of the secondary products arising from the decomposition of volcanic minerals.

"Manganese is as frequent as iron in lavas, being usually associated with it, though in very much smaller amount. In magnetite and in some varieties of augite and hornblende the protoxide of iron is at times partially replaced by that of manganese.

"In the manganese of these minerals and in the carbonic acid and oxygen of ocean waters we have the requisite conditions for the decomposition of the minerals, the solution of the manganese, and its subsequent deposition as a peroxide."[1]

These nodules have been examined in the same way as the meteoric dust. Naturally the chief manganese fluting (the chief auroral line) has been seen.

The question arises, therefore, whether the origin of these deep-sea concretionary deposits of iron and manganese, which are unrepresented in any deep-sea geological deposit, may not be in part, even if in small part, meteoritic, and represent, like the chondroi, another form of fallen dust.

Proof from Similar Velocities.

Again, the meteorites, as we have seen, enter our atmosphere with very different velocities. The same thing happens with falling stars, which on this account have been divided into three classes as follows:—

Class I. Swift, streak-leaving meteors.
II. Slow, with trains of sparks.
III. Small, quick, short-pathed, sometimes with streaks.

It has also been determined that the luminous effect which is common to the fall of a meteorite or the appearance of a shooting-star begins at about the same height. In fact, we have in meteorites, large fire-balls, and shooting-stars, a progression both with regard to the height at which they become visible and the nearness to the earth at which their luminosity is extinguished.

The actual determination of these heights was commenced by two Göttingen students—Brandes and Benzenberg—in 1798, at the suggestion of Chladni, with the result that the upper reaches of the earth's atmosphere were found to be pierced by bodies entering it with planetary velocities.

Profs. Herschel and Newton were the first to discuss the data accumulated on this subject,[2] while, as early as 1864, Father Secchi made use of the electric telegraph in securing simultaneous observations.[3] The results of these combined inquiries may be thus shown in the case of shooting-stars:—

	Beginning. Height in miles.	End. Height in miles.	Authority.
Europe and America, 1798–1863	70·1	54·2	H.
	73·5	50·6	N.
Italy, 1864	74·6	49·7	S.
Average ...	72·7	51·5	

In Herschel's values fire-balls are excluded, and hence the limits are narrower.[4] Fire-balls often arrive within 20 miles of the earth's surface, and then the concussion is of nearly the same intensity whether stones fall or not.

Such determinations as these, when the observations can be depended upon, can be made with the greatest nicety and by graphical methods. One of the earliest employed—a description of which will give a fair idea of the investigation—is due to Colonel Laussedat.[5]

The observations stating the path of each meteor among the stars having been obtained, a 12-inch celestial globe is "rectified" in the usual manner for the place and time. In this way we get first the azimuth and altitude of the beginning and end of each trail. This is done for each place at which the same meteor is observed.

The results are then plotted on a large-scale map, on which the altitudes and longitudes of the places of observation and the distances between them can be determined. The scale of the map permits the height of the intersection of the lines of sight to be at once found, and the agreement or disagreement of the observations can be noticed, thus allowing the worst observations to be rejected.

[1] Murray, NATURE, vol. xv. p. 340.
[2] Herschel, B.A. Report, 1863, p. 328; Newton, *Silliman's Journal*, 2nd Series, vol xxxvii., July 1864.
[3] *Bull. Meteor.*, vol. iii. p. 67.
[4] *Monthly Notices*, vol. xxv. p. 159.
[5] *Comptes rendus*, vol. lviii. p. 1100, 1864.

By taking such observations as these in different places it is possible not only to determine the height at which they enter but the velocity with which they pass through the upper regions of the air, even supposing they do not eventually get to the bottom. The lowest velocity determined up to the present time is something like 2 miles per second; the maximum is something like 50 miles a second; but we may say that the average rate of movement is 30 miles a second, which is about 150 times faster than a shell leaving one of our most powerful guns.

J. NORMAN LOCKYER.

(To be continued.)

THE ELECTRIC TRANSMISSION OF POWER.[1]

II.

THE next point to consider is the loss of power on the road between the dynamo at the one end and the motor at the other. This problem was perhaps seriously attacked for the first time in the discussion of a paper read by Messrs. Higgs and Brittle at the Institution of Civil Engineers in 1878, and that problem was considered in some detail theoretically and experimentally at the lecture I gave during the meeting of the British Association in Sheffield in the following year. It was then shown that, since the power developed by the generator and motor depended on the product of the current into the electric pressure, while the loss when power was transmitted through a given wire depended on the square of the current and was independent of the electric pressure, the economical transmission of power by electricity on a large scale depended on the use of a very large electric pressure and a small current, just as the economic transmission of much power by water depended on the use of a very large water pressure and a small flow of water. At that time it was not thought possible to construct a small dynamo to develop a very large electric pressure, or potential difference as it is technically called, and therefore it was proposed to join up many dynamos in series at the one end and many lamps or electromotors in series at the other, and to transmit the power by a very small current, which passed through all the dynamos and all the lamps in succession, one after the other.

You have an example to-night of the realization of this principle in the fifteen arc lamps that are all in series outside this Drill Hall, and are worked with a small current of only 6·8 amperes, as indicated in the wall diagram; and a further example in the thirty arc lamps at the Bath Flower Show, which are also all worked in series with the small current passing through them; but it is known now how to produce a large potential difference with a single dynamo, so that a single Thomson-Houston dynamo belonging to Messrs. Laing, Wharton, and Down supplies the current for each of the two circuits.

The electric pressure, or potential difference, between the terminals of any arc lamp is not high, but it is between the main wires near the dynamo as well as between these wires and the ground. How far does this lead to the risk of sparks or unpleasant shocks? That is a point that can be looked at in a variety of ways. First, there is the American view of the matter, which consists in pointing out to people exactly what the danger is, if there be any, and training them to look out for themselves: let ordinary railway trains, say the Americans, run through the streets, and let horses learn to respect the warning bell. Next, there is the semi-paternal English system, which cripples all attempts at street mechanical locomotion, because we are conservative in our use of horses, and horses are conservative in their way of looking at horseless tramcars. Lastly, there is the foreign paternal system, which, carried to its limit, would prohibit the eating of dinners because some people have at some time choked themselves, and would render going to bed a penal offence because it is in bed that most people have died.

We laugh a good deal at the rough-and-ready manner adopted on the other side of the Atlantic. The Americans, no doubt, are very ignorant of the difficulties that properly-minded people would meet with, but it is a blissful ignorance where it is folly to be wise. Every English electrician who has travelled in America comes back fully impressed with their enterprise and their happy-go-lucky success. They have twenty-two electric tramways, carrying some 4,000,000 passengers annually, to our four electric tramways at Portrush, Blackpool, Brighton, and Bessbrook. Why, New York city alone, Mr. Rechenzaun tells me, possesses 300 miles of ordinary tramway track, and Philadelphia 430 miles, so there is more tramway line in these two cities than in the whole of the United Kingdom put together. Now there would be no difficulty in proving, to anyone unfamiliar with railway travelling, that to go at 50 miles an hour round a curve with only a bit of iron between him and eternity would be far too risky to be even contemplated. And yet we do go in express trains, and even 80 miles an hour is beginning to be considered not to put too great a demand on the funds of life insurance companies. The American plan of basing a conclusion on experience rather than on anticipations is not a bad one; and if we follow that plan, then, taking into account that there are 75,000 arc lights alight every night on the Thomson-Houston high-potential circuits throughout the world, and the comparatively small number of people that have suffered in consequence (not a single person, I am assured, outside the companies' staffs) we are compelled to conclude that high potential now is what 30 miles an hour was half a century ago—uncanny rather than dangerous.

But it is possible to use a very large potential difference between the main wires by means of which the electric power is economically conveyed a considerable distance, and transformed into a very small potential difference in the houses where it is utilized. An electric transformer is equivalent to a lever, or wheel and axle, or any other of the so-called mechanical powers. You know that a large weight moving through a small distance can raise a small weight through a large distance; there is no gain in the amount of work, but only a transformation of the way in which the work is done. A large weight moving through a small distance is analogous with a high potential difference and a small current, while a small weight moving through a large distance is analogous with a small potential difference, and a large current, and an electric transformer is for the purpose of effecting the transformation with as little loss as possible, so that what is lost in potential difference may, as far as possible, be all gained in current.

Electrical transformation may be effected by (1) alternate current transformers, (2) motor-dynamos, (3) accumulators, or secondary batteries, (4) direct-current transformers. Of these apparatus, the eldest by far is the alternate-current transformer, as it is merely the development of the classical apparatus invented by Faraday in 1831, and familiar to many of you as the Ruhmkorff, or induction-coil. A combination of a motor and dynamo was suggested by Gramme in 1874. Accumulators are the outcome of Planté's work, while direct-current transformers are quite modern, and not yet out of the experimental stage.

After studying the literature on this subject, it appears, as far as I have been able to judge, that the first definite proposal to use a high potential difference in the street mains, and transform down to a low potential difference in the houses, was made in the lecture given by me at the meeting of the British Association in Sheffield in 1879, on which occasion I explained and showed in action the motor-dynamo principle suggested by Prof. Perry and myself. The apparatus on the platform is not unlike that shown on the former occasion: an Immisch motor working at 500 volts, and with a current of 6·8 amperes, is geared direct to a Victoria Brush dynamo giving five times that current, and we will now use this larger current to produce an electric fire. [Experiment shown.] Messrs. Paris and Scott have combined the motor and dynamo into one machine, which they have kindly lent me, and by means of which we are now transforming about 700 volts and 6·8 amperes into 100 volts and about 40 amperes used to light that group of sunbeam incandescent lamps or work these motors. [Experiment shown.]

Lastly, here is a working illustration of the double transformation proposed by MM. Deprez and Carpentier in 1881, by means of which—while the potential difference between the mains may be 2000 or 10,000 volts, if you like—not merely is the potential difference in the house so low that you could hardly feel anything if you touched the wires, but, in addition, there is the same security against shocks in the dynamo-room. This alternate-current machine is producing about 50 volts, which is transformed up to 2000 volts by means of this transformer. At the other end of the platform, by means of a similar transformer, the 2000 volts is transformed down again to 50 volts, employed to light that cluster of low-voltage incandescent lamps. [Experiment shown.] For the use of this apparatus I am indebted to the kindness of the Anglo-American Brush Company.

In this experiment there is, as a matter of fact, still more transformation than that I have yet mentioned, because, whereas in actual practice the alternate-current dynamo, as well as the small dynamo used to produce the current for magnetizing the electromagnets in the alternate-current dynamo, would be worked by

[1] Lecture delivered by Prof. Ayrton, F.R.S., at the Drill Hall, Bath, on Friday, September 7, 1888. Continued from p. 511.

a steam, gas, or water engine, I am working them both by electromotors, since a steam-engine or a water-wheel would be an unsuitable occupant of the Drill Hall. Practically, then, a steam-engine on the land belonging to the Midland Railway Company, on the other side of the Lower Bristol Road, is driving a Thomson-Houston dynamo; this is sending a small current working these high-voltage constant-current Immisch motors. The motors being geared with low-voltage dynamos the potential difference is transformed down, the first alternate-current transformer transforms it up again, and the second alternate-current transformer transforms it down again, so that there are in fact three transformations taking place in this experiment on the platform before you. For the benefit of the electricians present, I may mention that the two motors are running in series, and that their speed is kept constant by means of a centrifugal governor which automatically varies the number of the convolutions of the field magnet that are being utilized at any moment. In fact, since the dynamo maintains the current constant that is passing through each motor, the function of the governor may be regarded as that of proportioning the potential difference maintained at the terminals of either motor to the load on the motor at any moment.

A vast district in London, extending from Regent's Park on the north to the Thames on the south, from the Law Courts on the east to Hyde Park on the west, has over 20,000 incandescent lamps scattered over it all worked from the Grosvenor Gallery in Bond Street by means of alternate-current transformers which convert the 2000 volts maintained between the street mains into 100 volts in the houses, and this London Electric Supply Company have arranged for a vast extension of this system to be worked from Deptford.

In America, alternate-current transformers are, due to the remarkable enterprise of Mr. Westinghouse, used to light 120,000 incandescent lamps in sixty-eight towns. In fact the electric lighting of a whole town from a central station begins to excite less astonishment than the electric lighting of a single house did ten years ago.

The efficiency of a well-made alternate-current transformer is very high, being no less than 96·2 per cent. when the transformer is doing its full work, and 89·5 per cent. when it is doing one-quarter of its full work, according to the experiments made by our students. It certainly does seem most remarkable, and it reflects the highest praise on the constructors of electrical machinery, that motive power can be converted into electrical power, electrical power at low pressure into electrical power at high pressure, or electrical power at high pressure into electrical power at low pressure, or, lastly, electrical power into motive power, in each case with an efficiency of not less than 94 per cent.

As a further illustration of the commercial importance of this electric transformation I will show you some experiments on electric welding, one of the latest developments in electrical engineering. To weld a bar of iron one square inch in section requires a gigantic current of some 13,000 amperes. To convey this current even a few yards would be attended with a great waste of power; consequently, while an enormous current is passed through the iron to be welded, only a comparatively small current is transmitted along the circuit from the dynamo to the welding apparatus. Mr. Fish, the representative of Prof. Elihu Thomson, of America, to whom this apparatus is due, will be so kind as to first show us the welding together of two bars of square tool steel, the edge of each bar being $\frac{3}{4}$ of an inch, and the operation is, as you see, entirely completed in some fifteen seconds. For this experiment an alternate current of 20 amperes will be produced by the dynamo at the other side of the Lower Bristol Road, and this current will be converted by the transformer on the platform into one of 9000 amperes, large enough for 12,000 of these incandescent lamps if they were placed in parallel and the current divided among them. He will next try welding some thicker bars, and lastly he proposes welding together two pieces of aluminium which it is extremely difficult, if not impossible, to weld in any other way. The bars, as you see, are in each case pressed together end on, and, in consequence of the electric resistance of the very small gap between the bars being much higher than that of the bars themselves, the current makes the ends of the bars plastic long before it even warms the whole bar, so that I can, as you see, hold the bar at a distance of three or four inches from where the weld has been made without experiencing any marked sense of warmth. The heat is, in fact, applied exactly where we require it, the temperature can be adjusted with the greatest nicety so as not to burn the steel, and the softening of the bar is effected throughout its entire cross-section. Hence a very good weld indeed can be made by end pressure. We have to thank Mr. Fish, not merely for showing us these most interesting experiments on electric welding, but for supplying the electric power for many of the experiments I have been showing you, and for the electric lighting of the Drill Hall.

To Mr. Snell, the representative of Mr. Immisch, our best thanks are due for his having devoted several days in arranging the two high-voltage, constant-current motors, to drive the dynamos with that constancy of speed which you observe. This ingenious telpher model, to which I shall refer presently, is the handiwork of Mr. Bourne, and considering that it has had to be hastily taken to pieces, and hastily put together again, it is surprising that it works as well as it does. An ordinary watch is a very trustworthy, steady-going machine, but if one had to take it to pieces hastily, and as hastily to put it together again, one might expect it to lose. Indeed, if you or I had to do it, we should not be surprised if it did not go at all, and so was only right twice every twenty-four hours.

For the arrangements of the models and the smaller experiments, as well as for the admirable execution of many of the diagrams, our best thanks are due to Mr. Raine.

Did time allow I should like to describe to you to what perfection the system of economical distribution with accumulators, originally proposed by Sir William Thomson in 1881 and shown in its very simplest form in the wall diagram, has been brought by Mr. King, the engineer to the Electrical Power Storage Company; how the cells when they are fully charged are automatically disconnected from the charging circuit, and electrically connected with the discharging circuit; how the electric pressure on the discharging or house mains is automatically kept constant, so that the brightness of the lamps is unaffected by the number turned on; and how cells that are too energetic have their ardour automatically handicapped, and not allowed to give more current than is being supplied by the less active ones.

During the last few months fierce has been the battle raging among the electricians, the war-cry being "alternate-current transformers *versus* accumulators," while the lookers-on, with that better view of the contest that they are proverbially said to possess, have decided that the battle is a drawn one. Neither system is the better under all circumstances: if the district to be lighted be a very scattered one, use alternate-current transformers by all means; but if the houses to be lighted are clustered together at a distance from the supply of power, then the storing property possessed by accumulators, which enables the supply of electric power to far exceed the capacity of the dynamos and engines in the busiest part of the twenty-four hours, will win the battle for accumulators. Any direct-current system of distribution such as is furnished by accumulators has also the very great advantage that it lends itself to the use of the very efficient electromotors which I have been using this evening. Alternate-current motors do exist, but they are still in the experimental stage, and are not yet articles of commerce.

Secondary batteries have caused much heart-burning, for their users, from the apparent fickleness of their complex chemical action, yet but imperfectly understood. But we have at length been taught what is good and what is bad treatment for them; and after years of brave persevering application on the part of the Electrical Power Storage Company, that forlorn hope the secondary battery has become one of the most useful tools of the electrical engineers; and secondary cells, some of which, thanks to the kindness of that Company, I am using here to-night to supply power for lamps and motors, may now be trusted to have a vigorous long life. That Company, I learn, undertake henceforth to keep their cells in order, when used for central station work, for $12\frac{1}{2}$ per cent. per annum, and I understand that they have such confidence in them that they anticipate making no little money by incurring this insurance office responsibility. It is not, then, surprising that the Chelsea Supply Company have decided to use secondary batteries on a large scale for the economical distribution of light and power in their district.

Oliver Goldsmith said, more than a hundred years ago, in his "Life of Richard Nash, Esquire": "People of fashion at Bath, . . . when so disposed, attend lectures on the arts and sciences, which are frequently taught in a pretty superficial manner, so as not to tease the understanding, while they afford the imagination some amusement." I want not to be superficial, yet I must not tease your understanding, and so we will not lose ourselves in technical details. If, however, my remarks have led you to appreciate the vast economical importance of using very large electric pressures, and to grasp that, by substituting 2000 volts for 50 volts, when transmitting a certain amount of electric power, the current can be reduced to the one-fortieth part, and the waste of power, when transmitted along a given length of a given wire to the one-fortieth of the one-fortieth—that is, to the one sixteenth-hundredth part—your imagination will have been kindled as well as amused.

With a loss on the road of only 11 per cent., M. Deprez has, by using 6000 volts, transmitted 52 horse-power over a distance of about 37 miles through a copper wire only one-fifth of an inch in diameter. A piece of the actual conductor he employed I hold in my hand: the copper wire is coated with an insulated material, and then with a leaden tubing, so that the outside may be touched with perfect impunity, in spite of the high potential difference employed. M. Deprez's dynamo and motor were not nearly as efficient as he could make them now, so that his terminal losses were unnecessarily great, and the efficiency of the whole arrangement, wonderful as it was, was not so startling as it would otherwise have been. I have told you that the loss in dynamo and motor has actually been reduced to only $12\frac{1}{2}$ per cent.; so that, if a dynamo and motor of this efficiency had been used by M. Deprez, the total loss in the whole transmission over 37 miles would have been under 25 per cent. Indeed, by using only 1250 volts, Mr. Brown has succeeded in transmitting 50 horse-power supplied by falling water at Kriegstetten to Solothun, in Switzerland, five miles away, with an entire loss in the dynamo, motor, and the five miles of going and returning wire of only 25 per cent.; so that three-quarters of the total power supplied by the water at Kriegstetten was actually delivered to machinery at Solothun, five miles away.

In less than twenty years, then, from Gramme's practical realization of Pacinotti's invention, we have power transmitted over considerable distances by electricity with only a total loss of 25 per cent., whereas the combined loss in an air-pump and air-motor or in a water-pump and water-motor is 40 per cent., irrespective of the additional loss by friction or leakage that occurs *en route*. We cannot help feeling that we are rapidly arriving at a new era, and that it will not merely be for the inauguration of the quick transmission of our bodies by steam, or the quick transmission of our thought by telegraph, but for the economical transmission of power by electricity, that the Victorian age will be remembered.

I showed you a little while ago an electric fire. Was that a mere toy, or had it any commercial importance? To burn coal, to work dynamos, and to use the electric current to light your houses and your streets is clean and commercial; to use the current to warm your rooms clean but wasteful, on account of the inefficiency of the steam-engine. But when the dynamos are turned by water power which would otherwise be wasted, the electric current may be economically used, not merely to give light, but also to give heat. And when the electric transmission of power becomes still more perfect than at present, even to burn coal at the pit's mouth where it is worth a shilling a ton may, in spite of the efficiency of the steam-engine being only one-tenth, be the most economical way of warming distant towns where coal would cost 20*s*. a ton. Think what that would mean! —no smoke, no dust, a reform effected commercially which the laws of the land on smoke prevention are powerless to bring about, a reform effected without the intervention of the State, and therefore dear to the hearts of Englishmen.

I am aware that this idea of burning coal at the pit's mouth and electrically transmitting its power has quite recently been stated to be commercially impracticable. But is that quite so certain?—for in 1878 it was stated that, although telephones might do very well for America, they certainly would never be introduced into Great Britain, as we had plenty of boys who were willing to act as messengers for a few shillings a week. The phonograph was also declared to be worked by a ventriloquist, and electric lighting on a large scale was proved to be too expensive a luxury to be ever carried out. Putting a Conservative drag on the wheels is a very good precaution to take when going down hill, but it is out of place in the up-hill work of progress.

To-day the electric current is used for countless purposes. Not only is it used to weld, but by putting the electric arc inside a closed crucible, smelting can be effected with a rapidity and ease quite unobtainable with the ordinary method of putting the fire outside the crucible. If one had pointed out a few years ago that it was as depressing scientifically to put a fire outside a crucible when you wanted to warm the inside, as Joey Ladle, the cellarman, found it depressing mentally "to take in the wine through the pores of the skin, instead of by the conwivial channel of the throttle," who would have believed that in 1888, a 500 horse-power dynamo would be actually employed to produce an electric arc inside a closed crucible in the manufacture of aluminium bronze.

But, of all the many commercial uses to which the electric current may be put, probably, after the electric light, electric traction has most public interest. The English are a commercial people, but they are also a humane people; and when, as in this case, their pockets and their feelings are alike touched, surely they will be Radicals in welcoming electric traction, whatever may be their political sentiments on other burning topics of the day. It is not a nice thing to feel that you are helping to reduce the life of a pair of poor tramway horses to three or four years: it would be a very nice thing to be carried in a tramcar for even a less fare than at present. Now, while it costs 6*d*. or 7*d*. to run a car one mile with horses, it only costs 3*d*. or 4*d*. to propel it electrically. Indeed, from the very minute details that have recently been published of the four months' expenses of electrically propelling thirty cars at $7\frac{1}{2}$ miles an hour along a 12-miles tramway line in Richmond, Virginia, it would appear that the total cost—inclusive of coal, oil, water, engineers, firemen, electricians, mechanicians, dynamo and motor repairers, inspectors, linemen, cleaners, lighting, depreciation on engine, boiler, cars, dynamos, and line-work—has been only $1\frac{1}{4}$*d*. per car per mile. This is indeed a low price; let us hope that it is true. The tramway is, no doubt, particularly favourable for propelling cars on the *parallel* system (that is, the system in which the current produced by the dynamo is the sum of the currents going through all the motors on the cars) without a great waste of power being produced by a very large current having to be sent a very long distance, because the tramway track is very curved, and the dynamo is placed at the centre of the curve, with feeding-wires to convey the current from the dynamo to all parts of the track. But even in the case of a straight tramway line with a dynamo only at one end, it is quite possible to obtain the same high economy in working by employing a *large* potential difference and by sending a small current through all the trains in *series*, instead of running the trains in *parallel*, as is done on the Portrush, Blackpool, Brighton, and Bessbrook tramways.

This *series* system of propelling electric trains was oddly enough entirely ignored in all the discussions that have taken place this year at the Institution of Civil Engineers, and at the Institution of Mechanical Engineers, regarding the relative cost of working tramways by horses, by a moving rope, and by electricity; and yet this series system is actually at work in America, as you will see from an instantaneous photograph which I will now project on the screen, of a series electric tramway in Denver, Colorado; and a series electric tramway 12 miles long, on which forty cars are to be run, is in course of construction in Columbus, Ohio. The first track on which electric trams were run in *series* was the experimental *telpher line*, erected in Glynde in 1883 under the superintendence of the late Prof. Fleeming Jenkin, Prof. Perry, and myself, for the automatic electric transport of goods. A photograph of this actual line is now projected on the screen. The large wall diagram shows symbolically, in the crudest form, our plan of series working: the current follows a zigzag path through the contact pieces, and when a train enters any section the contact piece is automatically removed, and the current now passes through the motor on that train, instead of through the contact piece. The Series Electrical Traction Syndicate, whom we have to thank for the model *series* tramway on which the two cars are now running, are now developing our idea, but it has received its greater development in the States, where the Americans are employing it, instead of spending time proving, *a priori*, that the automatic contact arrangements could never work. Mental inertia, like mechanical inertia, may be defined in two ways. Inertia is the resistance to motion—that is the English definition: but inertia is also the resistance to stopping—that is the American definition.

In addition to the small waste of power, and consequent diminished cost of constructing the conductors that lead the current into and out of the passing trains, the series system has another very marked advantage. Some years ago we pointed out that when an electric train was running down hill, or when it was desired to stop the train, there was no necessity to apply a brake and waste the energy of the moving train in friction, because the electric motor could by turning a handle be converted into a dynamo, and the train could be slowed or stopped by its energy being given up to all the other trains running on the same railway, so that the trains going down hill helped the trains going up hill, the stopping trains helped the starting trains. At that time we suggested detailed methods for carrying out this economical mutual aid arrangement whether the trains were running on the parallel or on the series system. But there is this difference, that, whereas on the parallel system it is only when a train is running fairly fast that it can help other trains, the series system has the advantage that, when a motor is temporarily converted into a dynamo by the reversal of the connections of its stationary magnet, the slowing train can help all the other trains even to the very last rotation of its wheels. Brakes that save the power instead of wasting it are of purely English extraction, but their conception has recently come across the Atlantic with such a strong Yankee accent that it might pass for having been born and bred in the States.

Economy is one feature that gives electric traction the right to claim your attention; safety is another. This model telpher line worked on "the post head contact" system is so arranged that no two trains ever run into one another, for, in addition to each of the three trains being provided with an automatic governor which cuts off electric power from a train when that train is going too fast, the line is divided into five sections connected together electrically in such a way that as long as a train is on any section, A, no power is provided to the section B behind, so that if a train comes into section B, it cannot move on as long as the train in front is on section A. [Three trains shown running on a model telpher line with four automatic locks.] Whenever a train—it may be even a runaway electric locomotive—enters a blocked section, it finds all motive power withdrawn from it quite independently of the action of signalmen, guard, or engine-driver, even if either of the latter two men accompanied the train, which they do not in the case of telpherage: no fog, nor colour-blindness, nor different codes of signals on different lines, nor mistakes arising from the exhausted nervous condition of overworked signalmen, can with our system produce a collision. Human fallibility, in fact, is eliminated. While the ordinary system of blocking means merely giving an order to stop—and whether this is understood or intelligently carried out is only settled by the happening or non-happening of a subsequent collision—our automatic block acts as if the steam were automatically cut off; nay, it does more than this: it acts as if the fires were put out in an ordinary locomotive and all the coal taken away, since it is quite out of the power of the engine-driver to re-start the electric train until the one in front is at a safe distance ahead.

The photograph now seen on the screen shows the general appearance of the Glynde telpher line, which has recently been much extended in length by its owners, the Sussex Portland Cement Company; and a telpher line with automatic blocking on the broad principles I have described is about to be constructed between the East Pool tin-mine in Cornwall and the stamps. There will be four trains running, each consisting of thirty-three skeps containing three hundredweight each, so that the load carried by each train will be about five tons.

It may be interesting to mention that the last difficulty in telpherage, which consisted in getting a proper adhesion between the driving-wheels of the locomotive and the wire rope, has now been overcome. The history of telpher locomotives is the history of steam locomotives over again, except that we never tried to fit the electric locomotives with legs, as was proposed in the early days for steam locomotives. It is a tedious discouraging history, but it is so easy to be wise when criticizing the past, so difficult to be wise when prospecting the future. Gripping-wheels of all kinds, even the india-rubber tires used for the last three years, have all been abandoned in favour of simple, slightly loose, cheap iron tires, which wear for a very long time, and give a very perfect grip when the bar supporting the electromotor is so pivoted, pendulum-wise, to the framework of the locomotive that the weight of the motor no longer makes the locomotive jump in passing the posts, as it did until quite recently.

After several years of experimenting, we have in telpherage, I venture to think, at last a perfectly trustworthy, and at the same time a most economical, method of utilizing distant steam- or water-power to automatically transport our goods, and in time it may even be our people, over hills and valleys, without roads or bridges, and without interfering with the crops or the cattle, or the uses to which the land may be put over which the telpher trains pursue their snake-like way: we have, in fact, the luxury of ballooning, without its dangers.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 17.—M. Des Cloizeaux in the chair.—Complement to the theory of overfalls stretching right across the bed of a water-course (weirs, mill-races, and the like), by M. J. Boussinesq. In supplement to the theory worked out in the *Comptes rendus* of July 4, October 10 and 24, 1887, the author here deals with the discharge as influenced by the velocities of the currents at the overfall.—On M. Lévy's recent communication on the subject of Betti's theorem, by M. E. Cesaro. This theorem, which plays an essential part in Betti's "Teoria dell' Elasticità," is practically that of Green, which is capable of such manifold applications, and which M. Lévy has shown to admit of so many interesting corollaries in graphostatics. In the present paper M. Cesaro confines himself to proving that the formula of Laplace, giving the velocity of sound in rectilinear elastic mediums, is itself a consequence of Betti's fruitful theorem.—Compressibility of the gases, by M. E. H. Amagat.—On the chlorides of gallium, and on the value of the elements of the aluminium group, by MM. Nilsson and Otto Pettersson. Here are studied the two different chlorides Ga_2Cl_6 (or $GaCl_3$) and $GaCl_2$, as determined by M. Lecoq de Boisbaudran, the discoverer of gallium. The combinations are also given that are formed with chlorine by the elements of the third group of the natural system, whose chlorides have so far been studied. It is pointed out that aluminium and gallium displace three atoms, indium two, and thallium one of hydrogen of the hydrochloric gas. In this group, with the increase of the atomic weight the elements show an evident tendency to form several combinations with chlorine.—On ferrous chloride and the chlorides of chromium, by MM. Nilsson and Otto Pettersson. The preparation and properties are described of ferrous chloride, and of the two known chromium chlorides—the trichloride, $CrCl_3$, and the bichloride, $CrCl_2$.—Papers were communicated by M. René Chevrel on the great sympathetic nervous system of bony fishes; by M. Alexandre Vitzou on the incomplete intercrossing of the nerve-fibres in the optic chiasma of the dog; and by MM. Raphaël Dubois and Léo Vignon on the physiological action of para- and metaphenylene-diamine.

CONTENTS.

THURSDAY, OCTOBER 4, 1888.

DETERMINANTS.

Teoría Elemental de las Determinantes y sus principales aplicaciones al Algebra y la Geometría. Por Félix Amorétti y Carlos M. Morales. (Buenos Ayres: Imprenta de M. Biedma, 1888.)

"SOME books," says Bacon, "may be read by deputy, and extracts made of them by others;" and, at any rate so far as English readers are concerned, the work now under review belongs to this category. A very considerable portion of it is taken up with translations of selected passages from Muir's "Treatise on the Theory of Determinants," of which the following is a sample:—

"Teorema III.—*Toda determinante centrosimétrica del orden* 2m*ésimo* *es igual á la diferencia de los cuadrados de dos sumas de determinantes menores del orden* m*ésimo* *formadas con las* m *primeras filas.*"

"En efecto; el producto de las dos determinantes factores, por ejemplo, D y D′, en el caso del párrafo 37, es igual á

$$\tfrac{1}{2}\{[D + D']^2 - \tfrac{1}{2}[D - D']^2\},$$

y si D y D′ se desarrollan en función de determinantes de elementos monomios (22), las determinantes de una de las expresiones son iguales á las de la otra. Luego queda demostrado el teorema."

The above is almost word for word the same as § 138 of Muir's "Treatise," which we subjoin for the sake of comparing the translation with the original:—

"*A centro-symmetric determinant of the* $2m^{th}$ *order is expressible as the difference of the squares of two sums of minors of the* m^{th} *order formed from the first* m *rows.*

"The product of the two factors, D and D′ say, in the first case of § 137 is equal to

$$\{\tfrac{1}{2}(D + D')\}^2 - \{\tfrac{1}{2}(D - D')\}^2,$$

and when D and D′ are expanded (§ 29) in terms of determinants with monomial elements, the determinants in the one expansion are in magnitude the same as those in the other: hence the theorem."

We notice that Muir's formula is incorrectly printed in the translation; but it is only fair to add that such inaccuracies are rarely met with in the volume before us, which is more free from misprints than the first editions of mathematical books usually are. The translators do not appear to have caught the exact meaning of the words "are in magnitude the same as," which they have changed into "son iguales á." Quantities which are the same in magnitude (though differing, it may be, in sign) they call equal, and are consequently forced to translate the words "are equal" by "son iguales y del mismo signo" as they have done elsewhere in more than one place. But a much worse mistranslation (also from Muir) occurs on p. 85, where the single word "es" is used as the equivalent of "contains the term." A worse mistake than this one could not have been committed, even by those who, according to Hudibras, "translate,—

Though out of languages, in which
They understand no part of speech."

The above extract is taken from the second of the three distinct portions, or books, into which the "Teoría Elemental" is divided. The first of these books has to do with determinants in general, the second (consisting mainly of translations from Muir) treats of determinants of special form, and the third is reserved for algebraical and geometrical applications. The nomenclature adopted in the second book differs in some particulars from that employed by Muir. Thus our authors do not follow him in substituting "adjugate" for the more euphonious and more familiar adjective "reciprocal," and they agree with Scott and others in calling those determinants "orthosymmetrical" which Muir names "persymmetric." We think that their name "determinante *hemisimétrica*" is a distinct improvement on the old "*zero-axial skew* determinant," but we cannot see any special reason for speaking of determinants in which all the elements in one row are equal to unity as "determinantes multiples," and we do not consider that the fact of the equality of all the elements in the principal diagonal of any skew determinant is of sufficient importance to necessitate the use of the distinctive appellation "*pseudo-simétrica*" to denote such a skew determinant.

The second book contains most of the principal properties of the various kinds of symmetrical determinants, and of Pfaffians, alternants, circulants, and continuants, but not of compound or functional determinants: these are mentioned, but their properties are not investigated. The short chapter devoted to them merely defines compound determinants, Jacobians, Hessians, and Wrouskians, and then concludes abruptly with these words: "Por más interesantes que sean estas formas, la índole de esta obra no permite entrar en el estudio de ellas, para el cual se recomienda especialmente el notable tratado del profesor R. Scott, 'Determinants,' Cambridge, 1880."

Here our remarks on the second book (which finishes with this sentence) would come to a close if we did not wish to correct a mistake into which the authors have fallen as to the origin of the name *continuants*. These they say (see p. 112) "se denominan continuantes, por sugestión del profesor Sylvester." The real facts of the case are these. Prof. Sylvester was the first to discover the forms called *continuants*, to which he gave the name of *cumulants*. It was Muir who suggested the name *continuant* "as an exceedingly suitable and euphonious abbreviation for *continued-fraction determinant*," and as a "short literal translation of the equivalent term *Kettenbruch-Determinante*, which is the received name in Germany" (vide *American Journal of Mathematics*, vol. i. p. 344: letter from Mr. Muir to Prof. Sylvester on the word continuant, September 4, 1878).

Of the third book we have very little to say. It is nice easy reading for young beginners, and teaches them how to solve systems of linear equations, how to perform eliminations by means of Euler's, Bezout's as modified by Cauchy, or Sylvester's dialytic method, and how to calculate the roots common to two equations or the double roots of a single equation. There is a short chapter in which some of the most simple properties of the resultant of two equations are explained. The last chapter in the book is the only geometrical one; its principal contents are determinant expressions for the area of a triangle, a quadrilateral, and a polygon, in terms of the co-ordinates of their respective vertices, and some simple trigonometrical formulæ. On p. 173, in this chapter, we notice a curious misprint: in each of three successive formulæ (the usual expressions for the sine, cosine, and tangent of half an angle of a triangle in terms of its sides) a capital V takes the place of the sign of the square root.

The opening paragraph of the first book tells us of the origin of determinants, citing as evidence of their invention by Leibnitz his celebrated letter to L'Hopital, dated April 28, 1693. Their re-discovery by Cramer in 1750, and the rule (for the solution of a system of linear equations) which still bears his name, are next mentioned; but authors of a more modern date are summarily dismissed with the following brief notice:—

"Desde el tiempo de Cramer la teoría de las determinantes ha hecho notables progresos debido á los trabajos de Vandermonde, Laplace, Gauss, Cauchy, Jacobi, Sylvester, Muir, Baltzer y otros, no habiendo rama de las matemáticas en que no haya sido aplicada con ventaja."

We are not, however, left entirely in the dark as to the contributions to the theory made by these writers; for some theorems are called by the names of their respective authors, and a large number of others have these names indicated in brackets. For instance, the proposition which concludes the third chapter in the first book is thus enunciated:—

"Descomponer una determinante de orden $n^{\text{ésimo}}$ en una suma de productos formados cada uno de una determinante de orden $p^{\text{ésimo}}$ y de una determinante de orden $[n-p]^{\text{ésimo}}$ { Laplace }."

This is immediately preceded by—

"Teorema de Cauchy.—Si se elige una fila y una columna de una determinante cualquiera, el elemento común de ellas multiplicado por el respectivo complemento algebraico, más la suma de productos obtenidos multiplicando el producto de un elemento de la fila y de la columna por su respectivo complemento algebraico, es equivalente á la determinante dada."

The way in which these two propositions are treated in the present work will serve to exemplify the methods employed by its compilers for imparting knowledge to their readers. The proof of Laplace's theorem given by Scott, in § 5, chap. iii. of his "Determinants," is clearer than any other we are acquainted with; but it depends on some of the properties of alternate numbers. It is true that these properties are of the simplest kind, but then the notion of alternate numbers is a highly abstract one, quite as much so as the idea of a four-dimensional space. In order, therefore, to convey a clear conception of Laplace's theorem to students of average capacity, our authors have turned it into a problem, and, by considering what Prof. Sylvester calls a simple diagrammatic case, have shown how this problem can be solved, thereby bringing the *theorem* within the grasp of those whose minds are as yet unprepared to revel luxuriously in such abstractions as the alternate numbers.

On the other hand, the proof of Cauchy's theorem and the illustrative example appended to it have been reproduced, with only some slight vertal alterations, from § 62 of Muir's "Determinants," where the theorem in question is presented in a form eminently adapted for elementary instruction.

The first book ends with a rule for the division of determinants, which may be briefly stated thus: To divide $|a_{1n}|$ by $|b_{1n}|$, assume the quotient to be $|x_{1n}|$ and equate each element of the determinant formed by multiplying $|x_{1n}|$ and $|b_{1n}|$ to the corresponding element of $|a_{1n}|$.

The values of the elements $x_{11}, x_{21}, \ldots . x_{nn}$, of the assumed quotient $|x_{1n}|$ will then be determined by solving a system of equations of the form

$$b_{1n}\, x_{1n} + b_{2n}\, x_{2n} + \ldots . + b_{nn}\, x_{nn} = a_{nn}.$$

The article containing this rule should be expunged from all future editions of the work. Its practical inutility becomes apparent when we remember that, on solving the system of equations to which it leads, each x is found in the form of the quotient of two determinants; so that we have to perform many divisions instead of one. Those who are practically engaged in the work of mathematical tuition in the University of Buenos Ayres will doubtless be able to suggest other improvements, and if these suggestions are attended to, students in that University will possess in the second edition of the "Teoría Elemental" an introduction to the theory of determinants written in their own language and suited to their requirements.

In some respects we do not desire to see any improvement. The appearance of the book is as attractive as good paper, wide margins, and a bold clear type can make it. The authors have chosen for their motto the appropriate quotation from Sylvester: "For what is the theory of determinants? It is an algebra upon algebra; a calculus which enables us to combine and foretell the results of algebraical operations, in the same way as algebra enables us to dispense with the performance of the special operations of arithmetic." The table of contents is a model of completeness, and gives the enunciations of the theorems in full instead of merely indicating the pages and articles in which they occur. The volume ends with a selected list of treatises on determinants "que pueden servir de texto y que son dignas de especial mención." This will be of use to students who only want to be told what authors they should read, for the names mentioned are few and well chosen; while those whose object is to improve their acquaintance with the bibliography of determinants may fully satisfy their desire by consulting the two papers by Muir in the *Quarterly Journal of Mathematics* (one of them published in 1881, the other in 1886) to which reference is made.

Responding to the invitation—"agradeceríamos las indicaciones que se nos hicieran sobre omisiones ó errores que no hubiéramos advertido"—we call attention to a slight misprint in this reference, in which the word "Quarterly" has been mis-spelt "Quaterly." With the exception of those previously mentioned, no other *erratum* has come under our notice.

OUR BOOK SHELF.

The Geological History of Plants. By Sir J. W. Dawson, C.M.G., LL.D., F.R.S., &c. 8vo, pp. 290. With Illustrations. "International Scientific Series." (London: Kegan Paul, Trench, and Co., 1888.)

THIS book gives, in a connected form, a summary of the development of the vegetable kingdom in geological time.

Though likely to be of use to geologists and botanists, the treatment is sufficiently popular to be intelligible to the general reader. The floras of the successive geological formations are treated of in turn, from the oldest rocks down to comparatively recent times. The two longest chapters in the book are devoted to the vegetation of the Devonian and Carboniferous ages respectively, much of the matter here traversed having formed the subject of numerous scientific memoirs by the author. In the body of the work, accounts of the morphology and minute anatomy of the various plant-remains are given, with speculations as to their affinities, and in many cases restorations are attempted, illustrated by figures. The more special details as to classification, &c., are wisely placed in small type as a series of notes at the end of each chapter. The last chapter in the book consists of an interesting essay on the general laws of origin and migrations of plants. Many of the woodcuts leave much to be desired, more especially those dealing with histological subjects. These are, for the most part, scrappy and insufficiently described, and convey little to the mind. Comparisons between fossil remains and recent plants are often rendered valueless by strange inaccuracies as to the morphological value of the parts so compared. Thus the leaves of *Marsilea* (pp. 60 and 67) are described as being in whorls and cuneate in form, and in *Azolla* and *Salvinia* the leaves are "frondose and more or less pinnate in their arrangement." *Spenophyllum*, which possesses wedge-shaped leaves arranged in verticels on the stem, is set down as of probable Rhizocarpian affinity, on this mistaken comparison between its *leaves* and the *leaflets* of *Marsilea!* Much confusion also arises from a careless use of the terms sporocarp, sporangium, macro- and micro-spore, antheridium, &c., in connection with certain small bodies found in the Erian and Carboniferous beds, and conceived by the author to be the reproductive bodies of a rich, then-existing Rhizocarpian flora. Though there are many points in which palæobotanists may not be at one with the author—such as the reference of so many Palæozoic forms to Rhizocarps—the volume will be of service, especially to those to whom the larger treatises are not available.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Prophetic Germs.

I HAVE but just returned from abroad, and have hastened to read the number of NATURE for August 30. I find that the Duke of Argyll in his letter of that date makes some remarks which call for a few words from me. The Duke is not, it appears, prepared to defend the theory that the electric organ of *Raia radiata* is a "prophetic germ." He refers me to the paper of Prof. Ewart on this subject, whose opinion he quotes and accepts. I am not sure how far Prof. Ewart himself had considered the significance of the view which he put forward in regard to the nature of the rudimentary electric organs of skates; but I do not hesitate to say that there are no facts which have been made known at present, either by earlier observers or by Prof. Ewart, with regard to the electric organ of skates, which necessitate such a theory of prophetic germs as that imagined by the Duke of Argyll, or which can be shown to be inconsistent with the doctrine of progressive development by the natural selection of fortuitous congenital variations. If the Duke of Argyll will point out such facts, he will have made a contribution of some value towards the understanding of the laws of organic evolution.

In a subsequent portion of his letter the Duke of Argyll states: "If Prof. Ray Lankester will explain how 'natural selection' can act upon 'congenital variations,' which he calls 'non-significant'—*i.e.* which are not yet of any actual use—and if he will explain how this action can afford 'the single and sufficient theory of the origin' of (as yet) useless variations, he will have accomplished a great triumph in logic and philosophy."

I am unwilling to entertain the notion that the Duke of Argyll has intentionally constructed the above sentence by garbled quotations from my previous letter in order to produce the false impression that I have maintained such a view as to the action of natural selection. At the same time, I will observe that the method of discussion adopted by the Duke—namely, that of half-quoting the opinion which he attributes to an opponent and desires to render illogical in the judgment of others—is, to say the least of it, objectionable. It becomes easy when this method of partial re-statement is adopted for the disputant to insert words of his own mixed with the words of his opponent, and thus to misrepresent the latter's statement by unconsciously fabricating what the poet has condemned as the worst of fabrications—namely, one which is half a truth.

The point of the sentence which I have above quoted from the Duke of Argyll's letter depends upon the unwarrantable introduction on his part after the quotation of the word "non-significant" of certain words in explanation of that word. The Duke is kind enough to say that by "non-significant" I mean "which are not yet of any actual use." I have not had any private communications with the Duke of Argyll upon this matter, and am at a loss to understand how he should have come to think that he knows that this was what I meant by the word "non-significant." By whatever process he arrived at that conclusion I regret to have to say that it is absolutely erroneous. My meaning was nothing of the kind, and I was under the impression that I had stated with sufficient clearness what my meaning was. It appears that I did not state it clearly enough for all readers. I called the congenital variation which survives in the struggle for existence "non-significant" in regard to its origin and not in regard to its survival. It was, I think, clear to most readers that I was distinguishing between the Lamarckian theory of variation as due to the transmission of parental acquired characters and the Darwinian theory of variation as due to a "shaking up" of the germ-plasma at the union of egg-cell and sperm-cell. The variation—that is, the departure of a young animal or plant from the normal character of the species—would be, if it could be traced to the transmission from a parent of a character acquired by that parent in adaptation to the environment, significant; that is to say, it would have significance for the adjustment of the species in its very origin in the parent. On the other hand, the thousand and one slight or considerable departures from the mean specific form which occur in every possible direction in a brood of young fish or other organisms are "non-significant." They are due to a long-precedent disturbance of the germ-plasma when the form of the organism was undeveloped. No possible reaction of adjustment can be imagined which could produce adaptation in the structure of an animal or plant developed from a germ, if it be a proviso that such adaptation is to have relation to a physical cause of disturbance which once acted upon the germ whilst the adaptational results are to come into effective existence in the developed product of the germ. Hence I am led to speak of congenital variations as "non-significant" in relation to the disturbing causes which produce them.

The proposition that congenital variations are selected when they are not yet of any actual use is an absurdity which the Duke of Argyll had no justification whatever for suggesting as likely to be defended by me, and one which he arrives at by misrepresenting the meaning of the adjective "non-significant." As a matter of course, some one combination of congenital variations is "significant" in the sense which the Duke of Argyll chooses to give to that word—a sense in which I do not employ it: some one combination of congenital variations in each generation survives because it is "significant" in the sense of being useful. It is a common fallacy to suppose that natural selection is only operative in producing *new* species; on the contrary, it is never in abeyance, but is equally as active in maintaining an existing form as in producing a new one.

With regard to the origin of useless variations and the general question of uselessness, it is not to be expected that your columns should be given over to an exposition of the common-places of Darwinism. It is to be noted, firstly, that we have no right to conclude that a structure is useless to the organism in which it occurs because the Duke of Argyll is unable to see in what way it is useful; secondly, we have established the great principle of "correlation of growth," which is a brief way of stating that in organisms there is such an intricate binding together of the mechanism that when one part varies other parts vary concomitantly—so that a useful variation of the beak or eyelid of a bird (for example) may *necessitate* a concomitant and perfectly useless variation in the toe-nails or the tail-feathers; thirdly, useless structures undoubtedly exist owing to the potency of heredity, which is of such strength that long after a structure has ceased to be a matter of selection it is transmitted from generation to generation, though dwindled in size and more or less imperfect in structure.

I think there will be no difficulty by reference to one or other of the three considerations above stated in disposing of cases of so-called "uselessness," or "prophetic" functionless organs "on the way to use," which the Duke of Argyll may find to be stumbling-blocks in the way of his faith in Darwin, if he will submit them one by one for pulverization, though I am afraid the process will not interest your readers.

September 21. E. RAY LANKESTER.

A Shadow and Halo.

A FEW evenings ago, whilst walking down a sloping pasture, with the moon shining brightly at an altitude of about 20° behind me, and with no visible dew nor fog, yet with heavy dew on the grass, I noticed that the shadow of my head and shoulders was very sharply defined, but that it was surrounded by a halo of light, and that this halo or nimbus increased in brightness as my shadow was lengthened out because of the increasing slope; and not only was the brightness increased, but it extended now to my hips. That this was due to the greater depth of moist air through which the moon's light passed, by reason of the increase of the slope, I think was proved by the fact that in the neighbourhood of a high hedge, which would to some extent alter the conditions, this halo nearly wholly disappeared. At one time I thought that my eyes were deceiving me concerning this appearance, the contrast of the dark shadow with the surrounding brightly illuminated grass giving rise to the appearance above mentioned, but, by holding up my hand so as to cut off the view of the shadow, I still saw the brighter light which surrounded it, and this brightness still increased or decreased in intensity as the slope on which I took up my position was greater or less. There was no casting of a shadow on a fog-bank, as there was no fog at all, but rather the air was particularly clear. I noticed this phenomenon three nights in succession. I shall be glad to know if any other amongst your readers has noticed this occurrence, and will explain it. E. W. P.

Tamworth, September 29.

Sonorous Sands.

REFERRING to Mr. Carus-Wilson's letter recording the supposed discovery of musical sand in Dorsetshire, I may mention that about two years ago the late Admiral E. J. Bedford sent me three boxes of musical sand, one of them being labelled, "Musical sand; Studland Bay, Dorset, 1885; sonorous when collected." I am not aware whether Admiral Bedford himself discovered the sonorous properties of this sand, but it is clear that he was well acquainted with both the sand and its character in 1885. A. R. HUNT.

Torquay, September 27.

THE REPORT OF THE KRAKATÃO COMMITTEE OF THE ROYAL SOCIETY.

I.

AFTER an interval which has been prolonged partly by the unexpected continuance of the subsequent atmospheric phenomena, and partly through other circumstances incidental to publishing, the Report on the great eruption of the volcanic island of Krakatão in August 1883 is now before the world.

Every Committee is bound to issue a Report of some kind, but it rarely falls to the lot of a Committee to deal with anything at once so stupendous in its character and far-reaching in its consequences as the eruption which not only figuratively, but literally, vibrated through the world on August 27, 1883.

We, in these islands, may boast of our Essex earthquakes, and of the frequent little tremors to which a certain district in Perthshire is subject; but few of us, or our immediate neighbours, can, from our local experience, form the faintest conception of the terrific subterranean powers which ordinarily manifested themselves in the volcanic region of which Krakatão may be fitly termed the *focus*.

The first accounts which reached us by telegram, inaccurate though they were bound to be as regarded details, were scarcely exaggerations in point of magnitude; and, indeed, the cataclysm in this case rose superior to all artificial modes of transmission, by announcing the very date and hour, if not minute, of its culminating explosion through a series of air-waves, which recorded themselves no less than four times on every automatically recording barometer throughout the world.

Three other distinct and abnormal phenomena were: (1) the immense distance to which the sound-waves were propagated (altogether transcending anything hitherto on record; (2) the immense local height, destructive power, and subsequent wide diffusion of the accompanying sea-waves, which in this case were *not*, as is usually the case, due to earthquake action; (3) the simultaneous occurrence in the Javan and Indian area, and subsequently rapid extension, first round the equatorial zone, and, finally, to the whole world, of a most remarkable group of optical phenomena, including coloured suns, lurid and prolonged glows at twilight, large coronæ round the sun and moon, and a peculiar cirriform haze which was evidently connected in some way with these and the eruption.

It was plain, in the face of these preliminary facts, that the collection and discussion of such a grand series of exceptional phenomena gratuitously evolved out of Nature's own laboratory, could not fail to be of service to science, and that while the more local features and practical results of the episode might be left to the Dutch Government, to whom the district belonged, its attendant and subsequent phenomena deserved permanent record in the pages of scientific history.

On this basis, a Committee of the Royal Society was appointed on January 17, 1884, in the following terms:—

"That a Committee, to consist of Sir F. Evans, Prof. Judd, Mr. Norman Lockyer, Mr. R. H. Scott, General Strachey, and Mr. G. J. Symons, with power to add to their number, be appointed to collect the various accounts of the volcanic eruption at Krakatão and attendant phenomena in such form as shall best provide for their preservation and promote their usefulness."

The subsequent expansion of the Committee by co-operation of additional members, and the substitution of one—Captain Wharton—in consequence of the death of Sir Frederick Evans, is detailed in the preface.

The main object of the Committee was thus to collect facts and reduce them into a systematic and useful form. While this has been its primary object, it has been thought advisable to enlarge upon the original basis of the Report, and, while giving a *résumé* of all the leading opinions, especially those relating to the debated question of the relation of the optical phenomena to the eruption, to enter at some length into a discussion of the facts thus systematized. Though it is hardly to be expected that everybody will agree with the deductions arrived at by each author, and though it has been impossible to avoid omissions in a work embracing, in its latter sections, observations extending over three years, and a literature of its own, the main facts have not only been recorded, but, as the Chairman, Mr. G. J. Symons, says, can be readily verified.

Although, therefore, as time progresses, and human knowledge changes and enlarges, some of the conclusions drawn by the authors of the Report may be modified or reversed, the value and permanence of the facts and opinions quoted, will be secured by the unusual care which Mr. Symons has taken to verify all the references and quotations.

The work is divided into five parts:—

I. On the volcanic phenomena of the eruption, including the nature and distribution of the ejecta, by Prof. Judd, F.R S.

II. On the air-waves and sounds caused by the eruption, prepared under the direction of Lieut.-General R. Strachey, F.R.S.

III. On the seismic sea-waves caused by the eruption, by Captain W. J. L. Wharton, R.N., F.R.S.

IV. On the unusual optical phenomena in the atmosphere which began in 1883 and continued in part up to 1886 inclusive, and which included coloured suns, twilight effects, coronal appearances round sun and moon, sky haze, &c., by the Hon. F. A. Rollo Russell and Mr. E. Douglas Archibald. And

V. A short discussion of the magnetical and electrical phenomena, by Mr. G. M. Whipple.

Prof. Judd commences by pointing out how peculiarly favourable for the gigantic outburst was the position occupied by Krakatão. The marked linear arrangement of the volcanoes of Java and Sumatra points to the existence of a corresponding great fissure in the earth's crust; while across the Straits of Sunda lies another line of weakness, along which five volcanoes have been thrown up at different epochs. Krakatão lies precisely at the intersection of these lines. It is therefore a position where volcanic action, once having commenced, might be expected to display itself on its grandest and most intense scale.

The history of Krakatão, as traced by Prof. Judd, shows that, both in dimensions and activity, it may be considered to have been one of the largest and most destructive volcanic craters in the world. At one time, "its circumference, at what is now the sea-level, could not have been much less than twenty-five miles, and its height above the same datum plane was perhaps not less than 10,000 to 12,000 feet."

Then, at some unknown period, a terrible outburst seems to have occurred, far transcending the present one, which completely eviscerated the volcano, and reduced it to the condition of a basal wreck of three islands, one of which contained Rakata, a basaltic lava cone from which the island derived its name, and two smaller parasitical cones; while the other two represented the relics of the original crater, formed of the same material as the latter, viz. enstatite dacite. The relatively inconspicuous character of Rakata, and the adjacent cones and islets, as well as the absence of any serious volcanic action since 1680, seem to have warded off any suspicions which might have been entertained by the inhabitants on the adjacent coasts regarding either the former grandeur of the volcano or the possible renewal of its activity, certainly on such a scale as was witnessed on August 27, 1883.

Nature, however, rarely displays its grandest effects without giving premonitory warnings, and, in volcanic and seismic phenomena more particularly, by exhibiting the culminating outburst as the cumulative result of an aggregation of small and continuously operating hypogenic causes.

For some years prior to 1883, earthquakes had been of frequent occurrence in the vicinity, one of which destroyed the lighthouse on Java's First Point, and was felt even in North Australia, while on May 20 and 21 an eruption proceeded from Perboewatan, the most northern of the three craters which occupied the place of the original prehistoric volcano, and the same that was in eruption in 1680. This eruption, though only of a relatively mild (Strombolian) type, compared with its successor, was yet sufficiently striking to be accountable for some of the sporadic sky effects which, as we shall see, were noticed in its vicinity during and for some little time after its occurrence. For example, the captain of the German ship *Elizabeth*, when passing through the Straits on May 20, observed the height of the smoke column as it issued from the volcano to be over 30,000 feet, and found dust fall on his ship when it was more than 300 miles distant; while, according to Verbeek, the writer of the Dutch Report, the sounds were heard not merely at Batavia and Buitenzorg, 100 miles off, but even at Singapore, which is 518 miles away.

After this relatively minor, though absolutely violent eruption, a period of intermittent and subordinate activity prevailed, during which two other dormant cones reopened, the decrease in violence being thus probably made up for by the larger area in eruption. Finally, after a period of growing intensity—a fact which was attested by observations at Batavia and on board ships passing through the Straits—the entire volcano appears, on August 26, to have passed from the moderate (Strombolian) stage to the paroxysmal (Vesuvian) stage.

It would be unnecessary to recapitulate the accounts given of this terrific outburst, which lasted from 2 p.m. on Sunday, August 26, to the evening of August 27, and reached its culmination at about 10 o'clock on the latter day. The originals read like romances from the "Arabian Nights," though to attempt to adequately describe such a chaos would need the pen of a Dante coupled with the pencil of a Doré. The salient features were (1) the unusual height to which the smoke column was observed to ascend, viz. seventeen miles, by Captain Thomson, of the *Medea*—the nearest approach to which on any former occasion seems to have been thirteen miles at the eruption of Graham's Island (Julia) in 1831; (2) the extraordinary violence of the detonations; and (3) the accompanying atmospheric and electric phenomena. With respect to this latter point, the volcano was, in fact, a frictional hydro-electric generator of electricity on the largest possible scale.

One of the most important deductions arrived at by Prof. Judd from a study of this and other eruptions is the precise part played by water in aiding eruption.

It appears to be often thought that both slow percolation and the rapid introduction of water into reservoirs of lava are the direct causes of eruption; but Prof. Judd shows that, while the percolation of water is one of the contributory causes, it is not *the* primary cause, which he attributes, when discussing the nature of the materials ejected, to "the disengagement [by heat] of volatile substances actually contained in those materials."

According to this, which may be termed the "cartridge" doctrine of eruption (the lava representing both the powder and shot), the action of inrushes of sea-water, such as occurred in the present case, by chilling the surface of the lava, and augmenting the tension of the imprisoned gases, caused "a check and then a rally," analogous to what occurs in a geyser when sods are thrown into it. Prof. Judd attributes the excessively violent nature of the last stages of the great eruption of Krakatão to this "check and rally action," caused by the dissolution after evisceration of the crateral framework of the volcano, and the consequent admission of the sea in large quantities, a circumstance to which its position rendered it peculiarly accessible.

Prof. Judd considers the "*excessively violent though short paroxysms* with which it terminated" to be the special feature by which the eruption of Krakatão differed from others of similar rank. These, while characterized by a larger quantity of materials ejected, present no parallel to the final "exhaustive explosions of abnormal violence," together with the vast sea and air waves, and the subsequent optical phenomena, which accompanied that of Krakatão.

Prof. Judd next deals with the nature of the materials ejected, and draws attention to the different *physical* characters presented by the lavas ejected from Krakatão at different epochs, the final compact lavas of 1883 being porphyritic pitchstone, and obsidian, containing about 70 per cent. of silica, and so nearly identical *chemically* with those of some of the earlier outpourings as to suggest refusion.

The heavier lava dust which fell in Java and was examined by numerous geologists, including Prof. Judd himself, exhibits a peculiarity which he considers to be without precedent, in that it contains almost every variety of feldspar crystals. The base in which these crystals was found to be embedded presents great differences in its fusibility, the pitchstone melting with great difficulty, and the obsidian with ease. This latter point, in combination with other circumstances, leads Prof. Judd to one of the most important of his conclusions, viz. as to how eruptions come to differ not merely in magnitude but in quality: for example, how a volcano such as Krakatão at one time emits massive and viscous lava-streams as it did in former times; at another, pours forth a more liquid lava; and again, as on this occasion, bursts out with explosive violence into an eruption in which most of the lava is converted into pumice. He considers that the older lavas have been *chemically* acted on by water which has slowly percolated through the crust in the vicinity, and that the new compounds thus produced are not only more readily fusible, but more easily convertible into pumice. Volcanic action is thus concluded to be brought about not directly by the physical action of externally derived water, but by changes in the physical properties of rocks chemically altered through the medium of such water.

In connection with the optical effects which were witnessed subsequent to the eruption, and which are found to be connected chiefly with the finer solid ejecta, Prof. Judd finds evidence, both from a study of the Krakatão pumice as well as the finer dust which fell at great distances, that by the unusual violence of the explosions during the major outburst a large quantity of the very finest threads and dust of volcanic glass was thrown out into the higher atmospheric regions, where it might remain suspended for very long periods. He also points out that the absence of any sign of materials characteristic of Krakatão in the rainfall of distant places is no evidence against their wide diffusion, since the most characteristic substance in the Krakatão dusts was rhombic pyroxene, and this by reason of "its high specific gravity and its slight friability would be among the first to fall."

Prof. Judd brings his section to a close by a general review of the circumstances which have led him to adopt the view already enunciated regarding the cause of volcanic action, viz. that the liquidity of a lava and the violence of an eruption depend on the extent to which the lava has, as it were, been hydrated under the influence of slow aqueous percolation. Lavas of precisely the same composition, and at the same *temperature* may vary greatly in their eruptive action simply by the changes thus effected in their fusion-points. This refined form of the volcanic theory, which is put forward by Prof. Judd, appears to show that the Vesuvian stage of eruption is a paroxysmal form of earth sickness, due to lava gases indirectly generated by water action, while the quiet outpourings both from cones and fissures which have taken place so widely both in the past and present ages, represent the more normal welling up of lava which has been less altered by water action. For this reasonable deduction and the clearer insight afforded into the *modus operandi* of volcanic and seismic phenomena, we are, without doubt, indebted to Krakatão.

(*To be continued.*)

THE BRITISH ASSOCIATION.

SECTION H.[1]

ANTHROPOLOGY.

OPENING ADDRESS BY LIEUTENANT-GENERAL PITT-RIVERS, D.C.L., F.R.S., F.G.S., F.S.A., PRESIDENT OF THE SECTION.

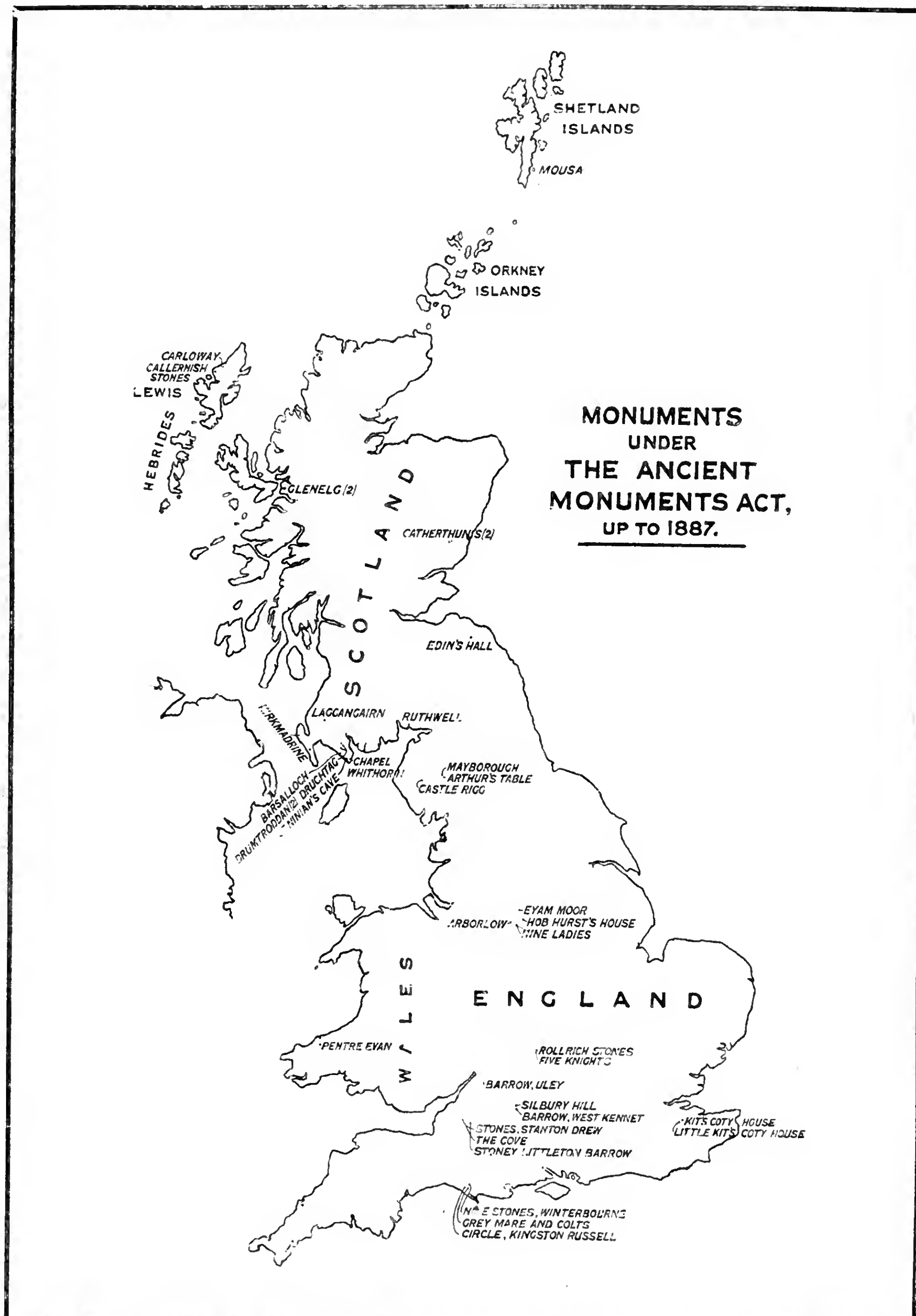
II.

THE accompanying map of Great Britain shows the monuments that I have been the means of obtaining by the consent of their owners.

The Pictish Tower at Mousa in the Shetlands, which is well known to be the best preserved monument of this class in the country, has been included by the owner, Mr. Bruce, and some necessary repairs have been done to it by the Government. In the Orkneys the owners of the scheduled monuments declined to make use of the Act, but they are well looked after. The same applies to the Bass of Inverurie, the Vitrified Fort on the hill of Noath, the Pillar Stones at Newton, in the Garioch, and the British settlement at Harefaulds, in Lauderdale, which latter, however, is in such ruinous condition that the remains of it are scarcely worth preserving. The Suenos Stone near Forres; the Cairns at Clava, on the banks of the Nairn; the Cat-stane at Kirkliston; the Burgh of Clickanim, have also been withheld by their owners, but most of them are very well taken care of. The Cairns at Minnigaff were nearly destroyed before they were scheduled, and are not worth preserving. The inscribed stone in St. Vigean's churchyard is preserved in the porch of the church, but it is not included. On the other hand, Edin's Hall, the largest and most southern of the remains of the Pictish Towers in Berwickshire, has been included by Mr. J. S. Fraser-Tytler; the Black and White Catherthuns have been added by Miss Carnegy Arbuthnot; both these are large camps having ramparts of stones and earthworks round them, and they are described in General Roy's work. The Pictish Towers at Glenelg have been included by Mr. James Bruce Bailey; they are in a very bad state of repair, and have been propped up by the Government. The inscribed stones at Laggangairn, New Luce, have been included by Lord Stair; they are at a great distance from any road or habitation, and the protection afforded them, beyond the powers contained in the Act, must be regarded as nominal. The Peter's stone, on the road from Wigton to Whithorn, has not been added; it is an important stone, and is in a dangerous position; it has already suffered damage, and it is to be hoped it will be included hereafter. The chapel on the Isle of Whithorn, supposed to be that built by St. Ninian, has been included by Mr. R. Johnstone Stewart; this was not in the schedule. The Pillars of Kirkmadrine have been included by Mrs. Ommaney McTaggart; they are the earliest Christian monuments in the country. I suggested that Government should contribute towards building a small chapel to contain them, which has been done. The Cross at Ruthwell, with its remarkable runes, which were gradually being destroyed and covered with lichen, so that its inscription could not be read, has also been added. I suggested that the Government could contribute towards building an annex to the neighbouring church to contain it, which has been done. This was not in the schedule. The cup-marked rock of Drumtrodden, Wigtonshire, has been added by Sir Herbert Maxwell, and Government has granted a certain sum towards building a shed over it to preserve it. It was not in the schedule, but is a good example of its class. Barsalloch Fort, Wigtonshire, the Moat Hill of Druchtag, the Drumtrodden standing stones, Wigtonshire, have also been added by Sir Herbert Maxwell. St. Ninian's Cave, with its early Christian crosses, has been included by Mr. Johnstone Stewart. In the Island of Lewis the remarkable standing stones in the form of a cross at Callernish, and the Broch at Carloway, have been added by Lady Matheson. This latter is, next to Mousa, the best Pictish tower in the country. In Cumberland, the Stone Circle on Castle Rigg has been put under the Act by Miss Edmondson. In Westmoreland, Arthur's Round Table, an earthen circle with a ditch in the interior, and Mayborough, a large circle with an embankment of stones and the remains of a stone circle within, has been included by Lord Brougham. In Derbyshire, Arborlow, a large circle similar to Arthur's Round Table, with the remains of a stone circle, the stones of which are prostrate, and a large tumulus near it, has been added by

[1] Continued from p. 518.

the Duke of Rutland. Hob Hurst's House, and the Circle on Eyam Moor, which also has a large cairn close to it, have been included by the Duke of Devonshire, and the Nine Ladies, a circle of small stones on Stanton Moor, by Major Thornhill. In Gloucestershire, Uleybury, a long barrow with a well-preserved stone chamber, has been added by Colonel Kingscote. In Oxfordshire, the Rollrich stones have been included by Mr. J. Reade. In Kent, Kit's Coty House by Mr. Brassey, which is the remains of a long barrow, the traces of which can be seen, with part of the stone chamber remaining. In Somerset, the



Stone Circles at Stanton Drew, by Mrs. S. B. Coates, and the Cove there by Mr. Fowler; the chambered tumulus at Stoney Littleton by Lord Hylton. It Wiltshire, the long barrow at West Kennet by the Rev. R. M. Ashe, and Silbury Hill by Sir John Lubbock. In Dorsetshire, the chambered long barrow, called the Grey Mare and Colts, near Gorwell, by Mr. A. B. Sheridan; the circle of Nine Stones near Bridehead Park, by Mr. R. Williams; the Stone Circle on Kingston Russell Farm, by the Duke of Bedford; and in Wales the Pentre Evan cromlech, one of the largest in the country, by Lord Kensington—

making in all thirty-six which have been placed under the Act with the consent of their owners. All these and many others have been surveyed; plans, drawings, and sections have been made of them, which are contained in the book now upon the table, which is open for the inspection of the members. I hope to publish these shortly. Besides these monuments which are included under the Act, a good deal of useful work has been done by communicating with the owners of other monuments, without using the Act.

I think it speaks well for the landowners that so many should have been willing to accept the Act, considering that so few of them take much interest in antiquities. There is not a more public-spirited body in the world than the much-abused landowners of England.

Those who have refused have generally done so on the grounds that they wish to remain responsible for their own monuments, and I think I may say, from my own observations, that there is very little damage to prehistoric monuments going on at the present time. Public opinion has done more than any Act of Parliament could do, and it appears to me that it is generally known throughout the country that any wilful damage to the monuments would be universally condemned.

But it is well to consider the operation of the Act, and how it may be improved. The provision which makes it illegal, ever after, to destroy the monuments that are now placed under the Act by their owners, and to enable magistrates to punish offenders summarily, appears to me excellent, and worthy to be retained. But there are defects to which it would be well to give attention. By the present Act, the Government are made responsible for all the monuments that are included, which entails expense; and as members of Parliament generally take very little interest in ancient monuments, and the great object of the Government must always be to curtail expenditure, additions to the list are not as a rule encouraged.

I last year obtained eleven new monuments, but I was told that this was too many, and that some must be omitted, so I selected three of the least important, and they have not been included. This, I think, is objectionable; the two provisions of the Act which I have mentioned should be applied as widely as possible. If the provision making Government responsible for the preservation of the whole of them is altered, there will be no inducement on the part of the authorities to reduce the number to be included. At present local archæologists wash their hands of the matter, thinking that there is a Government Inspector whose business it is to look after the monuments. This is a mistake; the proper function of the Inspector is simply to look after the monuments that are included, and to advise the Commissioners—not to obtain new monuments for the Act. I have done so because I was charged in a special manner with the organization and working of the Act on its first introduction, but it is beyond the proper functions of the Inspector. I have done it as a private individual who takes an interest in the subject, and any other private individual may do the same. Moreover, it is impossible for an Inspector to stand sentry over all the monuments that are put under the Act. The police are requested to look after them as well as they can, but damage must occasionally be done which local archæologists are in a better position to ascertain and to remedy, using the provisions of the Act for the purpose.

It may be that my position as a landowner, as Lord Stalbridge said in his letter asking me to take the appointment, may have had some effect in enabling me to persuade some of the other landowners, but you cannot insure always having a landowner for an Inspector, and it is desirable now to put the Act on a working footing. It is much to be wished that local Archæological Societies should be made to feel themselves responsible both for the inclusion of monuments under the Act, and their preservation afterwards; the Act arms them with full powers for the purpose if they think proper to use it.

At present no Archæological Society has rendered any assistance whatever, but Sir Herbert Maxwell, in Galloway, has not only offered his own monuments, he has persuaded his neighbours to do the same. What Sir Herbert Maxwell can do, others equally public-spirited can do also, if it is clearly understood that it rests with them to take action in the matter, and I think it should rest with them, because, being local, they can do more than a single Inspector charged with the supervision of the whole of the monuments of Great Britain. I think that the Government should continue to appropriate a small sum (it is now under £200 a year) to apply to such purposes as may be thought desirable, such as building sheds to preserve the monuments, but that they should not necessarily be held responsible for all the monuments placed under the Act, and that, the Bill being a permissive one, it should rest with the public to make use of it or not, as they may think proper. If there is no demand for the preservation of monuments, there is no reason why the country should be saddled with the expense of it. If there is a demand, let those who are interested use the law on the subject as they use any other to prosecute delinquents. I think, also, the provision that the new monuments before being included should rest forty days before Parliament might be advantageously abolished. The First Commissioner, with the practical knowledge of the Inspector, is fully competent to decide upon the monuments to be included. It is evident that, if it were desired to save any monument that might be threatened, the forty days would afford ample time to enable the destruction to be carried out before the Act could be applied. With these alterations I think the Act would take root in the country and produce better results. Of one thing, however, I feel certain: that, as long as the owner of a monument takes an interest in it, he is the best person that the public can look to for the preservation of it.

In conclusion it may perhaps interest the meeting if I say a few words upon the results of my recent excavations on the borders of Dorset and Wilts, upon which I have been at work for the last eight years, the detailed account of which is recorded in the two quarto volumes extending to 541 pages and 159 plates, the last of which is just completed.

I have excavated numerous barrows of the Bronze Age near Rushmore, about half-way between Salisbury and Blandford. Winkelbury Camp has been examined and sections cut through the ramparts; an Anglo-Saxon cemetery near it has been dug out, and two Romano-British villages thoroughly explored, the positions of which are shown on the map now exhibited on the walls, a reduced facsimile of which is given on p. 545.

In recording these excavations I have acted on the principle that views upon anthropological subjects are constantly on the change, as our imperfect knowledge of the early inhabitants of the country increases, and that when the records of excavations are confined to opinions and results, it is probable that those facts only which coincide with the theories current at the time receive the prominence they deserve.

The requirements of the future demand that everything should be recorded and tabulated in such a way as to be of easy access hereafter. I have therefore established a system of relic tables in which, without confusing the text and making it unreadable, every object, however small and apparently trivial, is inserted, and the great majority of them are figured in the plates.

It would occupy too much time to enter into details on the present occasion. The result has been to show by a computation from the bones of twenty-eight individuals, found buried in pits in the villages, that the Romanized Britons of this district were an exceedingly small race, having an average stature of not more than 5 feet 2·6 inches for the males, and 4 feet 10·9 inches for the females; that the tallest man was only 5 feet 7·8 inches, and he was an inch and a half taller than the next tallest man.

In head form, the great majority of twenty-six skeletons measured were mesaticephalic and mostly coffin-shaped, but three were hyper-dolichocephalic and two brachycephalic, which shows that the head form approached that of the Neolithic, long-barrow people, with a probable admixture of either Roman or Bronze Age types.

The stature is slightly less than that given by Thurnam for the long-barrow people of this district, but Dr. Garson informs me that, in a paper which he will read at this meeting, he will show that the height of the Romano-Britains whom I have discovered tallies as nearly as possible with that of some long-barrow bones found near Devizes. All are, of course, shorter than the skeletons of the Bronze Age, two of which I have found in the same locality, and which are of the usual tall stature and round-headed types of that people.

The Romano-Britons are also considerably shorter than the skeletons of the Anglo-Saxons found in the cemetery at Winkelbury, which are described in my second volume.

The problem, therefore, with respect to these Romanized Britons appears to be this: Are they the descendants of the long-barrow people, and do they owe their small stature to that circumstance, or is their small size to be attributed to their largest men having been drafted away into the Roman legions abroad?

Prof. Rolleston examined a number of skeletons from a cemetery at Frilford, which he believed to be Romanized Britons, and found that they were of large size, but in my address to the Royal Archæological Institute at Salisbury, last year, I expressed some doubt about the period of these skeletons, and in a paper since published by Dr. Beddoe I see that he rejects the evidence of their being Romano-Britons upon the same ground that I had doubted it, and he quotes Barnard Davies and Thurnam for the occurrence of other skeletons of these people of the same or nearly the same stature as those of the villages that I have explored.

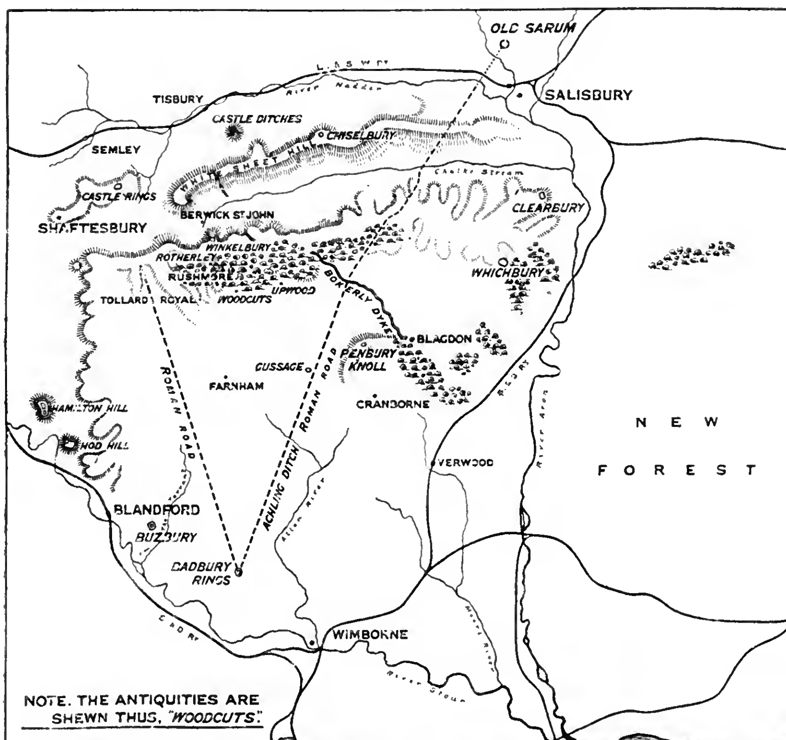
We are therefore evidently beginning to accumulate reliable information about these people, whose physical peculiarities are less known to us than any other prehistoric, or rather non-historic, race that has contributed to the population of this country.

Thurnam shewed that the large-sized, round-headed Belgæ probably penetrated no further westward than the borders of the district I am speaking of, and that the bowl barrows and the long barrows of the Stone Age predominated to the westward of it.

Since the present volume of my excavations was in print, I have quite recently made another discovery of considerable interest bearing upon this question.

Bokerley Dyke is an ancient intrenchment which cuts across the old Roman road from Old Sarum to Badbury Rings. It is an earthwork of considerable magnitude, with a ditch on the north-east side of it. It appears to have originally occupied all the open downland spaces intervening between the ancient woods, which latter probably, by means of felled trees, afforded sufficient defence without earthworks. It extends with its dependencies and detached prolongations more or less all the way from White Sheet Hill, on the north-west, to Blagdon Hill, on the south-east, a distance of about nine miles. Its origin and use have been frequently discussed by archæologists, but no one has hitherto assigned a right date to it. I have now cut two broad sections through it on either side of the Roman road, models of which are exhibited, with the result of proving that it is late Roman, or post-Roman, and is of the same date as the villages; Roman coins, to the amount of 500, of late date, extending to Constantinus and Gratianus, and pottery, having been found in both sections, all through the rampart, down to the old surface line. It appears that the dyke had been cut through ground occupied at an earlier date by the Romanized Britons, and that in forming the ditch they threw up the refuse from the habitations to form the bank, including the scattered coins and pottery. A human skeleton of similar character to those found in the villages was also discovered beneath the old surface line in one of the sections, the old surface line being clearly marked over it, showing that it had been buried there before the rampart was thrown over it. From this it appears probable that this dyke was thrown up to defend the Romano-British villages that are situated to the westward or rear of it, from an attack from the east, and that this must in all probability have been done at the time when the Saxon invaders were pressing upon them from the eastward.

MAP SHEWING THE AREA FORMERLY OCCUPIED BY CRANBORNE CHASE, WITH THE ANTIQUITIES CONTAINED IN IT.



SCALE OF MILES.

This discovery throws a flood of light upon the history of this part of the country at that time, and shows that the Britons must have made a stout defence against their Anglo-Saxon conquerors, sufficient perhaps to account for the apparent predominance of British blood which has been noticed amongst the existing population of the district.

Wansdyke, which runs from a spot not far to the north of the Bokerley Dyke in the direction of Bath, has the same defensive attitude as Bokerley, and the examination of it, which it is proposed to make, will show whether or not it is of the same period.

The observations of Dr. Beddoe and other physical anthropologists upon the present population of the country show that the people of the South-West of England are, as a rule, shorter and darker than those to the eastward, and my own observations upon the people of this particular district will, when they are systematized, tend to define the area of this ethnical frontier more precisely. It would be a remarkable result if it should hereafter be shown that the physical changes observable in the distribution of the existing population are in any way coincident with these lines of defensive earthworks of the Roman or post-Roman age: and if it should be further shown that the same physical characteristics have persistently belonged to the people of this region ever since the time of the Neolithic folk of the long barrows, we shall find ourselves in the presence of anthropological deductions of some value in their bearing on the history of England. I purposely avoid speaking with confidence upon this point, feeling certain that the necessary evidence for deciding the question lies buried in the soil of the district, and will hereafter be unearthed. I shall resume the inquiry as soon as the harvest, if such it can be called this year, is over; but without bias, and with a mind prepared to throw over any preconceived hypothesis the moment it shows itself to be untenable.

Section A—Mathematical and Physical Science.

Members of the Mathematical and the Mechanical Section had a meeting in the rooms of Section A for the special purpose of discussing the question of lightning-conductors. The chair was occupied by Prof. G. F. Fitzgerald, President of the Mathematical and Physical Science Section.

Mr. W. H. Preece, President of the Mechanical Section, opened the discussion, and said that if we wanted to know anything about atmospherical electricity, we had to go back to the works of Benjamin Franklin, 100 years ago. Up to 1870 there were absolutely no rules for the guidance of those who desired to erect lightning-conductors for the protection of buildings. In that year a great Conference was held on the subject, and the result of its deliberations was published in a book, and included a set of rules for the construction of conductors. We had since had great experience of them. He had under his supervision no fewer than 500,000 lightning-conductors. Some time ago a lectureship on atmospheric electricity was founded in memory of Dr. Mann, who experimented on the protection of buildings in South Africa. Prof. Oliver J. Lodge was selected as the lecturer, but, instead of cracking up the work of the Conference, he took the other line, and, if his statements were true, lightning-conductors would be of no use, and no buildings would be safe in a thunderstorm. Prof. Lodge had committed himself to fallacies which it was now his duty to bring before the meeting. The Professor assumed that a lightning-rod formed part of the flash. Well, it did not. Nobody had ever seen a flash of lightning strike a conductor. The function of a conductor was to prevent the possibility of the building being struck by the flash. If it should be struck, there was some defect in the construction of the conductor. Lightning did not go careering wildly about, but passed along a path prepared for it. There was another fallacy, viz. that a flash of lightning was instantaneous. There was no proof of that. We saw a flash of light, which indicated the path of the discharge, but how long the discharge lasted we did not know. There were invisible flashes of lightning, which was proved by the fact that persons had been killed under trees when there was no visible flash. He, however, came to that conclusion from the effect on telegraph-wires, where there were currents of sensible duration, showing that the flash was not instantaneous. The next part was the hardest to discuss. It was the assertion that lightning was oscillatory in its character; that it did not go direct from the cloud to the earth, but went flashing backwards and forwards with considerable frequency. This assertion was based more on mathematical reason than on absolute observation, and engineers had no great respect for mathematical development unless it were confirmed by absolute experiment. The facts against the theory were that electro-magnets were affected for a considerable duration of time by lightning-flashes. Iron and steel were affected, and he had heard letters of the alphabet signalled along the telegraph-wires by a flash—the letter R which needed three signs, C which needed four, and there was a case on record of G, which needed eight signs. Under those circumstances the flash could not be oscillatory unless the oscillations were very infrequent. A discharge from condensers or Leyden jars might be oscillatory, but they were dealing with flashes of lightning. While he was attacking Prof. Lodge in that way, he must say that no one had worked harder or more honestly in the matter. Prof. Lodge had made experiments, and they were correct, from which he deduced that the self-induction of copper was greater than that of iron. He also had repeated these experiments, but his deductions were just the opposite. There was no doubt the Professor was on the brink of a discovery. He had started a fresh hare, which electricians must follow up and kill. Self-induction was called up to explain all the phenomena which they did not understand, and he inclined to think it was very much what the Americans called a bug. In the telegraph science they had known it for many years, and called it electro-magnetic inertia. The next fallacy was that most conductors did not protect any area, but it was known from evidence that they did. He preferred to stand upon the experience of the past rather than upon Prof. Lodge's mathematical assumptions. There was a tendency to hasty generalization among mathematicians, but there could be no doubt that the experiments of Prof. Lodge and others were opening their minds to the true nature of electricity, and that they would in time be able to speak of the mechanical character of electricity. They wanted to know where the energy came from which was so destructive in a flash of lightning. Aqueous vapour condensed and falling as rain at the rate of 1 millimetre per acre per hour developed an energy of 600 horse-power per acre. There was the creation of the energy which only wanted further development to turn into a source of electrical energy. He felt convinced that the result of that discussion would be to establish the truth of the position taken up by the Lightning-Rod Conference, and would bring to the front what they were all anxious to see, the true theory of electricity shadowed forth by Prof. Fitzgerald in his opening address, and that would make this meeting an epoch in the history of electricity.

Prof. Oliver J. Lodge said he had no lightning-conductors under his supervision, and all his conclusions were formed from experiments, and if they were correct very few buildings were effectively and thoroughly protected at the present time; and, further, if his views were correct, lightning-rods would in the future cost very much less than now. The term electro-magnetic inertia seemed to imply that they knew more than they did, so he preferred self-induction until they attained to knowledge. Mr. Preece said that no properly-constructed rod ever failed, but in the report to the Conference there were a number of entire failures named. He had made some very careful experiments in which he provided alternative courses for an electric current, and he found that it required less electromotive force to send the current along a thin iron wire than along a thick copper one. According to Mr. Preece, the object of the conductor was to prevent a flash of lightning, but rods were struck and melted. The conductor had two functions to perform —to act as a point and prevent a flash if it could, and to carry off a flash when it could not help receiving one. The electric charge had some energy, and they could not hocus-pocus it out of existence. It might be better to let it dribble away slowly down a bad conductor than to let it rush headlong down a good one. The length of flash was a question for the consideration of meteorologists, and the duration of flashes was a point on which the same gentlemen might do good work. He had seen flashes which appeared to last two or three seconds, but he thought they must have been a succession of flashes. The fact that flashes deflected the compass-needle did not prove that they were not oscillatory, nor did it prove anything as to their duration. A momentary flash might produce the same effects. There was the question of a flash magnetizing a bar of steel. An oscillating current was able to do that; although Prof. Ewing used an oscillating current to demagnetize steel. The discharge of a Leyden jar caused an oscillating current. The charging was like lifting a pendulum rod suspended freely at one end. When the jar was discharged it was like releasing the pendulum; it must oscillate, and so must the electricity, and its oscillation would vary in accordance with the friction and other modifying causes. The greater the electro-magnetic inertia, the more certainly would there be oscillation. With regard to the protection of areas, the area which Mr. Preece imagined as protected was so small that they might give it him without discussion. There was, however, in his opinion no sure area of protection. Mr. Preece might have pressed him hard on the question of the conditions of a flash. He (the speaker) had assumed that the flash behaved as electricity did in an experiment. The cloud, however, was not like the tinfoil of a Leyden jar; it was made up of globules with spaces between them, and a discharge might be more like that of a spangled jar, or might be dribbled away a bit at a time, and not by great rushes. But they could not assume that it would always do so, and must prepare for the occurrence of a great rush. The true character of lightning must be discovered by observing lightning, and not by experiments in a laboratory. The spark of one induction-coil at a considerable distance would start another one sparking merely by its light. From that he came to the conclusion that when there was a very bright flash of lightning, it must involve very important consequences. There was no doubt that it would cause discharges all over the neighbouring area, and so he would say that areas of protection were misleading, and if a flash had that effect, they had better be without it if possible.

The Hon. Ralph Abercromby, who showed a number of photographs of lightning flashes, said there was no absolute evidence in the photographs of flashes of lightning following each other rapidly on exactly the same path. There was, however, distinct evidence of the tendency of lightning-flashes to occur parallel to each other. There seemed to be a tendency in lightning flashes to be ramified, to give off threads all round the main flash. Photography gave conclusive evidence that flashes were not so instantaneous as was generally supposed. It showed that the flash did not always jump from a cloud straight to the earth, but sometimes went meandering through the air and tying itself into knots, so that it could not be so instantaneous as was imagined. He was of opinion that lightning-clouds were generally more than 500 feet high, but lightning was rarely much higher than 10,000 feet high. By this he did not mean that lightning might jump 10,000 feet from the cloud to the earth; but that at an altitude of 10,000 feet on a mountain-side a thunderstorm was usually *below* the observer.

Lord Rayleigh said that, although some mathematicians were unpractical, yet it was to mathematics one must go to find the results of known causes under new circumstances. He had no special knowledge of lightning-conductors, but from his general acquaintance with electricity he should say that Prof. Lodge's experiments could hardly fail to have a most important practical application to lightning-conductors in the future. Mr. Preece spoke of the development of energy by the condensation of vapour into water, but the question was to find how some of that energy came to take the electrical form.

Sir W. Thomson said that mathematicians never predicted that the Atlantic cable could not be laid, but a celebrated engineer did so. He thought Prof. Lodge was in the American stage of inertia and Mr. Preece in the English stage. He believed that if Prof. Lodge proceeded with his experiments he would confirm his discovery that iron wire was a better conductor than copper. Self-induction was in the air, and they were talking of nothing else. He thought Mr. Abercromby's idea as to the duration was correct. It seemed to him probable that it was the sound of one spark which caused another rather than the light. There was the photograph giving three parallel flashes. It would be well if some experiments could be made to discover whether flashes occurring like that were simultaneous or followed one another, being started by the light or sound vibrations of the first. It was rather startling to find that a lightning-rod had protecting power over so small an area, and he would like to ask Mr. Preece whether copper had been experimentally proved to be better than iron. They could come to one conclusion from what they heard—namely, that houses made of sheet iron would be the safest possible places in a thunderstorm. The question of the effect of self-induction on statical discharges was a very important one. He suggested as a class experiment the discharge of a Leyden jar through a number of students (1) when they were arranged in zigzag rows, so as to have no self-induction in the path of the discharge; and (2) when they stood in a circle, so that the self-induction of the path was a maximum. The students should stand on insulating material. He thought the result of such an experiment would be to show that the students in the middle of the chain would feel the effect of the discharge far less in the second instance than in the first. With reference to the reports as to the occurrence of globular lightning, he believed them to be much exaggerated, and expressed an opinion that the whole effect might be a physiological optical delusion. Reiss experimented some forty years ago on the question of magnetism by jar discharges, and found that the direction of superficial magnetization sometimes was the one to be expected, sometimes the opposite one. He suggested new experiments as to the influence of the rate of oscillation on the result. The most efficient protection for gunpowder against lightning would be, he thought, to put it in a house whose exterior was entirely of iron and to put no lightning-rod on it.

Prof. Rowland observed that the conditions of Prof. Lodge's experiments were scarcely the same as those of actual lightning, and he pointed out that the length of the spark was no measure of the resistance of the conductor. Further, he showed some effects in Mr. Abercromby's photographs which were probably due to the astigmatism in the lens of the camera.

M. de Fonvielle, who spoke in French, observed that Sir William Thomson had said most eloquently that Mr. Preece was taking the English side of the question and Mr. Lodge the American side, but he must say that Sir William Thomson himself had taken the French side, and he had proposed a revolutionary system which consisted in the building of iron houses. He took the liberty, though being a Frenchman, to disagree with the great electrician, and to stand with Mr. Preece as an English conservative, with reference to lightning conductors. Lord Rayleigh said that mathematicians and physicists should unite together, but he supposed that Lord Rayleigh would agree with him in remarking that Mr. Preece was realizing that alliance in a very remarkable manner, for on the one hand he dealt with a large number of experiments and observations of natural facts, and on the other hand he introduced statistics, or rather the calculation of probabilities, which was one of the highest branches of mathematics. The experiments made in laboratories were different from those which were presented by Nature only so far as they were conducted on very widely different scales. On the previous day, in that hall, M. Janssen had proved by his observations on the action of oxygen on the composition of the electric light that in many phenomena there was a coefficient behind. He congratulated them on the aid they were now receiving from photography. He should advise the meeting to delay its opinion for the time until the completion in Paris of the Eiffel Tower, which would be the most extraordinary lightning-conductor in existence, being 1000 feet high. He must, moreover, state that Paris was practically free from calamities produced by lightning. They had erected a sufficient number of lightning-rods, according to the principles so admirably advocated by Mr. Preece, and that was a strong evidence that Mr. Preece was altogether travelling in the right direction, quite irrespective of any mathematical or physical demonstration.

Prof. George Forbes said that Mr. Preece did not mean to say that mathematicians came to wrong conclusions when they had all the right data, but that they sometimes came to a conclusion without taking all the data into consideration. Prof. Lodge had come to say that if iron was not better than copper, it was at least as good; but they could not be quite prepared to accept that, because the experiments might be tried in instances more nearly approaching the natural conditions, and in that case it was quite possible that copper would be found to be the best.

Sir J. Douglass said that his experience of lighthouses protected by lightning-rods covered a space of forty years, and was comforting to the members of the Lightning-Rod Committee. He never knew a rod fulfilling the conditions he prescribed to fail in protecting the lighthouse and adjoining buildings.

Mr. J. Brown suggested the use of a revolving camera in taking photographs, in order to separate flashes, and thus see if each is single or not.

Mr. Sidney Walker said that anything which would cheapen lightning-conductors would be gladly welcomed. In the cases where damage had occurred, he believed that the result was due to a defect in the conductor. He pointed out that iron would not stand the weather so well as copper, and that, besides, it would be affected by the gases at the top of a factory or similar place.

Mr. G. J. Symons said he had investigated every accident by lightning of which he could hear, and had so got valuable experience. The conclusion left on his mind was that if people would erect conductors precisely in accordance with the rules laid down by the Conference,[1] and fulfilling all the conditions, they would be absolutely safe. Where accidents occurred to buildings with conductors, there was a reasonable explanation to be found. Prof. Lodge's experiments were laboratory experiments, and to get the real facts they must have something on a much larger scale, perhaps by a series of interrupted conductors on posts on the tops of some of those high hills where storms frequently occurred. With regard to protected areas, there were only two cases on record, and those doubtful, of anything being struck within a protected area.

[1] Report of the Lightning-Rod Conference (Sp n, 1882).

Dr. Walker said he saw an obelisk on top of a hill struck. The top was knocked off, and the fluid came from the steps of the monument at fourteen different points, ploughing up the ground, and breaking rock at 100 feet distance.

Mr. Wood thought the black flash shown in one of the photographs was due to the reflection of one of the other flashes.

Lord Rayleigh said Stokes attributed that to the combination of gases in the path of the flash causing an opaque stratum.

Prof. Lodge said he could not understand why a conductor should have such a good earth. Why did not three points do at the bottom as well as at the top? If properly constructed conductors never failed, how was it that the hotel at Brussels was burnt, for that was considered protected in the most orthodox way? He would not say that conductors were of no use; they were of great use, but not absolutely certain. In his experiment he was bound to adopt the plan he did, because the experiments could not be done in any other way. It was only the outer surface of the conductor which conducted, and there was no particular good in the centre of a rod. A tube would do as well, and would be all the better if opened out into a flat bar, and yet better than that would be a strand of wires. Iron buildings, to be safe, must have perfect connections, for the smallest gap might give off a spark. That was the danger in houses supplied with gas; if the fluid travelled along the pipes and came to a gap, a spark and a fire might result.

Mr. Preece said the points between Prof. Lodge and himself were reduced to a very small compass indeed. He himself had always been a great advocate of iron on account of its cheapness. The use of copper caused needless expense in the erection of lightning-conductors. He believed every private house could be protected in accordance with the recommendations of the Conference for £1, if people would buy a coil of stranded iron wire a quarter of an inch in diameter, with the finial points, and have that put up.

The President summed up the discussion, and said the principal thing for them to pay attention to was that prevention was better than cure. There could be very little doubt that the presence of a considerable number of conductors afforded a great deal of protection to the area in which it existed, as was shown in the instance of Paris. It was desirable, if possible, that the whole country should be covered with conductors to prevent the discharge of flashes. There was no doubt that, though there might be room for improvement in the conductors, they had on the whole been right.

THE INTERNATIONAL GEOLOGICAL CONGRESS.[1]

II.

In order to understand the present status of the Congress, and to forecast its probable future, we must briefly note the work done at the two preceding meetings, and compare that with the general results of the meeting just closed. At Bologna the greater part of the time was occupied with discussions upon the exact meanings to be attached to various geological terms, and upon the general principles which should guide us in geological classification. Certain rules were then laid down, which probably few authors have consistently followed, and which it is unlikely will be universally adopted. At Berlin the discussions turned more upon precise questions of classification, especially those relating to the sedimentary rocks; upon the lines by which various groups of strata should be marked off; and, in some cases, upon the names by which these groups should be known. This change of procedure was necessitated by the progress made with the international geological map of Europe; the material for such discussion on classification having been provided in the shape of Reports from various national Committees, of which that from England, presented by Prof. Hughes, was by far the most complete.

At the London meeting the classification of the Cambrian and Silurian strata was fully discussed; and two other questions, only lightly touched upon before, were here considered in some detail—the nature and origin of the crystalline schists, and the upper limit of the Tertiary system.

In Bologna numerous votes were taken, in Berlin several, but in London none The English geologists were in a majority sufficiently large to carry any point upon which they were fairly well agreed, but no attempt was made to test this; and Prof. de Lapparent, in presenting a Report from the Committee appointed by the Council to consider the question of voting, paid a generous tribute to the English members for their self-restraint. There can be no doubt that the adoption of this Report marks an important epoch in the history of the Congress, and that resolutions hereafter voted will carry more weight than those which at present stand on its records. It recommended that members of the country in which the Congress meets should vote separately from the foreign geologists: if the votes of the two groups agree, the question will be taken as settled; if they disagree, the further consideration of the question will be postponed. The resolution further recommended that votes should not be taken on questions which are purely theoretical—such questions to be simply discussed, and various views obtained; and that decisions of the Congress should only refer to the more practical questions.

Two Commissions of the Congress have existed since the Bologna meeting—that on the Map of Europe, and that on Nomenclature and Classification. The work of the former is plainly marked out, and much has yet to be done. The other Commission has, however, in many respects served its purpose: it has obtained Reports from the various national Committees, most of which have been ably summarized by Prof. Dewalque. The future work of the Congress will partly lie in discussing these Reports, and in deciding such questions in general classification as may apply to wide districts, leaving minor points to be worked out by each country for itself. A Commission was therefore appointed with altered and somewhat wider powers; its functions will more fully shape themselves at the Congress in Philadelphia. As the future progress of the Geological Congress lies so much in the hands of this Commission, it may be desirable to record here the names of its members, which are to some extent the same as those already given (p. 519) for the Council of the London meeting, but there are some additions and changes:—Germany, Zittel; Australia, Liversidge; Austria, Neumayr; Belgium, Dewalque; Bulgaria, Zlatoski; Canada, R. Bell; Denmark, Johnstrup; Spain, Vilanova; United States, Hall; France, de Lapparent; Great Britain, Hughes; Hungary, Szabó; India, Blanford; Italy, Capellini; Mexico, Castillo; Norway, Kjerulf; Netherlands, Calker; Portugal, Delgado; Argentine Republic, Brackenbusch; Roumania, Stefanescu; Russia, Inostranzeff; Sweden, Torell; Switzerland, Renevier. Prof. Capellini was elected President of the Commission; and Prof. Dewalque, Secretary.

The Report upon the Map of Europe was presented to the Congress by Dr. W. Hauchecorne. This stated the progress which is being made. Four or five sheets of Central Europe will be ready for publication during the next two years, and it has been decided to publish the sheets as completed, each with its own title and index, instead of waiting for the completion of the whole of Europe, as was at first intended. A proof sheet (C iv.), containing a large part of Northern Germany, was exhibited; on this there are twenty-four different tints for the sedimentary formations, three for the Archæan, and nine for the eruptive rocks. The map is on the scale of 1 : 1,500,000, and will consist of forty-nine sheets. One colour is taken for each great group—Cretaceous, green; Jurassic, blue; &c. The subdivisions are shown by various modifications of these colours. As a rule, the lower subdivisions are shown by the darker tints, so that the map may be read with more facility than is usually the case with geo-

[1] Continued from p. 526.

logical maps. The map of the British Isles was handed in for publication at the closing meeting. Very little time was given to the map in the public sessions of the Congress, but the Map Commission had three long sittings, the results of which will be printed in the official Report. The most important points arrived at were the adoption of the term *Pleistocene* for the index of the map (the German term "*quartär*" to be bracketed with this); the separation of the modern deposits from the Pleistocene, and the mapping of the latter wherever practicable, the underlying formations (where known) to be distinguished by coloured lines; in modern eruptive rocks (those of volcanoes now active or only recently extinct) the stratified volcanic tuffs are to be distinguished from the cinders and the scoriæ.

M. Karpinski has been the representative of Russia on the Map Commission. On this occasion he was not present, his place being taken by MM. Nikitin and Tschernicheff. The latter submitted an important note on the crystalline schists of the Ural Mountains, which would have enlivened the discussion upon this question in the public meetings of the Congress. He states that the crystalline schists of the Urals contain limestones with a distinct hercynian fauna, and also that the schists pass horizontally into Devonian strata. It is probable that in cases of this kind (and similar cases elsewhere were referred to in the public discussion) the schists will be represented by the colour denoting their presumed age, whilst their present lithological character will be denoted by coloured lines. M. Nikitin raised a point which is important in many parts of Europe, but which is especially so in Russia—that is, the necessity of distinguishing *transition-beds*. He instanced the Volgian beds, which link the Jurassic with the Cretaceous; the Tartarian, between the Permian and the Trias; and others, spoken of by M. Nikitin as Permo-Carboniferous, which link the Permian to the Carboniferous. These transition-beds occupy immense areas in Russia, and cannot well be fitted into the existing classification.

The discussion on the crystalline schists occupied the whole of the sitting on Wednesday, and part of that on Friday. The material for this discussion had been provided by a collection of papers printed in advance and distributed at the opening. Translations from parts of this polyglot pamphlet have now appeared in NATURE. Essays in English were also contributed by five officers of the United States Geological Survey, with an introduction by Major Powell; and by Mr. Lawson, of the Geological Survey of Canada. One by Reusch, on Norway, also in English, was received too late for printing in the pamphlet, but it will appear in the full Report of the Congress.

This discussion derived additional value from the fine collection of rocks, maps, lectures, &c., illustrating this particular subject close at hand in the temporary Museum. The Geological Survey exhibited a large collection of rocks, maps, sections, &c., illustrating the North-West, the Central, and the Southern Highlands of Scotland; important collections of British rocks were also exhibited by Bonney, Blake, Hicks, Callaway, Cole, Hatch, Rutley, Wunsch, and others; foreign rocks were exhibited by Bell from Canada, Delgado from Portugal, Torell from Sweden, Reusch from Norway, Giordano and Mattirolo from Italy; whilst maps, drawings, models, &c., illustrating the discussion, were exhibited by Teall, Baltzer, Cadell, Ricketts, Lapworth, and others. Special mention should be made of the splendid collection exhibited by Heim, illustrating the deformation, crushing, &c., which the rocks of the Alps have undergone. All these exhibits are described in the Catalogue (54 pages with supplement of 4 pages). Several members of the Congress assisted in the arrangement of this Museum, but its success was chiefly due to the labours of Dr. Hinde, Mr. Teall, and Mr. Rudler.

In the foregoing notes we have not attempted to summarize the discussions. These were reported at some length in the *Times* and in other papers. We have preferred to devote the space at our disposal to a general survey of the meeting, and to note some points of importance which could not well be included in a formal report of daily proceedings. As already stated, the discussions may by some be held to have led to no definite result, inasmuch as no vote was taken and therefore no formal decision of the Congress can in future be appealed to. But the great value of such meetings lies in the opportunity afforded for personal discussion and the interchange of opinions, not only in the public sessions, but in the more easy and informal conversations over the exhibits in the Museum, in the corridors and reading-room, and at the friendly and social gatherings which made so pleasant a feature of the London meeting. We have no doubt that the general result of this meeting on geological opinion and progress will be at least as good as that of any which has gone before.

The London Congress was particularly fortunate in its place of meeting. Within the walls of the University of London there was ample accommodation for all the requirements of the Congress, whilst close at hand were the Jermyn Street Museum and the rooms of the Geological Society. Unfortunately the Honorary President, Prof. Huxley, was kept away by ill-health; Prof. Hughes, who has done so much for the Congress in England, was also unable to attend. The early death of M. Fontannes, who has so ably reported the proceedings of previous meetings, is a great loss to the Congress, and many fears were expressed that his place could not be adequately filled; but the labours of Messrs. Hulke and Foster in the Council, and of Barrois and Renard at the meetings, resulted in fuller reports than have appeared of any previous Congress.

REMARKS ON SOME OF THE MORE RECENT PUBLICATIONS DEALING WITH THE CRYSTALLINE SCHISTS.[1]

IN acceding to the invitation of the Geological Congress to contribute to the discussion of the crystalline schists, the author expresses his regret that his time has not allowed him to throw new light by fresh observation on the points of controversy. Other labours have for a long time completely occupied him; so that he has only been able to occasionally assist with advice a younger fellow-worker, Herr Emil Danzig, of Rochlitz, in his researches on the Saxon granulites. This work, which has but recently been brought to a close, and has been placed at the disposition of the members of the Congress, is recommended to the notice of those fellow-workers who are interested in these matters, for in it the granulite question has been completely treated and advanced another stage.

Prof. Lehmann still takes his stand on the results furnished him four years ago by his investigations on the old crystalline schists.

The, on the whole, favourable reception of those investigations assuredly indicates that the right path has been struck, and that an extension of our views on the crystalline schists has resulted from them. This is also proved by the fact that these views have also been successfully applied in other places. That in many cases the opinions advocated by the author have not been rendered quite correctly, cannot excite surprise. Such misconceptions were scarcely to be avoided.

Prof. Lehmann strenuously opposes the notion that his generalizations were made without due consideration, and draws attention to certain criticisms to which his work has been recently subjected.

As is well known, the controversy on the Saxon granulites turns on the question, whether their plainly developed parallel structure is to be regarded as true bedding in the sense of sedimentary deposition, or as of eruptive or plutonic origin. The same questions arise in the discussion of all other districts in which crystalline schists occur; the solution, however, will by no means always be the same. It is beyond doubt that a whole

[1] "Bemerkungen zu einigen neueren Arbeiten über Krystallinisch-schiefrige Gesteine," by Prof. J. Lehmann. Published by the International Geological Congress, London, 1888. (Abstracted from the German by Dr. F. H. Hatch.)

series of crystalline schists are of sedimentary origin, and it is a matter to be decided by detailed investigation which are to be considered as sedimentary and which as eruptive or plutonic. The results obtained by the author in the investigation of the Saxon "Granulitgebirge" and some adjacent districts do not therefore claim universal application.

The tentative interpretations given by him were arrived at by the close observation of the field-relations of the rocks in question during a geological survey extending over several years; and it can now only be a question in how far the interpretation, which has been recognized with certainty as correct for a series of phenomena, can be applied to other phenomena intimately related to them. The author admits that here and there he has gone somewhat too far in his tentative interpretation. It was scarcely possible, in so difficult a question as the "granulite question," which to-day has not yet reached its final limits, to go just so far that later experience should find nothing to modify.

But the description of the author's work by J. Roth (in a paper on "Zobtenite," read before the Berlin Akademie der Wissenschaften on June 23 of last year) as "a marvellous agglomeration of the most daring hypotheses" is scarcely justifiable.

In Prof. Lehmann's investigations on the crystalline schists it has, for the first time, been shown in the greatest detail that their present condition cannot be original, but must be one that has been influenced by the dynamic processes accompanying mountain-building. He is far from maintaining, however, that similar observations had not already been made; and he readily acknowledges that eminent investigators of the crystalline schists, such as Kjerulf and Michel-Lévy, had, at a much earlier period, made such observations. What is new is the mode and method in which the author utilizes his observations. Researches of this kind were sunk into oblivion: the theory of the sedimentary origin of the crystalline schists had become the ruling dogma; and the *Eozoon canadense* had also made its appearance in Europe.

Roth, in the paper referred to, maintains his old position, according to which the crystalline schists, including the phyllites, are plutonic and unaltered formations.

The evidence advanced by him to prove that the stratiform gabbros, which he terms zobtenite, cannot be numbered with the eruptive rocks is insufficient. The occasional observation of conformable relations with other crystalline schists is inadequate. This does not, however, hinder Roth from regarding it as proved that the Zobten rock cannot be eruptive. The isolated patches of the old rocks that crop out in Silesia are unfortunately extremely confused. The stratigraphical relations of these rocks, which are very highly metamorphosed, cannot be utilized to support either view, and no hope is to be entertained of more favourable exposures in the future.

Prof. Lehmann's views on the Saxon granulites have, in the main, been confirmed by the before-mentioned work of Herr E. Danzig. This work again shows how confused are the field-relations in the granulite-district, and that few exposures permit of an indisputable solution.

In the northern half of the Saxon district the granulite assumes a granular structure, and acquires a marked similarity to some "bedded" granites. These points have received especial attention from Herr E. Danzig. He comes to the conclusion that in many places no sharp line can be drawn between granulite and granite; further, that rocks, which belong undoubtedly to the granulites, present, like the granitic gneisses occurring in the granulite-complex and interbedded with mica-schists, the character of eruptive masses. They contain included fragments, and impregnate these as well as their immediate neighbourhood. The supposition formulated by Prof. Lehmann at the close of his researches in this district is thus confirmed—namely, that the Saxon granulite is a granite *massif*, which has been influenced in structure and composition by dynamic metamorphism.

This confirmation of his work induces the author to explain why he cannot accept the views advocated by E. Reyer in his newly-published work on "Theoretical Geology." Reyer holds the Saxon granulite-*massif* for "a mass of eruptive granite (*Massenerguss*), mantled over by 'tuffogenic' sediments (granulite), through which granite dykes are extruded from the central mass; while granite sheets (*Flankenergüsse*) are intercalated between its beds." Reyer might have gathered from the author's work that the Saxon granulites are, in the main, by no means highly metamorphosed: on the contrary, they deviate very little, in part not at all, from the original structure of eruptive granite rocks.

But apart from this, and without dwelling on the fact that we know absolutely nothing of the rocks underlying the Saxon granulites, the supposition that the alternation of mica-schists with granulite or granitic gneisses has been produced by an accumulation of successive lateral eruptions (*Flankenergüsse*) and precipitated sediments, cannot hold good.

The theoretical considerations of Reyer, the utility of which is gladly recognized by the author, and which in many cases can be supported by direct observation, must not be allowed to prejudice our judgment. The actual facts must first be established, and in so doing we do not encounter the streaky and platy structures which characterize the direction of movement in magmas. We see, in truth, something quite different. The "bedded" granite presents no zones of consolidation that follow closely the surrounding slates; we see rather an extraordinarily uniform mass of granite at first traversing, in a dyke-like manner, the slates, but afterwards insinuating itself between them, in both cases enclosing fragments of the traversed rock. Where the granite was intruded as a dyke these fragments lie without order, but where it forms a sheet the flat pieces are, almost without exception, arranged parallel to the walls of the dyke. We are accustomed to regard granite, occurring as a dyke, as younger than the rock in which was formed the crack along which the molten rock ascended, without wishing to deny that it has existed, from the very beginning, deeply hidden in the bowels of the earth, and is therefore, in reality, older than the slates it traverses. But it has become customary to observe the convention; indeed, it is necessary to do so if we do not wish to be involved in universal chaos.

For the "bedded" granite it is no simple matter to prove that it is younger than its hanging wall. Attentive examination shows that the apparently conformable boundary has no such very conformable course; that, further, the apparently sedimentary beds are sometimes distinctly detached, and turn out to be loose masses; finally, a whole series of detailed phenomena show that wherever there have been dislocations, the granite has followed the opening and has impregnated the slates. How far such an impregnation can be assumed to have taken place is a matter for personal experience.

In the granite dykes the inclusions and the boundary surfaces of the slates present exactly the same phenomena; only in this case the fragments do not all present a parallel arrangement.

One would be driven to deny the possibility of strata or slate-masses being split parallel to their stratification or their bedding, if we were to deny that the "bedded" granites do not as much constitute a case of intrusion along cracks as do the obliquely-running granite dykes. Why should there not be, among such a number of granite dykes that run unconformably, some that have been formed by the in-filling of cracks (of seldom more than 400 metres width) that follow the divisional planes of stratification or cleavage? It is not to be supposed that these were cavities, the wide sweeping arches of which were supported by the rigidity of the lateral rock-masses: as fast as the slates were separated the granite forced in its way, and filled up the crack as soon as it was developed.

This separation along parallel divisional planes and intimate impregnation with eruptive material, which can be followed in the minutest details with the greatest clearness, arouse the question as to whether the same phenomena have assumed greater dimensions—dimensions that would still be trivial in comparison with the masses erupted. The author has described several exposures in the Saxon granulite district that render any other interpretation impossible.

Kjerulf, Michel-Lévy, and others have described very similar relations among eruptive masses. Michel-Lévy has quite recently given expression to his opinions in a "Note sur l'origine des terrains cristallins primitifs," and in a "Note sur les roches éruptives et cristallines des montagnes du Lyonnais." His statement to the effect that the author and a portion of the German school assume a development of heat by the plication of the earth's crust is, so far as the author is concerned, incorrect. On the contrary, he has shown that a conversion of motion into heat has left no visible traces. He is quite at one with the French investigator as to the origin of the heat in the earth's crust.

The chief requisite in the discussion of the crystalline schists, is never to leave the solid ground of facts, and to pay particular attention to the collecting of these. If the statements of some authors are examined, it must awake astonishment to see with what positiveness statements are made, which, although of the greatest importance for the proper judgment of the genesis of rocks, do not correspond in the least to the facts, and by a little attention might easily have been avoided. The author has already had occasion to disprove the non-occurrence of fragments of clay-slate in the phyllite-gneiss of Goldkronach in the Fichtelgebirge, as maintained by Gümbel. A few hours' search sufficed to collect the clearest examples of abruptly fractured and injected enclosures From this occurrence Gümbel drew widely generalized conclusions. Whether it is enclosed fragments or concretions that are contained by a rock, is assuredly of the greatest importance for its proper explication.

The author then refers to the Ober-Mittweida conglomerate. Roth has described the pebble-like fragments enclosed in this rock, as concretions. The author cannot agree with him in this determination, and gives his reasons, which are mainly based on petrographical considerations, why he does not do so.

It will hardly be denied that dynamic processes involved in mountain-building, be the latter referred to whatever cause one will, have not only not been without influence on the structure of the rocks already in existence, but have also considerably influenced the distribution and the intrusion of the eruptive mas es. The observations which the author has published on the gabbros of the Saxon granulite district, on banded granulites, on the conglomerates of the Saxon Erzgebirge that have been metamorphosed to mica-schists, and on Bavarian "Pfahlschiefer," have not been refuted, although their re-examination would not have been attended by any especial difficulties.

The author then concludes by expressing the wish, that, in the further study of the crystalline schists, metamorphosed sediments, which can, it is true, be altered to true mica-schists, but never to true gneisses of uniform structure, should be kept distinct from those mica-schists of which we do not know the origin, and from the true granitic gneisses. If this be not done, the false conclusion is inevitable that sediments pass through mica-schists into gneisses. Metamorphosed, gneiss-like sediments, in which allothigenic minerals like feldspar are associated with authigenic quartz and mica, and which are, to the author's mind incorrectly, often designated gneiss, should be described as gneissose greywacke, or as gneiss-greywacke. Some such divisional line must be drawn if we are to obtain any enlightenment on the structure and origin of districts composed of crystalline schists and massive rocks. Dubious schists should be represented by a neutral colour, and not lumped in with the gneisses. The designation gneiss is meaningless as long as the most diversified crystalline and semi-crystalline schists are included under it.

Gneisses, to the author's mind, are granites possessing a parallel structure, which is partly original, partly the result of a more or less intense pressure during, or subsequent to, consolidation. Whatever else may be said, gneisses and granites are things that belong to the same category, and the author cannot reconcile himself to their separation, with respect to origin.

It cannot be natural to arrange in separate petrological categories eruptive granites and non-eruptive granites (gneisses), eruptive diorites and non-eruptive diorites, eruptive gabbros and non-eruptive gabbros, and to treat them from distinct points of view. Plutonic rocks do not differ from the similarly composed eruptive rocks, because they have not left the place in which they were formed.

How the origin of the plutonic rocks is to be conceived—whether as the primordial terrestrial crust, or as produced by the melting down or *diagenesis* of sediments, the latter supposition involving logically a similar origin for a part, at least, of the eruptive rocks—are questions of which the answers are at present less pressing, and which, with certitude, we shall probably never solve.

THE STRATIGRAPHICAL SUCCESSION OF THE CAMBRIAN FAUNAS IN NORTH AMERICA.[1]

A REVIEW of the opinion of American geologists on the succession of the Cambrian faunas shows that all have followed the scheme published by Sir William Logan in 1855 (Geological Survey, Newfoundland), in placing the Paradoxides fauna at the base, and then in succession the Olenellus and Dicellocephalus or Olenus faunas.

[1] Abstract of remarks made by Chas. B. Walcott, of the United States Geological Survey, before the meeting of the International Geological Congress in London, in the course of discussion on the Cambrian System, on September 18, 1888.

The discovery of *Olenellus Kjerulfi* beneath the Paradoxides zone in Sweden led me to re-examine the section of Cambrian rocks in New York, and finally to go to Newfoundland. After long search, I found a complete unbroken section on Manuel's Brook, Conception Bay, that showed the following conformable series:—

	Feet.
a. Archæan Gneisses.	
1. Conglomerate resting unconformably upon *a* ...	35
2. Sandstone, shale, and impure limestone with *Olenellus Bröggeri*,[1] and sixteen species of the Olenellus fauna	25
3. Greenish argillaceous shale	40
4. Red ,, ,, ...	4
5. Limestone	2
6. Greenish argillaceous shales with an abundant Paradoxides fauna at summit	270
7. Dark argillaceous shales, *Paradoxides, Microdiscus punctatus, Agraulus, Conocoryphe,* &c., &c., near base	295
8. Alternating bands of shale and sandstone, with *Orthis* in great abundance	400
Total	1071

Dip of strata, 12° to 15° N. All strata unaltered and undisturbed.

The above section proves that in North America, as in Sweden, the Olenellus fauna is beneath the Paradoxides fauna. This changes the American scheme of classification of the Cambrian system, and places it in harmony with that of Europe. The Olenellus fauna in America includes 42 genera and 112 species, and I now recall 4 genera and 20 species from Europe not known in America, which give a fauna of 46 genera and 132 species beneath the Paradoxides zone.

The following table exhibits the succession of the terranes as now known in America:—

TABLE I.—*Lower Silurian (Ordovician) System.*

	Subdivision.	Terranes.	Faunas.
Cambrian System.	Upper Cambrian	Potsdam, Knox, Tonto, Belle Isle, &c.	Dicellocephalus or Olenus.
	Middle Cambrian	St. John, Avalan, Braintree.	Paradoxides.
	Lower Cambrian	Georgia, Prospect, Terra Nova.	Olenellus.

A comparison of typical sections of the Cambrian system gives the following:—

TABLE II.

Sweden.	Wales.	Newfoundland.	New York.	Rocky Mountains.
Olenus zone.	Olenus zone.	Olenus zone.	Olenus zone.	Olenus zone.
Paradoxides zone.	Paradoxides zone.	Paradoxides zone.	Represented by other genera than Paradoxides.	Represented by other genera than Paradoxides.
Olenellus zone.	Unknown.[2]	Olenellus zone.	Olenellus zone.	Olenellus zone

It affords me pleasure to recognize the work of the Swedish geologists, and to fully coincide with their results, and thus firmly establish on the two continents the true order of succession of the oldest known Palæozoic fauna.

[1] Name proposed for new species of Olenellus.
[2] After this paper was read, Prof. Lapworth showed me specimens of Olenellus like *O. Bröggeri*, from Shropshire.

NOTES.

WE regret to have to record the death of the well-known traveller, Mr. William Gifford Palgrave. He died in his sixty-third year at Montevideo, where he was British Minister. Mr. Palgrave will be remembered chiefly as the author of the famous "Narrative of a Year's Journey through Central and Eastern Arabia, 1862-63," one of the most brilliant and fascinating books of travel of modern times.

DR. CARNELLY, of University College, Dundee, has been appointed Professor of Chemistry at the University of Aberdeen, in the room of Dr. Brazier, who has resigned.

THE Emperor of Japan has conferred the Order of the Rising Sun, of the Fourth Class, on Mr. Thomas Alexander, Professor of Engineering, Trinity College, Dublin, for services in the Imperial University of Japan.

MR. EDGAR THURSTON, Superintendent of the Government Museum, Madras, expects to arrive in England early in October. We understand that Mr. Thurston has made some valuable collections of corals and other marine animals.

DR. LATHAM will deliver the Harveian oration at the Royal College of Physicians on Thursday, October 18, at 4 o'clock.

THE Exhibition held by the Photographic Society of Great Britain was opened on Monday at 5A Pall Mall East. It will remain open daily, and on Monday, Wednesday, and Saturday evenings, until November 14. Every Monday evening transparencies will be shown with the Society's optical lantern.

THE French Government has reorganized its system of war aërostation. Henceforward the activity of the director of this department will be chiefly concentrated on the manufacture of captive balloons for the several *corps d'armée* and fortifications.

WITHIN a month a new central station for the electric light will be opened at the Palais Royal, Paris, for the shops, the galleries, the Conseil d'État, the Cour des Comptes, the Théatre Français, and the Palais Royal. The building of the cave in which the engines are to be placed in the courtyard is almost finished.

ON Tuesday the seventh International Congress of Americanists was opened at Berlin, in the large hall of the Rathhaus, before a brilliant gathering of archæologists. The opening address was delivered by Herr von Gossler, Minister of Public Worship, who warmly welcomed his hearers in the name of the German Emperor and the Prussian Government, and referred to the distinguished services rendered by the brothers Humboldt in unfolding the secrets of the New World. The Congress will sit till Saturday.

AT the recent meeting of the American Association for the Advancement of Science, Dr. Daniel G. Brinton read an interesting and suggestive paper on the alleged Mongoloid affinities ot the American race. He held that the asserted Mongolian or Mongoloid connection of the American race cannot be proved either by linguistics or by physical resemblances. Speaking ot the typical, racial American culture, he maintained that it is as far as possible, in spirit and form, from the Mongolian. "Compare," said Dr. Brinton, "the rich theology of Mexico or Peru with the barren myths of China. The theory of government, the method of house-construction, the position of woman, the art of war, are all equally diverse, equally un-Mongolian. It is useless to bring up single art-products or devices, such as the calendar, and lay stress on certain similarities. The doctrine of the parallelism of human development explains far more satisfactorily all these coincidences. The sooner that Americanists generally, and especially those in Europe, recognize the absolute autochthony of native American culture, the more valuable will their studies become."

THE following changes have recently taken place in the editing of German botanical journals. The place of Prof. de Bary, as editor of the *Botanische Zeitung*, has been supplied by Prof. Graf zu Solms-Laubach, of Tübingen, who has recently succeeded the late Dr. Eichler in the Botanical Chair at Berlin; he will act in conjunction with the late Prof. de Bary's coadjutor, Dr. Wortmann. Dr. Kohl, of Marburg, has associated himself with Dr. Uhlworm in the editorship of the *Botanisches Centralblatt*, in the place of Dr. W. J. Behrens, who has been compelled to relinquish the editorship from the pressure of other engagements.

THE interesting and valuable reports on colonial fruit, which have been appearing in the *Kew Bulletin*, are continued in the October number. Much information is given as to fruit in Sierra Leone, the Gold Coast, Lagos, Natal, Malta, Cyprus, Ceylon, the Straits Settlements, and St. Helena.

THE late Mr. Samuel Miller, of Lynchburg, bequeathed to the University of Virginia 100,000 dollars, the income from which was to be expended for "the advancement of agriculture as a science and as a practical art by the instruction therein, and in the sciences connected therewith, of the youth of the country." A part of the income is to be used to maintain the work in agricultural chemistry already carried on at the University; but, according to *Science*, the larger portion of the income will be spent in promoting instruction and research in biology. A biological laboratory is being fitted up, and the equipment has been ordered. The instruction will be by lectures, with associated laboratory work, and will cover general biology, zoology, comparative anatomy, and biology applied to agriculture. The Professor-elect is Mr. Albert H. Tuttle, recently Professor of Biology in the Ohio State University at Columbus.

AN interesting gas, allene, the isomer of allylene, the second member of the acetylene series of hydrocarbons, has been obtained in the pure state, and its constitution thoroughly investigated, by Messrs. Gustavson and Demjanoff, of Moscow. Very little, and that contradictory, has hitherto been published concerning this gaseous hydrocarbon, which differs so remarkably from ordinary allylene, and yet is represented by the same empirical formula, C_3H_4. The new method of obtaining it i very simple, consisting in the action of zinc dust upon an alcoholic solution of dibrom-propylene. Practically one starts with glyceryl tribromide, $C_3H_5Br_3$, allowing it to gradually drop from a stoppered funnel into a flask containing pieces of caustic potash, and connected with a condenser. The flask is heated in a paraffin bath to about 150° C., when the propylene dibromide distils over as an oil of acroleïn-like odour. When the requisite quantity of the glyceryl tribromide has been added, the temperature is allowed to sink to 130°, and water run into the flask. On continuing the distillation the rest of the oil passes over in the steam. The dried and re-distilled oil is then used for the preparation of allene. It is allowed to slowly pass in drops into a second flask furnished with an upright condenser, and containing zinc dust and 80 per cent. alcohol. The flask is heated in a water-bath, and after about twenty drops of the dibromide have entered, the evolution of gas begins, and may be nicely regulated by the speed of dropping. The gas passes by a leading tube from the condenser, and may be stored over water in a gas-holder, being far less soluble than allylene. The gaseous allene thus obtained is colourless, has a peculiar smell, reminding one of its isomer, and burns with a smoky flame. Unlike allylene, however, it yields no precipitate with ammoniacal copper or silver solutions, but gives white precipitates with aqueous solutions of mercury salts. It combines rapidly, under considerable rise of temperature, with bromine, forming a colourless tetrabromide, $C_3H_4Br_4$, liquid at ordinary temperatures, with a camphor-like odour, but condensing to a crystalline mas at − 18°. In this respect, again, it differs from the tetrabromide of allylene, which remains liquid when surrounded by a freezing mixture. The constitution was finally proved to be $CH_2{=}C{=}CH_2$, as expected, the tetrabromide being, consequently, $CH_2Br{-}CBr_2{-}CH_2Br$; while allylene possesses the constitution $CH_3{-}C{\equiv}CH$, being, in fact, methyl acetylene, its tetrabromide being, therefore, $CH_3{-}CBr_2{-}CHBr_2$, a substance very different from the tetrabromide of allene.

INVITATIONS have been issued to each maritime nation to send one or more delegates to attend an International Maritime Conference to meet in Washington on April 17, 1889. The objects of the Conference will be to revise the regulations concerning vessels at sea, to adopt a uniform system of signals to indicate the direction in which vessels are moving in fog, snow, or thick weather, and at night, to convey warnings of approaching storms and other important information, and to formulate regulations for the prevention of collisions. The importance of the subject is so great that a full attendance of delegates is expected.

IN the *Archiv der naturwissenschaftl. Landesdurchforschung von Böhmen*, Band vi. No. 5, 1888, is a valuable memoir by Prof. Franz Klapálek under the title "Untersuchungen über die Fauna der Gewässer Böhmens, Part 1, Metamorphose der Trichopteren," in which the transformations of nearly twenty species of Bohemian caddis-flies are detailed, with illustrative figures and copious introductory general remarks on the internal and external anatomy of the larvæ and pupæ. The author states that the larvæ may be divided into two sections, which he terms "*raupenförmige*" and "campodeoid" respectively, and which correspond pretty nearly with the divisions "inæquipalpia" and "æquipalpia" employed by systematists for the perfect insects. Prof. Klapálek has been very successful in breeding these insects, a matter always attended with difficulty, more especially with those forms that inhabit rapid streams and torrents. A further series of observations will appear next year.

SOME interesting prehistoric remains have been discovered near Basingstoke. Six urns have been disinterred, and stone implements of very rude form have been found in the field in immediate relation with the vessels, although none have actually been discovered buried with the pottery. The site of the interments is a field adjoining Dummer Clump, a conspicuous landmark in the parish of Dummer, and near Kempshott Park, the seat of Sir Nelson Rycroft, who is the owner of the estate. A shepherd was pitching hurdles, when the bar came in contact with a large stone, which, on being removed, was found to have covered two very rudely-formed vessels, of which the under one was pronounced by Dr. S. Andrews, of Basingstoke, to contain human bones which had undergone incineration. Subsequently, another urn was removed, of a much coarser character, bearing a band round the base of the rim ornamented with sunken dots. All the vessels are hand-made and apparently fire-baked, and the larger ones have suffered some damage from the plough, which must have repeatedly passed over them.

THE new number of the *Internationales Archiv für Ethnographie* (Band i. Heft 5) will fully maintain the reputation of this excellent periodical. Among the contents are an article on arrows from Torres Straits, by Dr. M. Uhle; a note on a singular mask from Boissy Island, North-East New Guinea, and queries on the lizard in the folk-lore of Australasia, by Prof. H. H. Giglioli; and a paper on the chewing of the betel-nut, by F. Grabowsky. The coloured illustrations, as usual, are admirable.

FISHING is to be resumed this season at the Sild oyster-banks, on the coast of Jutland, which have been preserved for six years. The oysters are reported to be plentiful and in splendid condition.

THE additions to the Zoological Society's Gardens during the past week include a Grivet Monkey (*Cercopithecus griseo-viridis* ♂) from North-East Africa, presented by Lord Archibald Campbell; a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Major Dudley Buckle, R.A.; a Bonnet Monkey (*Macacus sinicus* ♂) from India, presented by Mr. G. C. Gosling; two Sooty Mangabey Monkeys (*Cercocebus fuliginosus* ♀ ♀) from West Africa, presented by Mr. Edward Felton, R.E.; an Ocelot (*Felis pardalis* ♂) from Pernambuco, presented by Mr. E. Percy Bates; a Weka Rail (*Ocydromus australis*) from New Zealand, presented by Mr. H. Lindsay; a Rose-crested Cockatoo (*Cacatua moluccensis*) from Moluccas, presented by Miss Eve; a Puffin (*Fratercula arctica*) from Cornwall, presented by Mr. J. Muir Drew; a Common Snake (*Tropidonotus natrix*), a Common Slowworm (*Anguis fragilis*), British, presented by Mr. P. S. Hutchinson; a Common Viper (*Vipera berus*), British, presented by Mr. A. H. N. Smith; four European Tree Frogs (*Hyla arborea*), European, presented by Mr. Lionel A. Williams; two Grivet Monkeys (*Cercopithecus griseo viridis* ♂) from North-East Africa, deposited; a White-backed Trumpeter (*Psophia leucoptera*) from the Upper Amazons, received in exchange; two Collared Fruit Bats (*Cynonycteris collaris*), an Axis Deer (*Cervus axis* ♂), a Canadian Beaver (*Castor canadensis*), four Chilian Pintails (*Dafila spinicauda*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE SATELLITES OF MARS.—These faint objects have been successfully observed, during the late opposition, with the great telescope of the Lick Observatory. The building operations prevented the observations being carried on systematically, but measures of distance and position of one or both satellites were obtained on nine evenings between April 9 and April 28, and Phobos was seen as late as July 18, when the theoretical brightness of Mars was but one-tenth of what it was at the opposition of 1877, or one-fifth of what it will be at the coming opposition of 1890. A preliminary reduction of the observations gives the following corrections to the times of elongations as given by Mr. Marth in the *Monthly Notices* of the Royal Astronomical Society, and by the *American Nautical Almanac* respectively:—

		Marth.		American *N.A.*
		h.		h.
Phobos	...	+ 0·427	...	− 0·33
Deimos	...	+ 0·020	...	+ 0·35

Mr. Keeler, who made the observations, remarks (*Astr. Journ.*, No. 178) that, so far as his estimates of the brightness of the satellites go, they support Prof. Pickering's conclusion that Deimos is one half-magnitude brighter when on the eastern side of the planet than when on the western.

TOTAL LUNAR ECLIPSE OF JANUARY 28.—No. 4 of vol. xviii. of the Annals of the Harvard College Observatory contains an account of the observations made there of the eclipse of the moon of January 28. The observations were of three classes—first, of the occultations of Dr. Döllen's list of stars; secondly, of the variation in the actinic brightness of the moon; and thirdly, the search, by means of photography, for a possible lunar satellite. In this second inquiry Mr. W. H. Pickering found that the photographic brightness of the full earth was 23·6 times as great as that of the full moon, equivalent to an albedo of 1·7 times that of the moon. The diminution in brightness ascribed to the moon during eclipse is most remarkable, Mr. Pickering giving the uneclipsed full moon as 1,400,000 times as bright as during the central phase, or about *twice* the ratio existing between the sun and full moon. In the search for the satellite a succession of photographs were taken, the telescope being made to follow the moon's motion as closely as possible, so that the stars were represented by short trails. A satellite would have left a trail inclined to the star trails and of a different length. The result of the search was negative, and as a satellite of the tenth magnitude, would have been registered on the plates, it appears probable that the moon has no satellite more than 200 metres in diameter, unless it was involved in the shadow of the earth during the eclipse, or was very dark, or was moving with the same speed amongst the stars as the moon, but in the opposite direction, in which case it would have been mistaken for a star.

Photometric Observations of Asteroids.—It has frequently been suggested that the asteroids, shining by reflected light, and subject, it might be assumed, only to variations the amount of which could be calculated for any required date, would prove specially useful as standards of brightness in the photometric observation of the fainter stars. Mr. Henry M. Parkhurst has carried out recently a series of observations on several of these bodies, which throws considerable light on their suitability for such a purpose. His method of observation was to note the time which the asteroid took to disappear after passing a transit-wire, the telescope being stationary, and the light of the asteroid or comparison-star suffering diminution either by a wedge or more frequently by a deflector—a piece of glass with nearly parallel sides, placed in the telescope tube, about one-seventh of the way from the focus, and covering half the field. The results of Mr. Parkhurst's observations, which embraced eighteen asteroids, and extended over nearly nine months—April to December 1887—are given in No. 3 of vol. xviii. of the *Annals of the Harvard College Observatory*, and show that the asteroids are not appreciably self-luminous, and that the sun undergoes no noteworthy fluctuations in light in periods of a few days; nor, as a comparison with observations made in some former years would indicate, in more lengthened periods. But they also show that the phase-correction is not covered by allowing simply for the decrease in the area illuminated—a further correction is needed, and one peculiar to each asteroid. In two cases, also, Harmonia and Iris, several of the observations stand out in strong contrast to the rest, and appear to indicate a variation due to axial rotation, the planet probably being irregular in shape, or its surface in reflecting power. No variation depending, as in the case of Saturn's ring, on the position of the asteroid in its orbit, and the relative position of the earth, has been noticed, but this inquiry has only been extended to the four asteroids first discovered. The mean error of an observation, when the special phase correction and probable variations due to rotation have been allowed for, appears to be less for an asteroid than for the fixed stars, the mean error of an observation of the solar illumination in the inquiry referred to above being given as 0·116m.

New Catalogue of Variable Stars.—Nos. 179 and 180 of *Gould's Astronomical Journal* contain a new catalogue of variable stars by Mr. S. C. Chandler. Mr. Chandler is not only a diligent observer of variable stars, the discoverer of several, and a zealous computer of the elements of their variations, but several years ago undertook an important and much-needed work, viz. the complete study of the bibliography of known and suspected variables. This catalogue coming from his hand, therefore, will be especially valuable, and the more welcome since it is thirteen years since Schönfeld published his second catalogue. Mr. Chandler puts it forward as merely a preliminary publication, a second more definitive being designed to follow as soon as the investigations now in hand shall have been completed. The present catalogue is no mere compilation. Almost every star in it visible from the latitude of Boston has been observed by Mr. Chandler, who has also gathered together and discussed every available published observation. The catalogue embraces 225 stars, and of these the variations of 160 are distinctly periodic; for 12 the periodic character is ill-defined, 14 are irregular, 12 are Novæ, and the remainder have been too little observed for the character of the variation to be properly known. Of the 160 periodic stars, the elements of 124 are the results of Mr. Chandler's own work, 22 are Schönfeld's, and 14 those of other computers after Mr. Chandler had carefully confirmed them. A point sure to lead eventually to an important advance in our knowledge of the cause of variation has received much attention from Mr. Chandler, viz. the systematic perturbations shown by so many of the periods, and a table is given of these inequalities for 26 stars. A useful novelty is introduced in the numeration of the stars of the catalogue, for, instead of giving them consecutive numbers, each is distinguished by a number equivalent to one-tenth of its R.A. for the mean equinox of 1900·0, expressed in seconds of time, thus securing that the numeration need not be disturbed by fresh discoveries.

Minor Planet No. 275.—This object has been named Sapientia.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *OCTOBER* 7–13.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 7

Sun rises, 6h. 13m.; souths, 11h. 47m. 40·2s.; sets, 17h. 22m.: right asc. on meridian, 12h. 53·9m.; decl. 5° 46′ S. Sidereal Time at Sunset, 18h. 29m.

Moon (at First Quarter October 12, 5h.) rises, 8h. 18m.; souths, 13h. 38m.; sets, 18h. 46m.: right asc. on meridian, 11h. 44·6m.; decl. 10° 51′ S.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	8 47 ...	13 19 ...	17 51 ...	14 25·7 ...	17 24 S.
Venus ...	8 27 ...	13 17 ...	18 7 ...	14 23·4 ...	14 7 S.
Mars ...	12 18 ...	16 3 ...	19 48 ...	17 9·5 ...	24 33 S.
Jupiter ...	10 50 ...	15 2 ...	19 14 ...	16 9·2 ...	20 28 S.
Saturn ...	0 46 ...	8 16 ...	15 46 ...	9 21·8 ...	16 17 N.
Uranus ...	6 27 ...	11 59 ...	17 31 ...	13 5·2 ...	6 17 S.
Neptune..	19 10* ...	2 56 ...	10 42 ...	4 1·3 ...	18 54 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich).

Oct.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
11 ...	B.A.C. 6524 ...	6½ ...	20 35 ...	21 17 ...	72 0
12 ...	B.A.C. 6889 ...	6 ...	19 53 ...	21 6 ...	110 311
13 ...	20 Capricorni ...	6 ...	19 5 ...	19 55 ...	153 240

Oct.	h.	
7 ...	4 ...	Venus in conjunction with and 5° 6′ south of the Moon.
7 ...	5 ...	Mercury in conjunction with and 8° 8′ south of the Moon.
8 ...	6 ...	Mercury at greatest elongation from the Sun 25° west.
9 ...	1 ...	Jupiter in conjunction with and 3° 33′ south of the Moon.
9 ...	22 ...	Mercury in conjunction with and 3° 9′ south of Venus.
10 ...	3 ...	Mars in conjunction with and 4° 38′ south of the Moon.
10 ...	13 ...	Uranus in conjunction with the Sun.

Saturn, October 7.—Outer major axis of outer ring = 38″·9: outer minor axis of outer ring = 9″·8: southern surface visible.

Variable Stars.

Star.	R.A.	Decl.			h. m.	
	h. m.	° ′				
U Cephei	0 52·4 ...	81 16 N. ...	Oct.	6,	3 52	*m*
			,,	11,	3 32	*m*
Algol	3 0·9 ...	40 31 N. ...	,,	5,	1 8	*m*
			,,	7,	21 57	*m*
R Aurigæ	5 18·3 ...	53 28 N. ...	,,	7,		*m*
T Monocerotis ...	6 19·2 ...	7 9 N. ...	,,	10,	5 0	*m*
U Monocerotis ...	7 25·5 ...	9 33 S. ...	,,	12,		*m*
S Cancri	8 37·5 ...	19 26 N. ...	,,	11,	0 27	*m*
R Crateris	10 55·1 ...	17 43 S. ...	,,	7,		*m*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,,	11,	19 0	*m*
β Lyræ...	18 46·0 ...	33 14 N. ...	,,	7,	0 0	*M*
S Sagittarii	19 12·9 ...	19 14 S. ..	,,	11,		*M*
S Vulpeculæ ...	19 43·8 ...	27 1 N. ...	,,	11,		*m*
χ Cygni	19 46·3 ...	32 38 N. ...	,,	9,		*m*
η Aquilæ	19 46·8 ...	0 43 N. ...	,,	12,	21 0	*M*
R Sagittæ	20 9·0 ...	16 23 N. ...	,,	11,		*m*
T Vulpeculæ ...	20 46·7 ...	27 50 N. ...	,,	12,	2 0	*M*
Y Cygni	20 47·6 ...	34 14 N. ...	,,	8,	3 0	*m*
			,,	11,	3 0	*m*
δ Cephei	22 25·0 ...	57 51 N. ...	,,	7,	20 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
	°	°	
Near η Persei	42 ...	55 N. ...	Slow.
,, θ Geminorum ...	102 ...	34 N. ...	Swift; streaks.
	135 ...	80 N. ...	Swift; streaks.
,, κ Cephei	305 ...	77 N. ...	Slow; faint.

GEOGRAPHICAL NOTES.

A TELEGRAM from Mr. Joseph Thomson, dated Mogador, September 19, reports that he has been successful beyond expectation in his exploration of the Atlas Mountains. He left Morocco city on August 27, and after being driven back from the Urika Valley to the south-east of the city, he proceeded eastwards, and succeeded in crossing the range southwards from Imintanut into the Sûs district. From Rezaya he ascended the main range to nearly 13,000 feet. Mr. Thomson intended to return to Hava for a few days, and afterwards to proceed northwards to Fez, Mequinez, and Tangier, returning home about the middle of December.

THE Report for 1887 of H.M.'s Special Commissioner for British New Guinea, contains information of considerable geographical interest. This is especially the case with the Report of Deputy-Commissioner Milman, who has charge of the western district, lying between the Dutch boundary and the Aird River. Mr. Milman refers to the discoloration of the sea about the coast between Talbot Island and the Fly River, due, doubtless, to the vast bodies of fresh water that empty into the sea from the Fly, Tait, Katoer, Mai-Kassa, and other rivers. The Fly River, as far as it has been ascended by Mr. Milman, is thickly populated with a purely agricultural and hunting people, living in large communities; while some houses in the villages are over 200 feet in length. As the river is ascended, traces of careful cultivation are seen here and there on the banks, the gardens or plantations being kept free from weeds, and planted with crotons and other bright-leaved shrubs between the bananas or other fruit-trees, besides being systematically irrigated by dykes cut at regular intervals, which, filling at high water, remain full as the water recedes. About 60 or 70 miles above Soomaioot several large creeks or rivers join the main river, but whether they are flowing into the river, or only form other mouths of this vast system, remains to be proved. The shores of the Fly River, as far as Mr. Milman ascended, are uniformly low, but owing to its great width he is inclined to think they are not subject to inundation. A tidal wave or bore, according to Mr. Milman, ascends the river, but only on the right bank, which accounts for previous visitors not having noticed it. A marauding tribe coming from the westwards have been in the habit of making attacks on the people in the neighbourhood of Sabai Island, but the exact locality they come from is a mystery. Their language and customs are entirely different from those of the Sabai Island people. They had probably never seen a white man until the Rev. E. B. Savage (who happened to be at Sabai when their lights were seen on the mainland) fearlessly visited their camp, and tried to hold some intercourse with them. He describes them as a much lighter race than the rest of the New Guinea natives, and as having long straight hair, while some of them have their nasal-bone pierced in three places, into which are introduced pieces of bone or shell. They appeared entirely unacquainted with fire-arms. Civilization has so far advanced at Port Moresby that a reading-room has been erected, in which the *Times* and other English journals are kept, a hotel has been opened, and a supply of water laid on by means of pipes to the native village.

A RUSSIAN scientific explorer, M. K. Nossilof, has recently returned to Archangelsk from Novaya Zemlya, where he spent a year, from the summer of 1887 to August 1888. He has brought with him rich botanical, zoological, and mineralogical collections, and means to return to the island soon, as he has resolved to devote five years to its exploration. M. Nossilof is reported to have discovered beds of iron, copper, coal, gold, and sulphur, some of which, he believes, could be profitably worked. Among other results obtained by him are many interesting observations on the animal, especially the bird, life of the island, thirteen months' meteorological observations, surveys covering 2500 square kilometres of land, observations on the ice-conditions of the east and west coasts, and 125 kilometres of coast survey. He has, moreover, discovered three new islands. During the winter and spring, M. Nossilof undertook excursions into the Kara Sea, and he hopes by-and-by to undertake a series of soundings as far as the River Yenissei. In the coming winter he intends to fix his station at the east end of Matotshkin Schar, and to establish there a second meteorological station, making excursions along the coast and into the interior.

ELECTRICAL NOTES.

THE Volta Prize of 50,000 francs has been awarded by the French Institute to M. Gramme for his labours in introducing and perfecting the continuous-current dynamo. The prize is given to the inventor who has formed a memorable epoch in the history of electricity. M. Gramme is a Belgian by birth, but a Parisian by residence. He is entirely a self-taught, self-made man. Although Gramme was anticipated by Pacinotti, his invention was entirely independent, and Pacinotti's was completely dormant, and would probably have remained hidden and unknown but for Gramme's success. No one will contend that the prize has not been richly deserved.

CONSIDERABLE attention has recently been drawn to some experiments by Chappuis and Maneuvrier, in Paris, on the decomposition of water by alternate currents. It is well to point out that the whole question was thoroughly threshed out by Sir W. Thomson in 1853, and his paper in the June number of the *Philosophical Magazine* of that year gives all that is necessary to know on the subject. Jamin, in 1882, showed how electrolysis could be performed by alternate currents by inserting an arc in circuit, the opposing E.M.F. of the arcs producing a partial rectification of the alternate currents. Mr. J. F. Kelley has just repeated the experiment in Newark, U.S.A.

MR. LOWRIE (B. A., 1888), showed how the insertion of an opposing E.M.F. in an alternating-current circuit enables electrolysis to be effected and how it could be utilized to measure the electrical energy consumed in electric light installations. If a decomposing cell of copper sulphate, and a constant E.M.F. such as a secondary cell, be inserted in the circuit, the current in one direction is assisted, while that in the reverse direction is opposed, and the cell is acted upon by the difference: an average current flowing, depositing copper at the same rate as if no alternate currents were present. 0·23544 gramme of copper is deposited per kilowatt-hour, or every gramme of copper deposited means 4·205 kilowatt-hours expended.

PROF. EWING (*Philosophical Magazine*, September 1888) has published, with additions, the paper read by him and Mr. Low at the Manchester B. A. meeting, on the influence of a plane of transverse section on the magnetic permeability of an iron bar. A joint between two portions of an iron core possesses distinct magnetic resistance even when the surfaces are true planes. Compression reduces this resistance in the rough faces and eliminates it when the faces are true planes. In all cases the resistance greatly diminished as the point of saturation was approached. A film of gold leaf interposed between the faces and compressed has only a very little injurious effect. Compression, however, reduces the permeability of the solid core for moderate magnetizing forces, though the contrary effect occurs when the magnetization is strong. Villari found the same reversal in the case of longitudinal pull, but in the opposite direction.

LORD RAYLEIGH (B. A., 1888) has been endeavouring to discover if an electric current flowing through an electrolyte causes the velocity of light to vary through the liquid. He experimented with dilute sulphuric acid. The result was negative within the range of the experiment, which was extremely delicate. In H_2SO_4 diluted, one ampere per square centimetre does not alter the velocity of light by one part in thirteen millions, or by 15 metres per second.

IT is estimated that in the United States there are 5351 electric light plants and stations working 192,500 arc and 1,925,000 glow lamps, and consuming 460,000 horse-power. There are thirty-four electric railways, 138 miles in length, run over by 223 motor cars using 4180 horse-power.

SIR WILLIAM THOMSON (B. A., 1888) dealt with the diffusion of rapidly alternating electric currents in the substance of homogeneous conductors. The surface is affected first, and the depth to which the disturbance penetrates depends on the frequency of the alternations. With a frequency of 150 per second a cylindrical copper conductor is said to be penetrated to a depth of 3 mm. Hence, if this be true, conductors for powerful alternating currents such as are used in the Gaulard and Gibbs system, should be tubes or flat bars with a thickness of 6 mm.

TROUVELOT has by photography obtained effects which lead to the conclusion that flashes of lightning may last several seconds. He gave his apparatus a slight horizontal displacement, and found a broad ribbon-shaped band on his plate.

NOTES ON METEORITES.[1]

IV.

Meteorites are Bodies which, like the Earth itself, revolve round the Sun.

WE have seen that the phenomena which accompany meteorites entering our air, whether they are soon burnt up and give rise only to the appearance of a shooting or falling star, or whether they are bulky enough to withstand the melting process till they reach the earth's surface, are similar. We are now in a position to discuss the origin of all these phenomena on the assumption that they have a common cause.

It is not so many years ago since the planetary spaces were supposed to be untenanted by anything more tangible than that mysterious fluid called ether. This notion is exactly represented by the French equivalent for those spaces, *le vide planétaire.* Hence, not to mention imagined supernatural causes—such as that, for instance, embodied in the tradition that Saint Lawrence, on the anniversary of his martyrdom (August 10), shed burning tears—the cause of the phenomenon was ascribed to atmospheric perturbations, exhalations of sulphur, *ignes fatui*, and so forth. An account of the August shower of 1857, even, published in the *Bulletin de l'Académie Royale de Belgique*, is accompanied by a minute record of rain, temperature, atmospheric electricity, &c.

Leaving out of consideration the opinions of the ancients, among whom Anaxagoras and Seneca may be especially mentioned, as being in favour of a cosmical origin, it may be pointed out that Kepler[2] regarded meteorites and shooting-stars as akin, and derived both from the ethereal regions.

Halley was the next to express an opinion that shooting-stars were of cosmical origin, but to Chladni belongs the credit of having broached the theory which modern observations have so abundantly justified. This theory was that space was full of the matter which, attracted by the earth, entered its atmosphere, accompanied by luminous effects only in some cases, and by actual falls of the matter in others.[3] The general acceptance of this view was retarded by Laplace and others, who saw a more probable origin for the phenomena by supposing meteorites to be masses shot out of lunar volcanoes. The first step in the demonstration of such an origin, which is now universally accepted, was made when Chladni,[4] in 1794, showed that no known terrestrial agency was capable of producing masses like the meteorites which had been seen to fall. At his and Lichtenbergh's suggestion, Brandes and Benzenberg in 1798 showed that, whatever they appear to do, shooting-stars never shoot upwards, but always downwards towards the earth. At the same time he showed the similarity of phenomena presented by fire-balls, shooting-stars, and the fall of meteorites, to which we have already called attention. He subsequently returned to and strengthened this view.[5]

"Should it be asked how such masses originated, or by what means they were brought into such an insulated position, this question would be the same as if it were asked how the planets originated. Whatever hypothesis we may form, we must either admit that the planets, if we except the many revolutions which they may have undergone, either on or near their surface, have always been since their first formation, and ever will be, the same; or that Nature, acting on created matter, possesses the power to produce worlds and whole systems, to destroy them, and from their materials to form new ones. For the latter opinion there are, indeed, more grounds than the former, as alternations of destruction and creation are exhibited by all organized and unorganized bodies on our earth; which gives us reason to suspect that Nature, to which greatness and smallness, considered in general, are merely relative terms, can produce more effects of the same kind on a larger scale.

"But many variations have been observed on distant bodies, which, in some measure, render the last opinion probable; for example, the appearing and total disappearing of certain stars, when they do not depend upon periodical changes. If we now admit that planetary bodies have started into existence, we cannot suppose that such an event can have otherwise taken place, than by conjecturing that either particles of matter, which were before dispersed throughout infinite space, in a more soft and chaotic condition, have united together in large masses, by the power of attraction; or that new planetary bodies have been formed from the fragments of much larger ones that have been broken to pieces, either perhaps by some external shock, or by an internal explosion. Let whichever of these hypotheses be the truest, it is not improbable, or at least contrary to nature, if we suppose that a large quantity of such material particles, either on account of their too great distance, or because prevented by a stronger movement in another direction, may not have united themselves to the larger accumulating mass of a new world; but have remained insulated, and, impelled by some shock, have continued their course through infinite space, until they approach so near to some planet as to be within the sphere of its attraction, and then by falling down to occasion the phenomena before mentioned. . . . .

"It is worthy of remark that iron is the principal component part of all the masses of this kind hitherto discovered; that it is found almost everywhere on the surface of the earth as a component part of many substances in the vegetable and animal kingdom; and that the effects of magnetism give us reason to conclude that there is a large provision of it in the interior parts of the earth. We may therefore conjecture that iron in general is the principal matter employed in the formation of new planetary bodies; and is still farther probable by this circumstance, that it is exclusively connected with the magnetic power, and also on account of their polarity may be necessary to these bodies. It is also probable, if the above theory be just, that other substances contained in such fallen masses, such as sulphur, siliceous earth, manganese, &c., may be peculiar, not to our globe alone, but may belong to the common materials employed in the formation of all planetary worlds."

This paper of Chladni's, it will be seen, dates from just before the beginning of the present century.

The subject was invested with a new interest in 1799, when the great Humboldt, who was then travelling in South America, saw an enormous quantity of shooting-stars covering the sky. In his long account of the shower in his "Personal Narrative," he states that, from the beginning of the phenomenon, there was not a space in the firmament equal in extent to three diameters of the moon that was not filled at every instant with bolides and falling stars; while he was locally informed that during a previous display in 1766 the inhabitants of Cumana had beheld the neighbouring volcano, Cayamba, veiled for an hour by a similar display.

In the next display, observed in the year 1833, 240,000 meteors were computed by Arago to have been visible above the horizon of Boston on the morning of November 13; while Mr. Baxendell, who observed the shower from the west coast of Mexico, states that "the number of meteors seen at once often equalled the apparent number of the fixed stars seen at a glance."

Olmsted, when he had witnessed the shower of 1833 (a shower heralded and followed by less brilliant displays in 1831–32 and 1834–35–36), and when, moreover, he had compared the phenomena with those recorded by Humboldt and Bonpland in 1799, announced the view which has since been so brilliantly confirmed—that the appearances are due to the passage of the earth through a *storm,* so to speak, of planetary bodies.

This was the first blow given to *le vide planétaire.* Space, instead of being empty, was full of bodies, some of them being congregated into rings, each body composing the ring revolving like a planet round the sun. In fact, these rings may be compared to *tangible orbits*; indeed, they almost realize the schoolboy's idea of an orbit, as a considerable part of the path is occupied by a string of little planets, while in the case of our earth's orbit, for instance, each point of the path is occupied in succession only.

Still Olmsted did not accept the view that the falling stars were of the same nature as meteorites.

Olmsted also noted that, however numerous the falling stars might be, or in whatever direction they appeared, or whatever the apparent lengths of their paths, the lines of motion of these paths, retraced along the sky, nearly all found a common focus of emanation or visual crater of projection among the fixed stars. This has since been called the radiant point.

The most salient fact, noticed even by those who did not see its significance, during the subsequent display in 1866, was that all the meteors seemed to come from the same region of the sky. Among all those seen by myself from 11 p.m. on Tuesday till a.m. on Wednesday morning, *two only* were exceptions to the general direction. In fact, there was a region in which the

[1] Continued from p. 533.
[2] "Opera," ed. Fritsch, vol. vi. p. 157.
[3] "Ueber den Ursprung der von Pallas gefundenen Eisenmassen," p. 24.
[4] His paper on the Pallas iron is abstracted in *Phil. Mag.*, Tilloch's Series, vol. ii., 1798.
[5] See *Phil. Mag.*, Tilloch, vol. ii. p. 225, *et seq.*

meteors appeared trainless, and shone out for a moment like so many stars, because they were directly approaching us. Near this spot they were so numerous, and all so foreshortened, and for the most part faint, that the sky at times put on almost a phosphorescent appearance. As the eye travelled from this region the trains became longer, those being longest as a rule which first made their appearance overhead, or which rended westward. Now, if the paths of all had been projected backwards, they would have all intersected in one region, and that region the one in which the most foreshortened ones were seen.

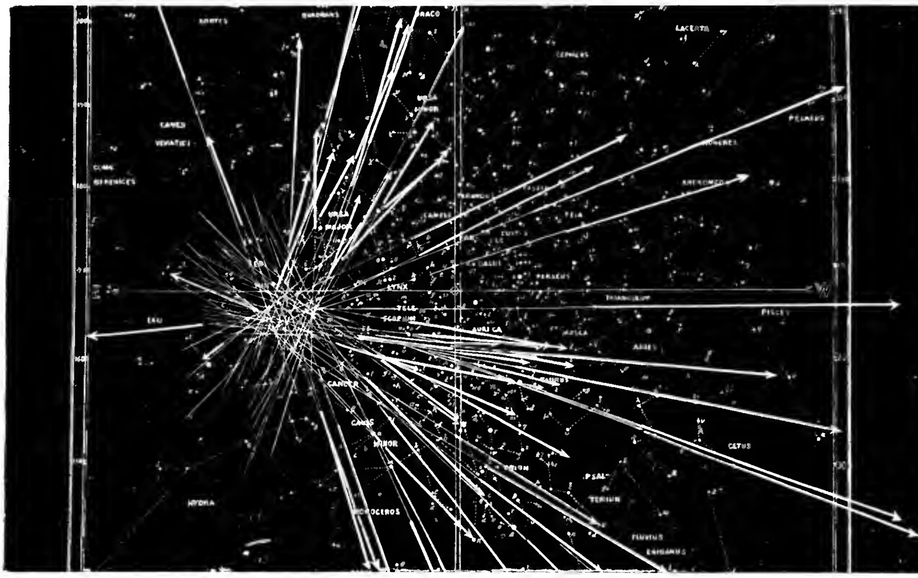


Fig. 7.—The radiant point of the November meteors.

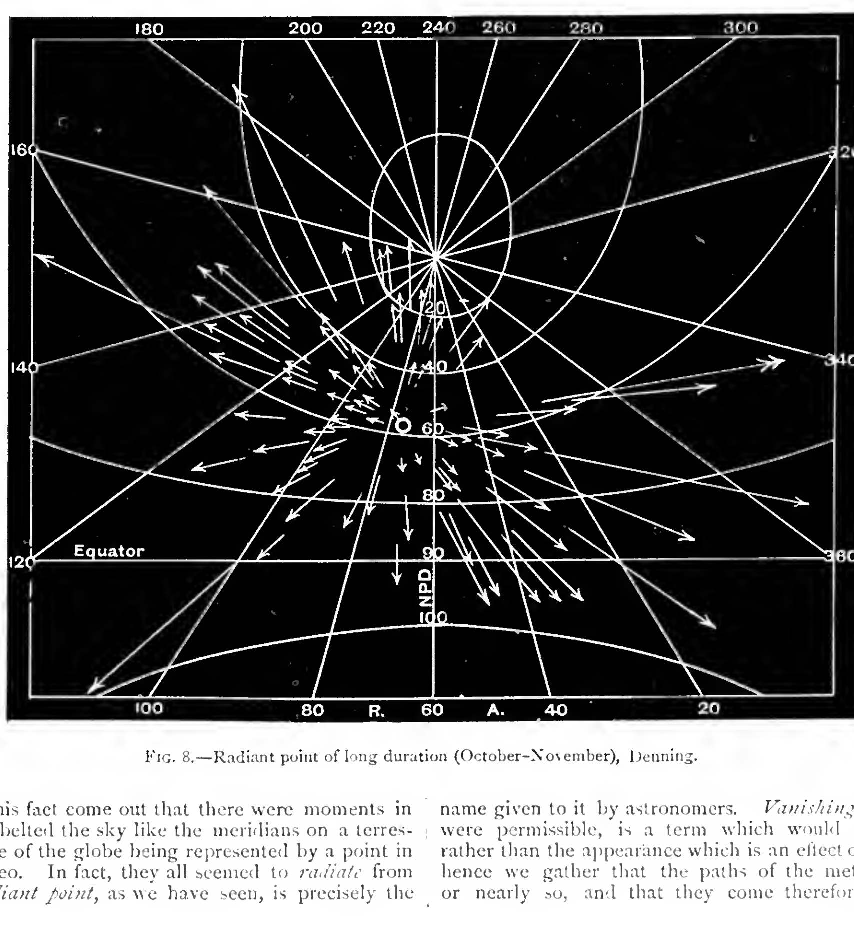


Fig. 8.—Radiant point of long duration (October–November), Denning.

So decidedly did this fact come out that there were moments in which the meteors belted the sky like the meridians on a terrestrial globe, the pole of the globe being represented by a point in the constellation Leo. In fact, they all seemed to *radiate* from that point, and *radiant point*, as we have seen, is precisely the name given to it by astronomers. *Vanishing point*, if the bull were permissible, is a term which would represent the fact rather than the appearance which is an effect of perspective; and hence we gather that the paths of the meteors are parallel, or nearly so, and that they come therefore from one point in the sky. The point from which they proceed in the case of the swarm we are now considering lies in the constellation Leo, situated in longitude 142° and latitude 8° 30′ N., according to Prof. Newton.

The radiants are generally of short duration, but Mr. Denning has shown that there are cases in which falling stars emanate from the same part of the sky for long periods of time.

One of these long-duration radiants between Auriga and Taurus is shown in the accompanying illustration (Fig. 8).

The next point, first brought to light by Olmsted, was that during a display the radiant point moves with the stars across the heavens. This is another strong argument in favour of the cosmical theory.

Meteors which are singly and occasionally observed, as we have seen, are called sporadic meteors, but in addition to these, which we may reckon to see every night, there are at certain times of the year very well known falls; so well known that we can say at once that on the 10th or 11th of next August more falling stars will be seen than are ordinarily visible. These are termed systematic meteors, and those to which we have just referred as appearing in November are of this class.

From 1833 to 1863 evidence was rapidly accumulated indicating that a very large proportion of the shooting-stars observed were not sporadic, but really systematic—that is to say, that at certain periods of the year meteors might be expected to diverge from their appearance in a particular part of the sky, and in greater numbers than from that part than from elsewhere.

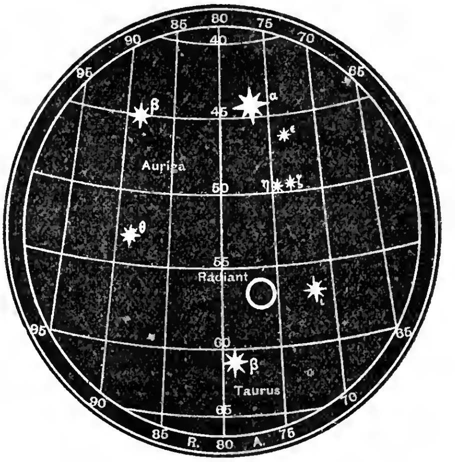


Fig. 9.—Position of the long-duration radiant among the stars.

During these years a considerable number of radiant points had been made out, and therefore the existence of a considerable number of streams or swarms had been suggested if not established. In 1863, Prof. H. A. Newton used these facts to strengthen the cosmical hypothesis.

The observations of Humboldt, modern observations, so to speak, were repeated, as we have seen, in 1833, on the same day (or one day later) of the same month on which Humboldt had made his observation in 1799, and again one day later in 1866 there was a recurrence of the same thing. Now these dates are separated by an equal interval of thirty-three years. The idea of periodicity was therefore suggested both for this and other displays, and gave rise to so great an interest in this question that an inquiry was set afoot as to whether falls had been seen before at previous intervals of thirty-three years, or whether it was a new thing seen first by Humboldt in 1799, or possibly by the Cumanese in 1766.

Prof. Newton took up the inquiry, and was soon able to show that the various chronicles of star-showers from the very earliest times, when properly discussed, indicated that the streams suggested by the observations since 1833 had really at variously-recurrent intervals since the beginning of astronomical observation given indications of their existence.[1] He especially indicated such cases of constant recurrences of showers in April, August, November, and December.

[1] *Silliman's Journal*, vol. xxxvi. p. 146, 1863.

The discussion of the dates of these showers in the early records showed a constant slow change of date in one direction or the other. This obviously demonstrated that the showers were independent of the tropical year—that is to say, of the earth's motion round the sun; and it is difficult to understand how a more definite proof of their cosmical origin could be afforded.

We may conveniently confine our remarks on this point to the inquiries relating to the "Leonid" swarm of meteorites which gives rise to the November display.

Newton and others found that we possess records, dating from A.D. 902, showing that about every thirty-three years since that time the heavens have been hung with gold. The Arab historian, Abu-l'Abbas ad-Dimashkî, chronicled the November star-shower of the year 1202 of our era in the following words, the while Chinese astronomers carefully watched the constellations in which the meteors appeared and vanished from the sight:—

"In the year 599, on the last day of Muharram, stars shot hither and thither, and flew one against another like a swarm of locusts; this phenomenon lasted until daybreak; people were thrown into consternation, and made importunate supplications to God the most High; there was never the like seen except on the coming out of the messenger of God—on whom be benediction and peace."

This table for the November display, from Prof. Newton, shows what the result of searching the old records was:—

Epochs of November Star-Showers.

Year.	Day on which the star-shower was seen.	Paris dates and hours.[1] d.	h.
902	October 13	12	17
931	16	14	10
934	14	13	17
1002	15	14	10
1101	17	16	17
1202	19	18	14
1366	23	22	17
1533	25	24	14
1602	28[2]	27	10
1698	November 9	8	17
1799	12	11	21
1832	13	12	16
1833	13	12	22
1863-68	14	13	14

These ancient records enabled Prof. Newton to place the planetary nature of the November ring beyond all doubt.

It is evident that if this ring crosses our orbit in a certain definite point in space, our earth will always traverse it when it occupies the same definite point of its orbit with regard to the stars, provided the ring does not change its place. But our ordinary year, called the tropical year, is affected by the precession of the equinoxes, as it is measured from equinox to equinox, so that we do not measure it by the stars, but by an empirical point called the first point of the sign Aries, which is actually at the present moment in the constellation Pisces. If we refer the recorded star-showers to the sidereal year, or a fixed equinox, we should find an almost absolute identity in the dates of their appearance if there were no perturbation, but we shall see subsequently that there is perturbation, and this is a final demonstration of cosmical origin.

If there is a swarm of meteorites falling in any particular direction towards the plane of the ecliptic these meteorites will take little account of the precession of the equinoxes or the tropical year; the earth must take the meteorites as she finds them. The one great jump in the table was due to the alteration of the calendar, as there was a difference of twelve days between the old and new reckoning. Prof. Newton, Prof. Adams, and others have given a complete demonstration that from the year 902 a swarm of meteorites has been encountered by the earth every thirty-three years or thereabouts, and nearly in the same part of her orbit round the sun.

By a study of the position and *lie* of the earth in her orbit we can see from what part of space these meteors, these more numerous swarms, come. Suppose, for instance, that at one

[1] H. A. Newton, *Bul. Ac. R. Belg.*, xvii. No. 6.
[2] In many countries the change from old to new style was made in this interval commencing from 1582 in Spain, Portugal, and Italy.

part of the earth's orbit there is a stream of meteorites plunging down nearly vertically towards the ecliptic; the earth in passing through them would receive the greatest number of blows on its exterior atmosphere on the hemisphere above the plane of the ecliptic at the time, while the other hemisphere would be entirely sheltered, so that the direction of the fall would be capable of demonstration by a consideration of the earth's direction and the relation of its surface to the plane of the ecliptic at the time.

The observations indicate that these bodies are moving towards the plane of the ecliptic, from its northern side, into that part of it through which the earth passes in her annual journey in November; they, in fact, are moving round the sun in an orbit inclined at a not very large angle—17°—to the plane of the earth's orbit.

Similarly, we might observe the August ring rising from one of its nodes, situated in the point of the earth's orbit occupied by our planet on August 10, not at a slight angle like the November ring, but at an angle of 79° or 80°.

It is important to make this point quite clear.

Let us conceive the sun and earth to be half immersed in an infinite ocean which will represent to us the plane of the ecliptic, and let us further for greater simplicity assume that the earth's motion round the sun (in a direction contrary to the hands of a watch) is performed in a circular path with the sun at the centre; let us, moreover, suppose the earth's path, or orbit, to be marked by buoys, remembering that astronomers define the position of a heavenly body in the plane by stating its *longitude*—that is, its angular distance, reckoning from right to left, from a particular start-point, as seen from the sun; and its *latitude*—that is, its angular height above the plane as seen from the same body.

Now, if it were possible to buoy various points of the earth's orbit in the plane of the ecliptic in the convenient manner before suggested, we should see the meteor-ring of "Leonids" meeting the waves of our hypothetical ocean, at a slight angle (17°), at the point of the earth's orbit occupied by our planet on November 14, the point where they pierce them being called the *node*. Where the other node lies, where the meteorites cross the plane again, we do not exactly know; we only know that they do not cross our orbit; if they did, another star-shower would occur in May.

Let us inquire into this point a little more closely. Let us, in imagination, connect the earth and sun by a straight line; at any moment the direction of the earth's motion will be at right angles to that line (or a tangent to its orbit); therefore, as longitudes are reckoned, as we have seen, from right to left, the motion will be directed to a point 90° of longitude behind the sun. The sun's longitude at noon on November 14 was 232°, within a few minutes; 90° from this gives us 142°, which, as we have seen, is precisely the longitude of the radiant point. This, then, is proof positive enough that in longitude at least the meteoric hail was fairly directed against, and as fairly met by, the earth.

But it will be asked, If the radiant point is situated in latitude 8° 30′, how comes it that the inclination of the ring is stated to be 17°? should it not rather be 8° 30′? To this question we may reply by another: How comes it that, when we are hurrying through a shower, we always incline an umbrella at a less angle with the ground than that formed by the falling rain? The answer is the same in both cases. In the case of the meteorites, if our motion in one direction differs little from theirs, they appear to us to fall at an angle which is also almost precisely half of their real one.

Similar ancient records relating to star-showers seen in March and April, and July and August, showed that the earth's longitude was always the same when they were observed, if it was referred to a *fixed equinox*. The constant longitude for the star-showers anciently recorded to have taken place in March–April corresponds to April 20·1d., 1850, and for a like number seen in July–August, August 9·0d., 1850.

Forms and dimensions of the orbit of the August meteors, all of them very steeply inclined to the ecliptic, were calculated among the many combined observations and determinations of heights of those meteors made at German Observatories to conclude their longitudes, in the years following the great November showers of 1832–33, by the German astronomer, Erman. *But an exact value of their velocity was still wanting*; and from an approximate measure of the velocity of the "Perseids," obtained from observations of a fine meteor of the shower in America on August 10, 1861, Prof. H. A. Newton found elements of the ring, concluding it to be not far from circular in form, and nearly perpendicular in its plane to the ecliptic.

It will be seen that the longitude for the showers recorded in October–November advances along the ecliptic from a fixed equinox with a uniform motion of 52″ per annum. Such a motion as this must be due to planetary perturbation, and hence we are in presence of cosmical phenomena.

It is to an American astronomer, Prof. Newton, that we owe the first investigation into the constitution of the November ring.[1] He first considered the question whether the ring is of uniform density, and whether it lies merely near our orbit; the variation in the brilliancy of the showers being caused by the action of the planets and moon on the earth and ring—the greatest perturbation of the earth being 9000 miles each way—sometimes throwing us into the ring, sometimes causing us to pass it without meeting it. He has shown, however, that the ring cannot be of uniform density throughout, but that, on the other hand, in one part of it there is a clustering together of the little bodies of which it is composed—a few stragglers being scattered along the rest of its circuit.

From other considerations he showed that the meteors revolve round the sun in a direction opposed to the earth's motion, the most probable time of revolution being, according to his first view, 354·621 days, our own being accomplished in 365·256 days. This is the same as saying that the annual motion of the group is $1 + \frac{1}{33 \cdot 25}$ revolutions. Consequently, the centre of the group is brought, on this view, into contact with the earth once in every 133 years, but the earth passes very near the centre four times in this interval.

On this view the orbit of the swarm would be nearly circular.

With regard to the rings generally, Prof. Newton made out in 1865[2] (1) that all the sporadic shooting-stars cannot belong to a narrow ring which has a diameter approaching in size that of the earth; and (2) that a large portion of the meteorites, when they meet the earth, are travelling faster than it, or else that the sporadic meteors form a series of radiants at some distance from the ecliptic, and hence come from a series of rings considerably inclined to the plane of the ecliptic.

Further, he pointed out that the distribution of the orbits of the meteorites must be one or other of the following:—

(1) They may form rings passing near the earth's orbit at many points along its circuit (sporadic meteors may be outliers of such a ring).

(2) They may form a disk in the plane of the ecliptic.

(3) They may be distributed at random like the orbits of comets.

J. Norman Lockyer.

(*To be continued.*)

SCIENTIFIC SERIALS.

American Journal of Science, September.—Cambrian fossils from Mount Stephens, North-West Territory of Canada, by Charles D. Walcott. The fossils here studied were first discovered last year by Otto J. Klotz, and partly described by Dr. C. Romiger. A comparison with specimens from the Middle Cambrian Terrane of Central Nevada shows that the two faunas are identical, and that consequently the Mount Stephens remains should be referred to about the horizon of the upper portion of the Middle Cambrian system. Other discoveries near the Kicking Horse Pass on the Canadian Pacific Railway seem to show that this fauna extends all along the western side of the great Keweenawan continental area from Southern Nevada far into British America.—History of changes in the Mount Loa craters (continued), by James D. Dana. Here are studied the relations of Kilauea to Mount Loa, arguments being advanced to establish the independent origin of the former, contrary to the author's earlier views on the subject. But his old conclusion is confirmed that volcanoes are not safety-valves, but are rather indexes of danger, pointing out the parts of the earth's crust that are most subject to earthquakes. A contrast is also drawn between volcanoes of the Mount Loa and Vesuvius types, the discharges of the former being almost exclusively outflows, those of the latter upthrows of cinders combined with lava-streams.—On the formation of deposits of oxides of manganese, by F. P. Dunnington. The main object of this paper is to show that manganese sulphate has probably taken a very important part in the

[1] *Silliman's Journal*, Nos. 111 and 112.
[2] *Ibid.*, vol. xxxix.

formation of deposits of manganese ore.—Maxwell's theory of the viscosity of solids and certain features of its physical verification, by Carl Barus. These researches tend to show that Maxwell's theory is a version of Williamson's theory of etherification and of Clausius's theory of electrolysis. The transition made is from unstable groupings of atoms to unstable groupings of molecules. But while preserving minutely all the essentials of Maxwell's argument, the experiments here described go one step further, showing that viscosity is a phenomenon evoked by certain changes of molecular structure, the inherent nature of which is ultimately chemical.—On the origin of primary quartz in basalt, by Joseph P. Iddings. Here are described certain specimens of basalt occurring in the vicinity of the Rio Grande Cañon, which exhibit a remarkable number of porphyritic grains of quartz. A theory is proposed to account for the possible origin of this porphyritic quartz.—Mineralogical notes, by Geo. F. Kunz. Here are studied some specimens of phenacite and quartz pseudomorphs from Maine, a variety of transparent oligoclase and a cyanite from North Carolina, an apatite from New York, and an aragonite pseudomorph from Arizona.—An appendix of 42 pages contains a complete list of the late Asa Gray's writings, chronologically arranged and disposed in three categories: (1) scientific works and articles, 1834-83; (2) botanical notices and book reviews, 1841-87; (3) biographical sketches, obituaries, &c., 1842-88.

SOCIETIES AND ACADEMIES.

London.

Entomological Society, September 5.—Dr. D. Sharp, President, in the chair.—Dr. Sharp mentioned that he had received, through Prof. Newton, a collection of Coleoptera from St. Kilda, consisting of *Carabus catenulatus* (1), *Nebria brevicollis* (12), *N. gyllenhalii* (3), *Calathus cisteloides* (20), *Pristonychus terricola* (1), *Pterostichus nigrita* (71), *Pt. niger* (31), *Amara aulica* (4), *Ocypus olens* (1). The species being nearly all large Geodephaga, he thought probably that many other Coleoptera inhabited the island. He remarked that these specimens showed no signs of depauperation, but were scarcely distinguishable from ordinary English specimens.—Mr. South exhibited a melanic *Aplecta nebulosa* from Rotherham, bred with five others of ordinary form, and an albino of the same species from Devonshire; a very curious dark variety of *Plusia gamma*; two dark varieties of *Eubolia limitata* from Durham; *Dicrorhampha consortana* from North Devon.—Mr. Champion exhibited *Harpalus cupreus*, *Leptusa testacea*, and *Cathormiocerus maritimus* from Sandown, Isle of Wight.—Mr. Elisha exhibited the following Microlepidoptera: *Œncana atricapitana*, *turionana*, *juliana*, *derasana*, *capreana*, *pomonana*, taken off *Sorbus aucuparia*; *sodaliana*, *zephyrana*, *trigeminana*; also *Schiffermulleriella horridella*, *alpella*, *fuscoaurella*, *therinella*, and *semidecandrella*, on *Cerastium tetrandrum*.—Mr. Jacoby exhibited three boxes of Coleoptera, collected partly by Mr. Fruhstroffer, containing some rare *Cetoniadæ*, *Faussidæ*, &c.—Mr. E. Saunders exhibited *Amblytylus delicatus*, Perr., a new British bug, taken at Woking.—Mr. Jacoby mentioned that he had taken the larva of *Vanessa cardui* on a narrow white-leaved plant in his garden.—Mr. Enock mentioned that out of a batch of two males and six females of the Hessian Fly kept together, all six females had laid fertile eggs, so that each male must have impregnated more than one female.

Paris.

Academy of Sciences, September 24.—M. Des Cloizeaux in the chair.—Generalization of a theorem of Gauss, by M. J. Bertrand. This theorem is thus expressed: Whatever be the attracting body, the mean value of the potential at the different points of a sphere is equal to the relative potential at the centre of the sphere. The demonstration supposes the sphere to be exterior to the attracting body, and the present paper deals with the theorem when this condition is not fulfilled, and it is shown that by substituting for the full sphere a spherical surface the theorem still holds good.—Complement to the theory of overfalls, by M. J. Boussinesq. Various applications are given to the theory established in the previous paper (*Comptes rendus*, September 17, p. 513) regarding the influence exercised on the discharge by the velocity of the current at the overfall.—Observations of Brooks's comet (August 7), and of Barnard's comet (September 2), made with the 0·38 m. equatorial at the Observatory of Bordeaux, by MM. G. Rayet and Courty. The observations for Brooks's comet are for the period from September 5-17, those for Barnard's comet from September 11-17.—On the physiological action of *Hedwigia balsamifera*, by MM. E. Gaucher, Combemale, and Marestang. This plant, which has been classified and described by Descourtilz ("Flore des Antilles," iii. p. 263), belongs to the family of the Terebinthaceæ, and grows in the West Indies. The experiments on guinea-pigs and rabbits here described show that the alcoholic extract from the bark of stem and root is highly toxic, a dose of 0·161 gramme proving fatal. The aqueous extract is less toxic than the alcoholic, but both produce rapid and considerable lowering of the temperature, paralysis, and convulsions, spreading progressively from the lower part of the marrow to the rachidian bulb.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Lessons in Elementary Physics, new edition: Balfour Stewart (Macmillan).—Ungdomsskrifter, Första Serien, Första Häftet: Carl von Linnés (Stockholm).—The Frog, 3rd edition: A Milnes Marshall (Cornish, Manchester).—Primer of Micro-Petrology: W. Mawer (London).—Memory: F. W. Edridge-Green (Baillière).—Mathematischen Theorien der Planeten-Bewegungen: Dr. O. Dziobek (Barth, Leipzig).—Examples in Physics: D. E. Jones (Macmillan).—A Text-book of Physiology, 5th edition, Part 1: M. Foster (Macmillan).—The Centre of the Central Sea: J. N. Emra (Kegan Paul).—Johannes Kepler und der Tellurisch-Kosmische Magnetismus: Dr. S. Günther (Wien)—Synopsis of the Vertebrate Fauna of the Puerco Series: E. D. Cope (Philadelphia).—Morphologisches Jahrbuch, 14 Band, 2 Heft: C. Gegenbaur (Leipzig).—Zeitschrift für Wissenschaftliche Zoologie. xlvii. Band, 1 Heft (Leipzig).—Geological Record for 1880-84: Topley and Sherborn (Taylor and Francis).—The Calendar of the University College of Wales, Aberystwyth, 1888-89 (Cornish, Manchester).—The Analyst's Laboratory Companion: A. E. Johnson (Churchill).—Memoirs and Proceedings of the Manchester Literary and Philosophical Society, 4th series, vol. 1 (Manchester).—Photography for All: W. J. Harrison (Iliffe).—Ornamental Waterfowl: Hon. Rose Hubbard (Simpkin)—Jahrbuch der Meteorologischen Beobachtungen der Wetterwarte der Magdeburgischen Zeitung, Jarhgang vi. 1887 (Magdeburg).—Proceedings and Transactions of the Royal Society of Canada for the Year 1887, vol. v. (Dawson, Montreal).—Catalogue of Variable Stars: S. C. Chandler (Lynn, Mass.)—Report on the Condition of Growing Crops, &c., August (Washington).—La Zoología de Colón: J. I. de Armas (Habana).—Vierteljahrs-Wetter-Rundschau, Band i. Heft 3 and 4 (Mittler. Berlin).—Journal of Morphology, vol. ii. No. 1 (Ginn, Boston).—Mind, October (Williams and Norgate).—Journal of Anatomy and Physiology, October (Williams and Norgate).—The Geological Magazine, October (Trübner).

CONTENTS.

THURSDAY, OCTOBER 11, 1888.

THE ZOOLOGICAL RESULTS OF THE "CHALLENGER" EXPEDITION.

Report on the Scientific Results of the Voyage of H.M.S. "Challenger" during the Years 1873–76, *under the command of Captain George S. Nares, R.N., F.R.S., and the late Captain Frank T. Thomson, R.N.* Prepared under the superintendence of the late Sir C. Wyville Thomson, Knt., F.R.S., and now of John Murray, one of the Naturalists of the Expedition. Zoology—Vol. XXVI. Published by Order of Her Majesty's Government. (Printed for Her Majesty's Stationery Office, and sold by Eyre and Spottiswoode, 1888.)

THE first memoir in Vol. XXVI. is the second part of the Report on the Crinoidea collected during the voyage, and is by Dr. P. Herbert Carpenter. The first part treated of the Stalked Crinoids: this treats of the Comatulidæ.

Since Müller's well-known memoir on the genera and species of the Comatulidæ, no systematic work on this interesting group has until now made its appearance. Several new species have no doubt during these forty years been described, but with the publication of each the subject became more and more confused, and the painstaking and laborious revision of the known species forms by no means the least important portion of the present memoir. In it we find the result of many years' careful study of the "Comatulæ," based not only on the collections made by the *Challenger*, but on those made by other Expeditions in various seas, and on the examination of almost all the types to be found in European or American Museums.

Lamarck's familiar and appropriate name Comatula is retained by the author as the name of a family of Neocrinoids, which now contains six genera with recent species, viz. Antedon, Actinometra, Atelecrinus, Eudiocrinus, Promachocrinus, and Thaumatocrinus: of these genera over 180 species are now known, a large advance beyond the 35 species referred to by Müller, and of the former number 88 are described in detail as new from the *Challenger* collections. The author remarks that even this large number is considerably lower than that mentioned in his preliminary Report, but adds that the large experience gained by the examination of numerous specimens has obliged him often to write under one specific name forms which at first had seemed most distinct.

This Report is morphological, as naturally the opportunity was wanting for dealing with details of development. We have first a general introduction, in which there is a sketch of the progress made from the days of de Fréminville; next a chapter on the centro-dorsal plate and calyx, in which there is no lack of controversial matter. The errors of Vogt and Yung might better have been referred to in footnotes, and the continuance of the author's descriptions would not then have been interrupted. It scarcely concerns the reader who is studying Carpenter to know what "the student of Vogt and Yung" would or would not learn from their writings.

The chapter on the geographical and bathymetrical distributions is an important one. Our present knowledge of the recent species is too imperfect for any generalization respecting their geographical distribution or the origin of specific types. The species occur in immense abundance over certain large areas, such as the Caribbean Sea, and more especially the Eastern Archipelago and Australasia. The species of other seas have been made known to us by the dredgings of the *Challenger;* and other collections, both from the Arctic and sub-Arctic seas and from the Southern Indian Ocean, have yielded some valuable information. Although abundant near the coasts in the Arctic Ocean and on both sides of the North Atlantic, no species has been dredged at a greater depth than 800 fathoms in the Atlantic, nor were any forms met with in either of the *Challenger's* two traverses of the North Atlantic; and, while one species is recorded from Madeira and the Canaries, none have as yet been found at the Azores, Cape Verdes, or Bermudas. The two Mediterranean species range as far north as Scotland. In the Florida Channel, and in the Caribbean Sea, Comatulæ abound. None are known from the African coast, between Cape Verde and the Cape of Good Hope, except one species met with at the equatorial island Rolas. The only Actinometra common to both sides of the Atlantic is found at St. Paul's Rocks. Some few of the Caribbean species extend therefrom down the South American coasts to Cape Frio; while, in mid-Atlantic, species have been dredged at moderate depths off Ascension, St. Helena, and Tristan d'Acunha. Closely allied to the North Atlantic species are those found at Heard Island and Kerguelen. Various species are found at Simon's Bay, Natal, Madagascar, Mauritius, Seychelles, Zanzibar, Red Sea, Kurrachee, Ceylon, Bay of Bengal; while in the seas of the great Eastern Archipelago they occur in most bewildering confusion. No species as yet have been taken on the coasts of New Zealand—though one or two approach the East Cape of the North Island—nor at Tasmania. Two species are recorded from the Straits of Magellan, and single species are known to occur at Chili and Peru; but there are none apparently on the western shores of North America. In the Pacific the species are extremely rare. While essentially littoral forms, three species were found at depths of from 345 to 755 fathoms, from the green mud off the Japanese coasts; and one, *Antedon abyssicola*, from a depth of 2900 fathoms, at Station 244 in the North Pacific.

So far as present knowledge goes, the Comatulidæ first appeared in the time of the Middle Lias, and were thus of later date than the Pentacrinidæ; they were fairly abundant in the Jurassic and Cretaceous epochs, especially so at certain periods. The recent forms occupy an immensely more extended area than the extinct ones, for, with the exception of a species of Antedon from Algiers, and another from Syria, no fossil Comatulid has been found out of Europe, not even in the Indian Tertiaries, otherwise so rich in Echinoderm remains; and while none are to be found in America, it is not without interest to note that Pentacrinoid remains are very common at certain horizons of the Jura Trias over wide areas of the western territories, thereby indicating that the conditions of that age were not altogether unfavourable to the existence of Crinoid life. The Middle Lias of France contains two species of Antedon, the oldest yet known, and the genus occurs together with Actinometra, in the Lower Oolites of both France and England; while if *Bourgueticrinus ooliticus*, McCoy, is a Thiolliericrinus, as supposed by de Loriol, then it is the earliest known species of this remarkable genus.

The fifth chapter is on the classification of the family, and is followed by the descriptions of the specimens. In a seventh chapter there is a detailed account of the bathymetrical distribution, and a station list of all the "Comatulæ" which were obtained by the various British Expeditions for deep-sea exploration between the years 1868–82. Appended to this is a list of all the known living species of Comatulæ, with their distribution in depth and space. As to the latter, all the principal stations are given. In the analysis of this list (p. 383) the total number of living species is given at 180, but from the list itself there would seem to be 188 species. Possibly the seven additional species of Antedon and the one species of Actinometra named but not described may account for this discrepancy. The Report is accompanied by seventy plates.

In congratulating the author on the successful accomplishment of his onerous task, we allude to his apology for the delay in its publication to state our conviction that none such was needed. Investigations like those here recorded might be more quickly accomplished were it possible to devote to them the whole working hours of the investigator's life; but when instead they have to be carried on during the hours of rest from arduous professional duties, hours that might more prudently have been devoted to repose, the case becomes quite different, and the wonder to us is that so much has been done within the time.

The second memoir in the volume is a Report by Sir Wm. Turner on the Seals collected during the voyage. In the first volume of these Reports, Sir W. Turner's Report on the Bones of the Cetacea which had been collected by the Expedition appeared. In the present Report we have detailed descriptions of the species of Macrorhinus, Leptonychotes, Otaria, and Arctocephalus, procured at the Kerguelen and Heard Islands, off the Falklands, in Messier Channel, and at Juan Fernandez. This is followed by an outline of the classification of the Pinnipedia, in which the diagnoses of all the genera and those of most of the known species are given.

In a third part there is a description of the brain of the elephant seal and of the walrus, with a comparison of the convolutions of the brain of the seals and walrus with those of the brains of the Carnivora, and of apes and of man. Part IV. gives an account of the visceral anatomy of the elephant seal. In an appendix there is an elaborate account by Dr. W. C. Strettell Miller of the myology of the Pinnipedia. Ten plates accompany this Report.

The third and last memoir in this volume is an exceedingly interesting supplement to his Report on the Actiniaria, by Prof. Richard Hertwig.

This supplement contains a description of additional specimens found from time to time as the various other groups of marine forms were being worked out. Amongst the material occurred species previously described, but enabling in a few cases fresh details to be added. Several, however, represented new and interesting genera, but in some cases the material was in so bad a state of preservation as to preclude description. Prof. R. Hertwig's Report was published in 1882, and since then Andres's monograph of the Actiniaria has appeared. Some criticisms on his classification preface the description of the new species; and a synopsis of the Hexactiniæ according to Hertwig's views, is given.

In the description of genera and species we find an account of a new species of Moseley's genus Corallimorphus, *C. obtectus*. It was found at Station 157, and on it Hertwig in his Report had chiefly based his description of *C. rigidus*, Mos., the type specimen of which latter has now been found. A new genus, Ilyanthopsis, is established for a single specimen from the Bermudas; it seems in shape intermediate between Aiptasia and Anemonia; it was attached. Aulorchis is a new genus belonging to the group of forms devoid of tentacles, the specimen (*A. paradoxa*) was found at Station 299, at a depth of 2160 fathoms. With the assistance of Dr. Erdmann, a revision of the Zoantheæ is given, based on an examination of the condition of the cœnenchyma, arrangement of mesenteries, structure of sphincter, condition of integument, and colonial formation. The solitary forms are relegated to Sphenopidæ, the colonial to Zoanthidæ, of which five genera—Zoanthus (Cuv., p.p.), Mammilifera (Lesueur), Epizoanthus (Verrill), Polythoa (Lamx.), and Corticifera (Lesueur)—are recognized. In an appendix a new genus and species is described, *Stephanidium schulzii*, found off Zebu, which appears to belong to the Zoantheæ, but differs in the absence of incrustations and the non-formation of a colony.

We notice one defect in this memoir, that the references to the authorities for known genera and species are omitted. There are four plates representing the new forms.

OUR BOOK SHELF.

A Bibliography of the Foraminifera, Recent and Fossil, from 1565 *to* 1888. By C. Davies Sherborn, F.G.S. Pp. i.-viii. and 1–152. (London: Dulau and Co., 1888.)

THE attention of naturalists for many years has been drawn to the minute animals of the sea, and with increasing interest as they have become better known by researches as well in abyssal as in shallow waters. Their fossil representatives have also long been noticed and extensively sought for in very many strata of different ages in various parts of the world.

The Foraminifera are among these multitudinous objects of interest to the microscopist, and through him to the naturalist in general, and the geologist in particular.

The simplicity of structure in the Foraminifera, and, at the same time, their manifold and indeed interminable varieties of form, often symmetrically elegant, have given rise to numerous namings and descriptions, often without adequate figures. Hence their nomenclature has been confused among the multitude of authors who have either mentioned, or more fully treated of, these minute organisms. Consequently, for a basis in determining the relative value of the so-called species, their right names, and order of discovery, a bibliography of the Foraminifera, having long been desiderated, was attempted by different writers in 1848, 1854, 1858, 1859, 1878, 1884, and 1886–8; but each of these catalogues was imperfect. We are pleased to be able to say that a complete list of the books and papers treating of Foraminifera is now before us, combining accuracy and fullness of detail as to title, author, date, size, and place of publication. A short note of explanation or pertinent remark is in many cases added to the entries of the rare and little-known publications. Mr. Sherborn thus enumerates about 700 authors, with full title of book or memoir, carefully systematic abbreviation of titles of periodicals, and place of publication as given in the originals. Notices and general reviews having original information are included. Mr. Sherborn has examined all the works he has catalogued, with very few exceptions, and these are properly marked "not seen." The authors most prolific of memoirs are Brady, Carpenter, Carter, Dawson, De la Harpe, D'Orbigny, Ehrenberg, Folin, Fornasini, Gümbel, Haeusler, Hantken, Karrer, Jones, Munier-Chalmas, Neugeboren, Parker, Reuss, Robertson, Schlumberger, Schultze, Seguenza, Soldani, Stache, Terquem, Terrigi, Uhlig, Van den Broeck, Wallich, and Williamson. Former lists have evidently been carefully collated and corrected: and the life-dates (birth and death) of deceased authors have been entered as far as possible.

Several of the older papers are now catalogued for the first time, such as "Camerarius's papers, 1712 and 1717; Klein's, 1754; Schroeter's, 1803; and Wulfen's, 1791"; we also find "the correction of the hitherto inaccurate references to Spengler's papers; the original place of publication of Modeer's letter to Soldani; and Ricca's 'Discorso,' with the engraved portrait of Soldani"; and, "among those of scientific importance, ... the earlier issue of Fichtel and Moll (which carries back their scientific names five years); D'Orbigny's list of the Foraminifera of the Vienna Basin, published by J. von Hauer seven years before the full description appeared; the note on D'Orbigny's 'Planches inédites'; Boué's paper on the Nummulites; and Silvestri's rare and interesting paper on Soldani's 'Testaceographia.' For the first time, too, an endeavour has been made to enumerate the important memoirs published by the Hungarian authors with some approach to completeness."

The whole work has been conscientiously done, with scrupulous exactness; and the industrious author has made it a labour of love for several years, since he began to study Foraminifera. Having so full a knowledge of the subject, he might with advantage, we venture to think, give further aid to students and others by publishing an index and synonymy of all the recorded genera and species of Foraminifera.

In the preface to the bibliography, Mr. Sherborn fully acknowledges the help he has received from his many friends at home and abroad; and he refers to such analogous and collateral bibliographies as have been aids in his research. This work will without doubt be fully appreciated by biologist and palæontologist; and we cordially agree with the author in his remark that "sincere thanks are due to Mr. F. Justen (Dulau and Co.), to whose generosity and scientific sympathies I owe the publication of my manuscript." T. R. J.

Earth Knowledge. Part II. By W. J. Harrison, F.G.S., and H. R. Wakefield. (London: Blackie and Son, 1888.)

THIS book, in conjunction with the companion volume issued a few months ago, is chiefly intended for the use of students preparing for the Science and Art Department's examinations in Physiography. The book is far too small for its subject, and in consequence, only very bare outlines of the different branches of the subject can be given, and much is omitted which we should expect to find. It is scarcely possible, for instance, to give an adequate amount of information about the sun in half a dozen small pages; yet the authors have attempted to do this, and the result is what might be expected—namely, a very scanty chapter. No mention is made of the fact that the corona is of variable form, and since only one drawing is given, a student would be likely to infer that its form is constant. Again, the possibility of observing prominences whenever the sun is visible, and the peculiarities and variability of sun-spot spectra are not touched upon at all. No chapter on the sun can be regarded as complete which does not treat of the various solar phenomena in relation to the sun-spot period.

Again, the classification of stars according to their spectra (p. 78) is not treated nearly so fully as its importance demands. Notwithstanding the fact that there are two distinct kinds of red stars, one giving indications of metallic fluting absorption, and the other of carbon absorption, we are simply told that in the red stars the lines are more numerous than in stars like Arcturus (p. 79).

On p. 126 we read:—"Although the sun's mass is so very much greater than that of the moon—being nearly sixty million times as great—yet the tide-producing force of the sun is only about seven-sixteenths that of the moon, because the sun is nearly 400 times farther off the earth than the moon." Although this statement is quite true, a little further explanation is necessary to make it consistent with the arithmetical fact that sixty millions is greater than the square of 400. It is only fair to say, however, that the importance of considering the differential attractions of the sun and moon on opposite sides of the earth, instead of the total attractions, is well brought out with regard to the precession of the equinoxes.

On the whole, the drawings are excellent, but that on p. 29, showing the action of the spectroscope, is rather misleading; we would remind the authors that the slit is usually placed in the principal focus of the collimating lens, and that there is nothing to converge the rays of light to a point inside the tube.

Without the aid of a well-informed teacher, the book is far from sufficient to fulfil the purpose for which it has been written.

An Introduction to the Science and Practice of Photography. By Chapman Jones, F.I.C., F.C.S. (London: Iliffe and Son, 1888.)

WE have here quite a new departure from the ordinary books on photography, the subject being treated not from the mechanical but from the scientific point of view, and the author has succeeded in placing before us a very useful work.

The volume is divided into three parts. The first consists of fifteen chapters, the more important among them treating of the transmission and intensity of light, reflection by plane and concave mirrors, refraction of light and the forms and properties of lenses, &c., concluding with a chapter on the spectroscope, colour-sensitiveness, and the absorption of light. In Part II. are described various forms of cameras, camera-stands, exposure-shutters, followed by some very interesting chapters on the history and special properties of the many and various forms of lenses. Part III. consists of twenty-four chapters extending over 100 pages, in which are described the manufacture of collodion and gelatino-bromide plates, and all the different modes of developing, printing, toning &c., including carbon-printing, Woodburytype, and other photo-mechanical processes.

In the appendix are tables of English weights and measures, and a comparison of them with the metrical system, preceded by an explanation of the methods of testing lenses. The volume is well illustrated, and the varied information contained in it ought to give it a wide circulation.

Numerical Examples in Practical Mechanics and Machine Design. By Robert G. Blaine, M.E. (London: Cassell and Co., Limited, 1888).

IN this volume there is an excellent collection of examples, the teaching power of which has already been tried by students attending the lectures at the Finsbury Technical College, who, as is stated in the preface, written by Prof. John Perry, have worked through them and obtained "a real good working knowledge of the application of the principles of mechanics and machine design; . . . their knowledge was always ready for use."

The examples, as a rule, are thoroughly practical, and may be taken as illustrating Prof. J. Perry's book on "Practical Mechanics," and Prof. Unwin's book on "Machine Design."

To make the volume more complete, useful rules and constants, together with tables of sines, cosines, tangents, and cotangents, of angles from 1° to 45°, are added, concluding with a table of the squares, cubes, square roots, cube roots, and reciprocals of all numbers from 1 to 100, and of approximate fifth roots from 1 to 1000.

A Text-book of Physiology. By M. Foster, F.R.S. Fifth Edition. Part I. comprising Book I. (London: Macmillan and Co., 1888.)

THIS work was originally published in 1876, and it has become so widely known that we need not now do much more than note the appearance of the first instalment of a new edition. In this edition—the fifth—considerable changes and additions have been made. The changes, however, do not affect the character of the book; and Prof. Foster explains that the additions, with the exception of the histological paragraphs, are caused, not by any attempt to add new matter or to enlarge the general scope of the work, but by an effort to explain more fully and at greater length what seem to him to be the most fundamental and most important topics. He has introduced some histological statements, not with the view of in any way relieving the student from the necessity of studying distinct histological treatises, but in order to bring him to the physiological problem with the histological data fresh in his mind. Hence in dealing with the several histological points the author has confined himself to matters having a physiological bearing. This first part will be followed as soon as possible by the second and third parts.

The Analyst's Laboratory Companion. By Alfred E. Johnson. (London: J. and A. Churchill, 1888.)

DURING the past four years, Mr. Johnson has had in everyday use in the laboratory a manuscript book of factors and tables. The work grew by constant additions, made as required; and in the end, as he explains in the preface, it became complete enough to encourage him in the belief that it might prove useful to analysts generally. Accordingly he has issued the present little volume, and no doubt he is right in thinking that the large amount of labour involved in the calculation of the many original tables here published may be found to save much of the time otherwise required by the analyst in working out the results of analysis. For the convenience of students not well acquainted with logarithms, of which he has made free use, he has given an account of them, adding examples fully worked out and chosen so as to include and explain the difficulties generally felt in connection with this subject.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Prophetic Germs.

I REGRET to find that I put an erroneous interpretation upon the phrase "non-significant organs," as used by Prof. Ray Lankester. I never doubted that it meant organs or structures which were non-significant in respect to actual use; that, in short, it was his phrase for what other men have variously called aborted or rudimentary organs. He now explains that "non-significant," in his terminology, means any variation from hereditary forms which is fortuitous—as unknown in respect to its origin as it is in respect to its actual or future use. Although I see no value in this phrase as descriptive of anything that exists, I see great value in Prof. Ray Lankester's admission that natural selection cannot act upon any structure which is not already developed up to the stage of actual use. This is really all I want for my previous argument, because all organs whatever do actually pass through rudimentary stages in which actual use is impossible. In no possible case, therefore, can selection explain the origin of any organic structure. I rejoice to find Prof. Ray Lankester denouncing as "an absurdity" the idea that "congenital variations are selected when they are not of any actual use." It must therefore be quite according to the admitted constitution and course of Nature that we should find organs "on the rise," as well as organs "on the wane." All germs must be prophetic of their future use, so long as they are in germinal stages; and, if evolution be true, the world ought always to have been full of them, and ought to be full of them now, unless the creative or evolutionary work has been arrested, at least locally, and for a time. ARGYLL.

Inveraray, Argyllshire, October 8.

The Geometric Interpretation of Monge's Differential Equation to all Conics.

WITH reference to the remarks of "R. B. H." (NATURE, June 28, p. 197) on my interpretation of the differential equation to all conics, I wish to point out that the objections he seems to take do not appear to be well founded. The difficulty he finds is that the geometrical interpretation given amounts to the fact that "a conic is a conic." But it is easy to see that there is no peculiarity in this; it arises simply from the well-known fact that all the geometrical properties of any given figure are inter-dependent: one of them being given, the others may be deduced as legitimate consequences from it. "R. B. H." takes the proposition which constitutes my interpretation, and then, coupling it with the other theorem that the osculating conic of any conic is the given conic, comes to the conclusion that a conic is a conic, and, apparently, he takes it to be very strange; but, as a matter of fact, given *any two* properties of a conic (or of any other curve), we can only come to the conclusion that the conic is a conic (or that the given curve is what it professes to be). Take, for example, the geometric interpretation of the differential equation of all right lines, which is $q = 0$; it simply means that the curvature vanishes at every point of every right line, which is equivalent to the fact that a straight line is not curved, or that a straight line is a straight line. There is certainly nothing strange in this: it is the legitimate effect of the process employed. Would "R. B. H.," on this ground, reject the geometrical interpretation of the differential equation of all straight lines? Surely the process is nothing but a piece of quite unobjectionable verification. Similarly, the differential equation of all circles, $(1 + p^2)r - 3pq^2 = 0$, means that the angle of aberrancy vanishes at every point of every circle. Combining this with the self-evident proposition that the normal and the axis of aberrancy coincide in the case of a circle, we may come to the conclusion that a circle is a circle; but I submit that this is really a verification, and surely no ground for rejecting the interpretation. Indeed, the question whether such processes are to be regarded as verifications or not seems to me to be much the same question whether every syllogism is a *petitio principii* or not. But as I have elsewhere, in the papers referred to in my last letter (p. 173, *ante*), fully discussed what a geometrical interpretation properly ought to be, I need not enlarge further on this point.

As to the difficulty which "R. B. H." feels in drawing a curve at every point of which the radius of curvature vanishes, I may remark that this is a "limiting case," and the matter becomes clear when my interpretation is paraphrased thus: "If the radius of curvature of the aberrancy curve of a given curve vanishes at every point, that curve degenerates into a conic."

Finally, I fail to see why an interpretation is to be rejected simply because the property it enunciates happens to admit of an easy verification. The conic has an infinite number of properties, and the chief difficulty in discovering the geometrical interpretation of its differential equation has been to find out which of these numerous properties is adequately and most appropriately represented by the Mongian equation. The question has been, what fact in the history of the conic, if I may say so, is most intimately associated with the vanishing of the Mongian; and that fact, I believe, is given in my interpretation. Whether the fact admits of an easy verification or not seems to me to be wholly foreign to the question.

Calcutta, July 27. ASUTOSH MUKHOPADHYAY.

Upper and Lower Wind Currents over the Torrid Zone.

AFTER my arrival in China in 1883, I made inquiries, among persons who had kept meteorological registers, concerning the direction from which clouds usually come here, but was told that they came from all directions without any apparent order. But the observations made during January 1884, printed in the Weather Report published on February 11, showed at once clearly that the lower clouds came from the east, and that the directions veered with increasing height, the highest clouds coming from the west, as explained in the text of the Annual Weather Report published on February 17, 1885. This might have been expected in analogy with what obtains in cyclones, as the trade-wind blows into the calm belt as if this were the centre of a depression drawn out to extend round the whole earth near the equator.

The Hon. R. Abercromby, to whom my Reports were sent without delay, convinced himself of the truth of those remarks during a tour round the world, and addressed a letter to NATURE on the subject on October 26, 1885, but it is of importance that the subject should be investigated at fixed observatories within the tropics, where hardly enough attention has hitherto been paid to the movements of clouds, to judge from what has hitherto been published.

In the Annual Weather Report for 1885, it is stated that, from June to September inclusive, cirri come from two different directions—from about north-east while a typhoon is in existence somewhere, their direction often backing from about east to north while the centre of the typhoon is yet over 700 miles away; and from about west when there are no signs of a typhoon. But cirri are rarely seen in summer except before typhoons, through whose agency vapour is evidently carried up to the higher regions of the atmosphere. It is, however, to be expected that the existence of the southerly monsoon (caused by the low barometer in the northern part of the Chinese Empire) during the summer to some extent influences the movements of the clouds.

The following table exhibits from four years' observations (1884 to 1887 inclusive) the average directions from which the wind comes at the Observatory, about 150 feet above M.S.L., and at the Peak about 1850 feet above M.S.L., as well as the average directions from which the upper and lower clouds come, but the difference between the latter is so great that intermediate directions will be missed:—

Month.	Obs.	Peak.	Lower C.	Upper C.
January ...	E 11° N ...	E 10° N ...	E by S ...	W by S
February...	E 15 N ...	E 17 N ...	E by N ...	W
March ...	E 4 N ...	E 17 S ..	ESE ...	W by S
April ...	E 3 N ...	E 30 S ...	SE ...	W by S
May... ...	E 11 S ...	E 44 S ...	SSE ...	WNW
June... ...	E 51 S ...	E 67 S ...	S by E ...	NNW
July	E 46 S ...	E 87 S ...	S by E ...	NE
August ...	E 72 S ...	S ...	S ...	NE
September	E 12 N ...	E 1 N ...	ESE ...	NNE
October ...	E 15 N ...	E 8 N ...	E by N ...	W by S
November	E 28 N ...	E 19 N ...	ENE ...	W by S
December	E 26 N ...	E 18 N ...	E by N ...	WSW
Mean ...	E 6 S ...	E 22 S ...	E 30° S ...	W 33° N

If an observer outside the earth were to determine the period of this planet's rotation by observing spots formed by clouds, he would obtain different values according to the level of the respective cloud-layer, just as we obtain different values for the period of rotation of Jupiter from observations of different classes of spots. In the case of the earth, the observation of the highest clouds near the equator might possibly furnish a value of the period too short by a tenth, and there is no doubt it would be different nearer the Poles. W. DOBERCK.

Hong Kong Observatory, August 11.

The Natural History of the Roman Numerals.

SOME time ago I had the pleasure of reading in your journal (vol. xxxvi. p. 555) an interesting article by Mr. Lymburn on the above subject. In this the writer shows the probable evolution of the X ten, from the V hand, and thence the broad arrow, ↑. As the Scandinavians used this arrow sign, calling it *tir* or *tyr*, as an equivalent for T in the Runes (see Taylor, "The Alphabet," vol. ii., p. 18), it is therefore connected with the Greek *tau*, the headless cross, the X of the Semitic languages. I have no doubt that many of your readers take an interest in anything bearing on this subject. This is my apology for calling their attention to an article published in the last volume of Transactions of the New Zealand Institute,[1] wherein I break new ground by showing that the word *tau* was known in Polynesia as a cross, as ten, and probably as meaning "writing."

I have given, in the different dialects of New Zealand, Samoa, Tonga, Hawaii, &c., the meanings of the word, and shown its entry into other compound words. A brief *précis* runs as follows:—

Tatau (*ta-tau*) is the Tahitian word which Cook brought to us, and is better rendered by his spelling *tattow* than by our English *tattoo*. In Maori, *tatau* means to count, to repeat one by one; but in Hawaiian it means to write, to make letters upon, to print as upon *tapa* (native cloth) as in former times. In this Hawaiian, *tau* means to dot, to fix the boundaries of a land or country, to give publicity to a thing. In Tahitian, *tatau* means not only to tattoo, but to count, number; in Samoan, *tau* is to count, and in Marquesan, *tatau* to reckon. In composition, too, it enters into many words, such as teacher, pupil, genealogy, &c., and it seems impossible but that the tattooing (at one time done in "three-marks" and arrow-heads) meant some kind of character or script.

As to the numeral "ten," I bring some interesting evidence which I cannot condense.

As to the figure of the cross being used as a sacred sign, there are innumerable evidences to that effect in the Polynesian islands; notably that the Southern Cross is called in Tahitian *tau-ha* ("four-cross"), and that the cross X was the taboo sign in front of Hawaiian temples. I have since learnt that in the Solomon Islands the cross taboos anything to the chief.

Wellington, N.Z., August 5. EDW. TREGEAR.

Indian Life Statistics.

THOUGH several weeks have now elapsed since Dr. Hyde Clarke's inquiry about the effects of lucky and unlucky times and seasons upon the Indian birth-rate was published (in NATURE of July 26, p. 297), none of your readers in England who happen to be acquainted with India have come forward to answer it. I therefore write to point out that, though the times of Hindu *marriages* are to a very great extent controlled by supposed lucky or unlucky days, months, or years, these have nothing whatever to do with variations in the birth-rate, for the usual age of marriage of girls is from eight to ten years, and child-bearing at the earliest does not commence before twelve or thirteen.

With regard to the *Holi* and other religious festivals, I have it on the authority of Mr. J. C. Nesfield, Inspector of Schools in Oudh, who has made a life-long study of Hindu castes and their customs, that, whatever the origin and primary significance of the *Holi* may have been, it is not now connected in any special manner with the multiplication of the species. The religious ceremony to which the Hindu looks for the furtherance of his desire for offspring is the *Durga Pujah*, or worship of the consort of *Shiva*, which is the occasion of the annual family reunion all over Bengal. In the Upper Provinces a totally different festival is celebrated at the same time of the year—the *Rām Lila*, a sort of dramatic performance or mystery-play, commemorating the expedition of Rāma to Ceylon for the recovery of his lost wife; but Mr. Nesfield says that during the *Rām Lila* some member of every family is specially set apart to conduct a ceremonial worship of *Kāli*, or *Durga*, ending with the sacrifice of a male kid, and that the object of this ceremony is to obtain the favour of *Kāli* and her consort for the continu-

[1] Trans. N.Z. Inst., vol. xx., "Ancient Alphabets in Polynesia," by E. Tregear, F.R.G.S. (London: Trübner and Co.)

ance of the family. Now the *Durga Pujah* and its equivalent ceremony in Upper India occur in October, *i.e.* at the beginning of the healthy season with abundant food-supplies. This is one more instance of the perfect adaptation of the Hindu religious calendar to the natural changes of the seasons.

S. A. HILL.

Allahabad, September 9.

A Shell Collector's Difficulty.

CAN any of your readers help me in the following case? I am a shell-collector, and my minute and delicate species (*Diplommatina* and such like) are kept in glass tubes. I have lately observed that some of the tubes in the cabinets were becoming opaque; a milky efflorescence seemed clouding the *inside surface*. I found the same thing in a box containing about 100 that I had placed on one side. I then opened a box of 500 which had never been unpacked since they were received, some four years ago. *All* these are more or less affected! I then opened a third box, from another maker, and in this 500 I observed many beginning to be affected. What can be the reason? Each of these tubes is tightly corked, and I see the glass under the cork is *not affected*. I have tried various means to restore the clearness without avail. I have boiled some, and roasted some in the sun, steeped others in alcohol, oil, &c.; nothing seems to do any good. Can any of your scientific readers divine the cause, and suggest a remedy?

E. L. LAYARD.

British Consulate, Noumea.

"Fauna and Flora of the Lesser Antilles."

IN the article on this subject in NATURE of August 16 (p. 371), it is stated that Guilding discovered a *Peripatus* in Dominica many years ago. This is, I believe, an error, for Guilding's *Peripatus juliforme* was found by him in St. Vincent, an island to the south of Dominica, and the first specimen of *Peripatus* found in this island was, I understand, the one now in the British Museum, taken home by Mr. G. Angas.

The rediscovery of the Dominica *Peripatus* is rather curious. In 1883–84, at the special request of Prof. Moseley, I searched for the animal in all likely places, but did not succeed in finding any specimens. At that time Prof. Moseley and I were not aware of Mr. Angas's discovery. I mentioned my non-success to Mr. Ramage, and asked him to look out for the interesting animal, and, strange to say, soon afterwards his boy brought him three specimens, but Mr. Ramage has not been able to obtain any more. I employed the same boy after Mr. Ramage had left Laudat, and he brought me two specimens, and said that he could find no more although he had searched for several days. These two I sent to Prof. Moseley at Oxford. A few weeks ago another specimen was brought to me from the windward (or eastern) side of the island by the same boy, who found it about 300 feet above the sea, not far from the coast. Laudat is on the leeward side, at an elevation of about 2000 feet above the sea, and on the margin of the virgin forest. The six specimens of the Dominica *Peripatus* recently found may not belong to a new species, but the rarity of the animal is interesting. Had it been common in any degree, Mr. Ramage and I must have found it, but neither of us has succeeded in doing so.

Mr. Ramage, who has been labouring with unflagging zeal, leaves to-day for St. Lucia, but he will return here later on in the year, so as to continue his botanical work. His specimens of the forest flora form, I believe, the most complete collection that has yet been made in the island, and his enthusiastic work deserves recognition.

H. A. ALFORD NICHOLLS.

Dominica, West Indies, September 15.

Sun Columns.

WITH reference to the simultaneous appearance of five sun columns described by Mr. Brauner (August 30, p. 414), the following descriptions of three different manifestations of the phenomenon may perhaps be of interest.

April 19, 1887, 7.25 to 7.37 p.m., calm, sky clear except a smoky grayish haze low on the western horizon, behind which the sun had set. The solar rays concentrated into one perpendicular continuous beam of uniform diameter with the sun, and reaching to an altitude of about 20°. The beam sharply defined, and of a reddish tint strong enough to be detected behind the haze. Near the summit a few tinted strips of fine cloud forming an angle, and giving the whole the appearance, as described by the person who called my attention to it, of "a ship's mast and yards." No trace of side rays visible.

June 10, 1888, 8 to 8.25 p.m., sun set below horizon; to an altitude of about 10°, sky comparatively clear, only a little cirro-stratus; above this, to an altitude of 20°, the cirro-stratus much more dense, and in this part only was a sun column distinctly visible, terminating abruptly, and showing no trace in the cirro-cumulus above. In the lower 10° there was also no evidence of the column. It was at first of an old gold colour, then gradually changed to a deeper red by 8.15 p.m., when the clouds on both sides were suffused with the same tint, and by 8.27 it had disappeared.

These two cases I observed from my own residence; the third has been communicated to me by Mr. W. Manning, who was chief officer of the ship *Balmore* when he witnessed the phenomenon. Not having access to the ship's log, he could not give me the exact date and position, but it was some four or five years ago, "in about 25° or 30° S. lat., and from 120° to 130° W. long., during the first dog watch (4 to 6 p.m.), observed the sun at an altitude of about 25° of a dull red colour, with all its rays apparently drawn together and forming a pillar of light reaching from the sun down to the horizon, and about the sun's diameter in breadth." Mr. Manning told me that of all the curious sights he had seen at sea none had been so impressed on his mind as this sun pillar.

These are instances of continuous pillars from the sun upwards and downwards, one showing the half furthest from the sun only.

HY. HARRIES.

Rosebank, Hounslow, September 28.

THE REPORT OF THE KRAKATÃO COMMITTEE OF THE ROYAL SOCIETY.[1]

II.

AN appendix to Prof. Judd's section on the geologica aspects of the eruption embraces a series of data collected by Dr. Meldrum, F.R.S., of Mauritius, regarding the falls of dust and the occurrence of masses of pumice throughout the Indian Ocean in 1883–84, which he hac already communicated to the British Association in 1885 Mr. Scott's prefatory note thereon shows that while such data are of value in exhibiting the immense magnitude o the eruption they cannot help to throw much fresh ligh upon the question of the Indian superficial oceanic circulation, since the pumice was evidently affected almost as much by the motion of the air as by that of the water Thus, while a comparison of the two maps reveals a genera westerly drift in the direction of the well-known left handed circulatory system of the Southern Indian Ocean, a detached phalanx of pumice masses off the north-west coas of Australia in 1884 (in the second map) shows, as Mr Scott observes, a probable drift thither "before the north west monsoon which would prevail in those seas fron November 1883 to March 1884."

In one other point, however, apart from their genera interest, these data are valuable in confirming the genera westerly trend of all the ejecta at the time of the eruptior —a fact whose significance becomes subsequently so marked when dealing with the spread of the optica phenomena.

In the plates of geological sections which are appendec to this Part attention should be paid to (3) (4) (5) (6) o Plate 4, in which natural and artificial pumice and dus from Krakatão are compared, since they have an im portant bearing on Prof. Judd's conclusions.

Part II. of the Report, which deals with the air wave and sounds caused by the principal eruption of Krakatã on August 26 and 27, was prepared, under the directio of Lieut.-General Strachey, F.R.S., principally by Mr R. H. Curtis, of the Meteorological Office.

The air-waves, as apart from actual sounds, were on of the most extraordinary features of this unique out

[1] Continued from p. 542.

burst; for, while it is possible that similar waves were propagated through the atmosphere during great eruptions in former years, these appear to be the only instances recorded of anything of the kind on such a vast scale since the establishment of continuous self-recording barometers.

That air-waves caused by the sudden expansion of the erupting gases could leave a perceptible record on all the barometer traces as far as the antipodes of Krakatão, is of itself a sufficiently remarkable fact, but that such waves could record their passage back and forwards no less than seven times, is a circumstance which even now, five years after its occurrence, fills us with astonishment. A selection of forty-seven stations has been made, which, as far as possible, represent the habitable world; and the times of passage of the wave from Krakatão to the antipodes and from the latter back to Krakatão have been deduced by comparing the significant, and in many cases similarly-shaped, notches in the barometer traces.

Of course, where, as in the present case, the form of the wave itself was complicated, gradually became deformed, and was traceable for no less than 127 hours from its commencement, perfect accuracy in determining the precise moments of passage of the various phases could scarcely be expected. Yet it is evident on the face of it that a very high degree of accuracy has been attained, by which not only can the precise moment of the great outburst be determined by the simple process of calculating backwards, but also certain variations of velocity be traced in portions of the wave which took different routes over the globe.

The general pace at which the air-wave spread outwards in concentric circles from Krakatão as a centre, was 700 miles per hour, which is slightly less than the velocity of sound at zero Fahrenheit, viz. 723 miles. The entire circuit of the globe and back was thus made in about thirty-six hours. Also, by a careful comparison of times and probable errors, the probable moment of *the greatest explosion* is calculated to have been 2h. 56m. G.M.T., or 9h. 58m. local time, on the morning of August 27.[1]

This great explosion appears to have been not only the culminating point of the Krakatão eruption (the preceding minor outbursts appearing as a mere roughening of the barometer scale, or a series of moderate oscillations on that of the gasometer at Batavia), but owing to its surpassing intensity, a feature *altogether peculiar to this eruption*, and one by which it will always be distinguished from others, such as that of Asama (Japan) and Skaptar Jokull in 1783, or Tamboro in 1815, which, in respect of the amount of material ejected in the form of lava, and other effects, appear to have equalled if not exceeded it.

One of the most interesting results of this discussion of the Krakatão air-wave has been the discovery of its variation of speed according as it travelled *with* or *against* the earth's rotation. As a general fact it may be said that such variation is plainly traceable to the prevalent drift of the winds.

Thus in the extra-tropics the wave moving from west to east was accelerated, and that from east to west retarded, by about 14 *miles* per hour; while within the tropics the wave which passed through Mauritius and Loanda was affected in a precisely reverse manner, the passage eastwards being retarded, while that westwards was comparatively unaffected, the amount corresponding to an east to west wind of about 10 *miles* an hour. It is at least curious to notice, that on p. 35 of the "Motions of Solids and Fluids," by Prof. Ferrel (Washington, 1882), the value of the due E. to W. component of the trades between 15° N. and S. lat. is given as 10 *miles* per hour, while the mean of the W. to E. component of the anti-trades for latitude 45° at the earth's surface and a height of 3 miles above it, is exactly 14½ *miles* per hour.

[1] This differs by only 4 minutes from 10h. 2m., the epoch determined from fewer data by M. Verbeek.

The greatest general retardation took place in the Southern Ocean, possibly owing to the low temperature of the southern hemisphere in August. All these points are very distinctly shown in the diagrams.

As regards the actual sounds, the facts are without precedent. The unvarnished record reads like a fairy tale. When we are told that at distances of over 2000 miles from the volcano, the noise was like the firing of heavy guns, and that at numerous points of the Indian Ocean steamers were despatched in search of supposed vessels in distress, we are prepared to accept with less hestitation the numerous other collateral evidences of the enormous explosive energy which generated them.

The area over which the sounds were heard is roughly estimated at *one-thirteenth* of the entire surface of the globe. In other words, it was nearly equal to Europe and Africa together, or slightly exceeded that of both Americas. All these details are illustrated by numerous diagrams.

Part III., by Captain W. J. L. Wharton, R.N., F.R.S., deals with the so-called seismic sea waves generated during the eruption; one of which not only dealt death and destruction all over the Straits of Sunda, but travelled as far as Cape Horn, and possibly the English Channel.

It appears that there were two sorts of waves generated—one of long period (two hours), which alone recorded itself on the automatic gauges and travelled to great distances; and others of much shorter period, which were mostly confined to the immediate vicinity of the volcano.

The only hypothesis by which the facts can be reconciled, according to Captain Wharton, is that at the time of the greatest explosion, at 10 o'clock on August 27, "waves of both characters would be more or less synchronously formed," the longer wave being caused by upheaval, and the shorter ones, which caused the destructive effects in the Straits of Sunda, by the displacement due to ejected masses or fragments of the volcano falling into the sea all round it.

In proof of upheaval, which appears to be the only probable cause of the longer wave, Captain Wharton cites the generally shallowed condition of the sea immediately surrounding Krakatão, especially on the northern side.

We cannot, however, help observing that, according to Prof. Judd, the geological evidence is entirely against upheaval throughout the area; and the formation of the new shoals and islands is attributed by him *solely* to the piling up on the sea floor of the coarser matter, including the framework of the volcano, which was ejected during the explosive outbursts. It is a remarkable fact, indeed, that during the eruption there was no trace of any local seismic disturbance such as might be supposed to accompany an upheaval of the ground. A variety of peculiar effects were witnessed, such as clocks stopped, lamps broken, and houses cracked, but all of these were traceable to air and not earth vibrations.

The precise cause, therefore, of the long wave will, as Captain Wharton says, "ever remain to a great extent uncertain." One fact, however, remains clear—that both it and its minor predecessors were distinctly connected with corresponding explosions from the crater, which recorded themselves in unmistakable language on the gasometer pressure-gauge at Batavia. Whatever the precise proximate cause, therefore—whether slow upheaval, according to Captain Wharton, or the impact of falling matter, according to Prof. Judd—the action *commenced* with each explosion.

The height of the local manifestation of the great wave at 10 o'clock is estimated to have been 50 feet, though in places where it reached the shore it appears to have run up to 70 feet.

The terribly destructive effects of these shorter "superseismic" waves, of which this one appears to have been the greatest, are amply detailed in M. Verbeek's Report, and the accompanying views of the localities visited. They reached the above majestic height only in the immediate vicinity of the volcano, rapidly falling off in size at a comparatively short distance from the Sunda Straits.

The longer waves, with the original period of two hours, are traced by automatic and eye observations to have proceeded mainly in a westerly direction from Krakatão, being noticeable at Ceylon, all over the western part of the Indian Ocean, the south coasts of Africa and South America, the west coast of Australia, and possibly—though the evidence is not free from doubt—as far as the west coast of France and the entrance to the English Channel. In other directions, such as the China Sea, the Pacific, and the Gulf of Mexico, they do not seem to have been felt, the supposed indications not being compatible in any way with the times and distances.

As a general result, it may be said that the mean depths deduced by the formula $V = \sqrt{gh}$, from the best data for the speed of the waves, corresponded fairly with that given by the soundings, but in nearly every case the formula gave a smaller depth than the soundings. This and other circumstances lead us to conclude, not so much that the formula is incorrect, but that, with so few, and in some cases such badly placed, automatic gauges, and from such complex oscillations as seem to have occurred in many of those discussed in this section, it is scarcely possible to arrive at anything but a very rough approximation to the mean depths. The shelving of the bottom near land, which in many cases is not well determined, and the possible existence of ridges in mid-ocean, constitute obstacles to a determination of mean depth, which is all the passage of such waves can indicate. In so far, however, as they yield an approximate check of this kind on soundings, their observation ought to be encouraged by the establishment of more automatic gauges in suitable spots.

One very peculiar feature of the Krakatão long waves is that, while their original period when leaving Krakatão was two hours, they became subdivided (possibly by an interpolated series caused by reflection from the coast of Java) into waves of half this period; and, by the time they reached the North Atlantic, into waves of about one-quarter of this period. Their consecutive oscillations could thus only be identified with those of the original oscillations by doubling or quadrupling the observed periods.

Although at great distances from Krakatão the height of the largest long wave was, as might be expected, only a few inches; at such comparatively remote places through the more open route to the west as Ceylon and Mauritius, the higher and shorter waves made their presence felt to heights of several feet, and created considerable astonishment as well as damage in these localities.

Like the air and sound waves, the occurrence of seismic waves on such a scale and over such a wide area appears to have been quite unprecedented; and their discussion, like that of the former, will in the present case probably yield results of considerable value to hydrography as well as other branches of science.

(*To be continued.*)

FOUNDATIONS OF CORAL REEFS.

THE following extract from a letter from Captain Aldrich, R.N., H.M. surveying-ship *Egeria*, now employed in the Pacific Ocean, is interesting from several points of view.

". . . . The following morning at daylight (July 10) we picked up 268 fathoms (volcanic rock) some considerable distance southward of the Pelorus Reef. This, again, will involve a further search. Twelves miles to the northward the depth was 444, and two subsequent soundings at five-mile intervals gave 713 (ooze) and 888 (ooze). From here the soundings continued to grow shoaler, until in lat. 22° 51′ S., long. 176° 26′ W., we sounded in 335 fathoms (cinder), being close to the assigned position of the Pelorus Reef. The water deepened again to 719 (cinder), when we hove to for the night. On July 11 we continued about this position, the shoalest sounding being 246. On the 12th we continued the search, and by following up at quarter-mile intervals struck 95 fathoms late in the afternoon. Prepared a beacon, and the following day (July 13), after excellent star observations, sounded and shoaled as yesterday, and when the men were standing by to slip the beacon, discoloured water was reported from the mast-head; it was almost immediately seen from the deck, and by 9 a.m. the beacon was dropped in 24 fathoms, with a stretch of light-greenish water extending in a northerly and southerly direction for about half a mile. The whalers were lowered, and remained all day in this green water.

"Meantime more discoloured water was reported from aloft, and I sent Mr. Kiddle up with his glasses, and he verified the report; so, leaving the boats on the Pelorus, I went with the ship, and, after going two miles, I made out the small streak from the poop. It had remained as steady as possible, and had every appearance of being a very small shoal. The ship was taken to within 100 yards of it, and the dingy lowered to get a sounding on it; no bottom, however, could be got, so the ship was put in the middle of it and a sounding of 150 (no bottom) obtained. A bucket of this water was drawn and a bottle of it preserved, but I do not see anything in it to account for the light greenish colour, and it may be that the colouring matter may not lie actually on the surface; the fact remains, that this small patch was sighted at very nearly three miles distance from aloft, and that even when within 100 yards of it I believed it to be shoal-water, and that a sounding of 150 (no bottom) was actually obtained in the middle of it. On our return to the Pelorus, I was not, therefore, much astonished when I found that no very shoal water had been got by the boats. The ship was anchored in 14 fathoms, not far from the beacon, and the wire machines put into the whalers, and a search on bearings from the standard compass and mast-head angles carried on during the afternoon and on the next day, July 14. Nothing less than 14, however, was got, and I am under the impression that nothing less is to be met with, as the bottoms are loose ashes and cinder; so that, as in the case of the Graham Shoal, there may have been a shoal quite recently which does not exist now. I think that had there been anything dangerous about it we should have seen it, as anchoring in 14 fathoms mid-ocean caused many inquiring eyes to be cast around. . . . .

"Another curious thing about the greenish water is that I went over it all in the ship; and the line between it and the dark water was most distinct. Moreover, the shoalest sounding of 14 fathoms was not found in the light water, but in the dark water alongside it. There was no sign of coral among the bottoms brought up. . . My attention was pretty well occupied at this time, and i[t] did not occur to me to do more than have a bucket o[f] the water drawn from the green colour to preserve, which has been done. Afterwards, I much regretted that I did no[t] get specimens from different depths, as certainly this is [a] most curious instance of, in one case, picking up a shoa[l] from the existence of some colouring matter, not coral, and, in the other, of being almost positive that a shoa[l] existed where an actual sounding proved it not to do so. I can quite excuse a man reporting a shoal under suc[h] circumstances, and it may be that a good many of th[e] reported dangers have come on the charts in this way. . ."

The position of the Pelorus Reef referred to is in la[t.] 23° S., long. 176° 25′ W., about forty miles south [of] Pylstaart Island, which is volcanic. The reef was origin[-] ally reported in 1861 by H.M.S. *Pelorus*, Commodor[e] Seymour (now Lord Alcester), the ship passing within on[e-] third of a mile of it, when breakers were distinctly seen.

Lord Alcester assures me that there was no doubt of the breakers, otherwise it might be thought that the deceptive appearance that misled Captain Aldrich, also misled the officers of the *Pelorus*.

It thus appears probable that, as in some other cases (of which the Graham Island in the Mediterranean is perhaps best known), the cinders and ashes which formed, and still form, the summit of the volcanic mound originally thrown up, are being by wave-action gradually swept away, and will continue to be so removed until the top of the bank is reduced below the limit of such action, or, as in the case of the Graham Shoal, the solid rock is laid bare.

If so, it is another case of the preparation of a suitable foundation for coral builders by a process directly the reverse of that of building up by marine organisms on mounds that have failed to reach the surface, suggested by Mr. John Murray to be the principal method.

It remains for those who have made submarine eruptions their study to say whether a mound raised in the sea is covered with loose matter in a sufficient percentage of cases to justify this mode of coral-foundation-making being given an important place amongst others.

In the latest known cases of islands so formed, viz. Steers and Calmeyer Islands, thrown up near Krakatão in 1883, and Falcon Island, which appeared in 1885 in the Tonga Group, the surface structure was loose. The two former very shortly disappeared below the level of the sea. What is happening to the latter is not known, as it is seldom sighted; but from its volume and height (290 feet) the process of reduction, even if no compact nucleus exists above water, must be slow.

The deceptive appearance of the masses of minute organisms which floated in the vicinity of the bank is no doubt an abundant source of false reports. These clouds of matter are commoner in inclosed and calmer waters, like the Red Sea, than in open oceans, where they are so much more liable to be dispersed by the waves before they can accumulate to any size. The assistance they afforded in this instance to the searchers is remarkable, and so far as I know unique, as they are generally found in deep water. W. J. L. WHARTON.

RECENT VISIT OF NATURALISTS TO THE GALAPAGOS.

CAPTAIN J. M. DOW has placed at my disposal the subjoined short account of a visit recently paid to the Galapagos Group by the United States steamer *Albatross*, which will, I am sure, be of much interest to naturalists. P. L. SCLATER.

U.S. Commission of Fish and Fisheries,
Steamer "Albatross," Acapulco, Mexico,
April 24, 1888.

CAPTAIN J. M. DOW, *Panama*.

MY DEAR SIR,—Thinking that you might like to know something of the results of our trip to the Galapagos, I take this opportunity of writing.

Leaving Panama on the morning of March 30, we made during that day six hauls of the trawl in depths from 7 to 51 fathoms. These gave us fine results, including many species with which you are doubtless familiar. The fishes included species of *Upeneis*, *Arius*, *Polynemus*, *Aphronitia*, *Serranus*, *Selene*, *Prionotus*, *Hæmulon*, *Synodus*, *Tetrodon*, *Ophidium*, *Sciæna*, *Micropogon*, *Lophius*. We were delighted to see *Thalasophryne* and two allied species. The number of shells, Crustacea, &c., was almost innumerable. The care of so much material kept us very busy. The next day we sounded off Cape Mala, and found the depth to be 1927 fathoms. No more dredging was done until we neared the Galapagos on April 3, when we made a haul in 1379 fathoms, where the amount of material obtained was small, although it included some very good things. At the islands we made visits to eight of the principal ones, Most of our days were spent on shore, beginning early in the morning, and oftentimes bird-skinning and other work was prolonged far into the night. The islands presented a very inhospitable look along the shores, with the black lava cropping about everywhere; but in two of them (Chatham Island and Charles Island) the interior was extremely fertile and pleasant. Collecting was always difficult; but, with the co-operation of officers and men, we obtained a great quantity of material. We naturally looked to the birds first, on account of Darwin's previous work there. We have over 250 good bird-skins, besides several hundred specimens in alcohol, and a few skeletons. Of the fifty-seven species before reported from there, we obtained examples of fifty or more, and we have, in addition, several which are apparently new to science. We hope, with our material, to settle some of the curious problems of these islands.

We secured specimens of all the reptiles which have been before found there, and also hope that we have two or three new lizards. The tortoises excited great interest, and it would please you to see the many large ones which are now crawling about our decks. We expect now that we shall be able to raise them in the States.

Fishing was good at all of our anchorages, and we all had sport in catching fishes over the ship's side. We got between thirty and forty species in all, including a large brown "grouper," which is there caught and salted for the Ecuador market.

One night, while running from one island to another, we stopped and drifted for a while, and put the electric light over the side. Besides many small things, large sharks came around in great numbers. More than twenty were seen at once, and I know that the sight would have pleased you. We all regretted that you were not with us. Notwithstanding the necessity for rapid work, good-fellowship always prevailed as usual. I hope that some time you may take a trip with me on the *Albatross*, and see how we do it.

Hoping that this will not prove too long an account for you,

I remain,
Yours very sincerely,
LESLIE A. LEE.

THE BRITISH ASSOCIATION.

SECTION A—MATHEMATICAL AND PHYSICAL SCIENCE.

A Simple Hypothesis for Electro-magnetic Induction of Incomplete Circuits; with Consequent Equations of Electric Motion in Fixed Homogeneous or Heterogeneous Solid Matter, by Sir William Thomson.

(1) To avoid mathematical formulas till needed for calculation consider three cases of liquid[1] motion which for brevity I call Primary, Secondary, Tertiary, defined as follows:—Half the velocity in the Secondary agrees numerically and directionally with the magnitude and axis of the molecular spin at the corresponding point of the Primary; or (short, but complete, statement) *the half velocity in the Secondary is the spin in the Primary, and* (*similarly*) *half the velocity in the Tertiary is the spin in the Secondary*.

(2) In the Secondary and Tertiary the motion is essentially without change of density, and in each of them we naturally, therefore, take an incompressible fluid as the substance. The motion in the Primary we arbitrarily restrict by taking its fluid also as incompressible.

(3) Helmholtz first solved the problem—Given the spin in any case of liquid motion, to find the motion. His solution consists in finding the potentials of three ideal distributions of gravitational matter having densities respectively equal to $1/4\pi$ of the rectangular components of the given spin; and, regarding

[1] I use "liquid" for brevity to signify incompressible fluid.

for a moment these potentials as rectangular components of velocity in a case of liquid motion, taking the spin in this motion as the velocity in the required motion. Applying this solution to find the velocity in our Secondary from the velocity in our Tertiary, we see that the three velocity components in our Primary are the potentials of three ideal distributions of gravitational matter having their densities respectively equal to $1/4\pi$ of the three velocity components of our Tertiary. This proposition is proved in a moment,[1] in § 5 below, by expressing the velocity components of our Tertiary in terms of those of our Secondary, and those of our Secondary in terms of those of our Primary; and then eliminating the velocity components of Secondary, so as to have those of Tertiary directly in terms of those of Primary.

(4) Consider now, in a fixed solid or solids of no magnetic susceptibility, any case of electric motion in which there is no change of electrification, and therefore no incomplete electric circuit, or, which is the same, any case of electric motion in which the distribution of electric current agrees with the distribution of velocity in a case of liquid motion. Let this case, with velocity of liquid numerically equal to 4π times the electric current density, be our Tertiary. The velocity in our corresponding Secondary is then the magnetic force of the electric current system;[2] and the velocity in our Primary is what Maxwell[3] has well called the "electro-magnetic momentum at any point" of the electric current system; and the rate of decrease per unit of time, of any component of this last velocity at any point, is the corresponding component of electromotive force, due to electro-magnetic induction of the electric current system when it experiences any change. This electromotive force, combined with the electrostatic force, if there is any, constitutes the whole electromotive force at any point of the system. Hence by Ohm's law each component of electric current at any point is equal to the electric conductivity multiplied into the sum of the corresponding component of electrostatic force and the rate of decrease per unit of time of the corresponding component of velocity of liquid in our Primary.

(5) To express all this in symbols, let (u_1, v_1, w_1), (u_2, v_2, w_2), and (u_3, v_3, w_3) denote rectangular components of the velocity at time t, and point (x, y, z) of our Primary, Secondary, and Tertiary. We have (§ 1)—

$$u_2 = \frac{dw_1}{dy} - \frac{dv_1}{dz}, \quad v_2 = \frac{du_1}{dz} - \frac{dw_1}{dx}, \quad w_2 = \frac{dv_1}{dx} - \frac{du_1}{dy} \;.\; (1)$$

$$u_3 = \frac{dw_2}{dy} - \frac{dv_2}{dz}, \quad v_3 = \frac{du_2}{dz} - \frac{dw_2}{dx}, \quad w_3 = \frac{dv_2}{dx} - \frac{du_2}{dy} \;.\; (2)$$

Eliminating u_2, v_2, w_2 from (2) by (1), we find—

$$u_3 = \frac{d}{dx}\left(\frac{du_1}{dx} + \frac{dv_1}{dy} + \frac{dw_1}{dz}\right) - \left(\frac{d^2u_1}{dx^2} + \frac{d^2u_1}{dy^2} + \frac{d^2u_1}{dz^2}\right); \text{ \&c.} \quad \ldots \; (3)$$

But, by our assumption (§ 2) of incompressibility in the Primary—

$$\frac{du_1}{dx} + \frac{dv_1}{dy} + \frac{dw_1}{dz} = 0 \; . \; . \; . \; . \; . \; . \; . \; (4)$$

Hence (3) becomes—

$$u_3 = -\nabla^2 u_1, \quad v_3 = -\nabla^2 v_1, \quad w_3 = -\nabla^2 w_1 \; . \; . \; . \; (5)$$

where, as in Article xxvii. (November 1846) of my "Collected Papers" (vol. i.)—

$$\nabla^2 = \frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2} \; . \; . \; . \; . \; . \; . \; . \; (6)^4$$

This (5) is the promised proof of § 3.

(6) Let now u, v, w denote the components of electric current at (x, y, z) in the electric system of § 4; so that—

$$4\pi u = u_3 = -\nabla^2 u_1; \; 4\pi v = v_3 = -\nabla^2 v_1; \; 4\pi w = w_3 = -\nabla^2 w_1 \; . \; (7)$$

which, in virtue of (4), give—

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0 \; . \; . \; . \; . \; . \; . \; . \; (8)$$

Hence the components of electromotive force due to change of current, being, (§ 5)—

$$-\frac{du_3}{dt}, \; -\frac{dv_3}{dt}, \; -\frac{dw_3}{dt},$$

are equal to—

$$4\pi\nabla^{-2}\frac{du}{dt}, \; 4\pi\nabla^{-2}\frac{dv}{dt}, \; 4\pi\nabla^{-2}\frac{dw}{dt} \; . \; . \; . \; . \; . \; (9)$$

and therefore if Ψ denote electrostatic potential, we have, for the equations of the electric motion (§ 5)—

$$u = \frac{1}{\kappa}\left(\nabla^{-2}\frac{du}{dt} - \frac{d\Psi}{dx}\right); \; v = \frac{1}{\kappa}\left(\nabla^{-2}\frac{dv}{dt} - \frac{d\Psi}{dy}\right);$$

$$w = \frac{1}{\kappa}\left(\nabla^{-2}\frac{dw}{dt} - \frac{d\Psi}{dz}\right) \; . \; . \; . \; . \; . \; . \; (10)$$

where κ denotes $1/4\pi$ of the specific resistance.

(7) As Ψ is independent of t, according to § 4, we may, conveniently for a moment, put—

$$u + \frac{d\Psi}{\kappa dx} = \alpha; \; v + \frac{d\Psi}{\kappa dy} = \beta; \; w + \frac{d\Psi}{\kappa dz} = \gamma \; . \; . \; (11)$$

and so find, as equivalents to (9)—

$$\frac{d\alpha}{dt} = \nabla^2(\kappa\alpha); \; \frac{d\beta}{dt} = \nabla^2(\kappa\beta); \; \frac{d\gamma}{dt} = \nabla^2(\kappa\gamma) \; . \; . \; (12)$$

The interpretation of this elimination of Ψ may be illustrated by considering for example a finite portion of *homogeneous* solid conductor, of any shape (a long thin wire with two ends, or a short thick wire, or a solid globe, or a lump of any shape, of copper or other metal homogeneous throughout) with a constant flow of electricity maintained through it by electrodes from a voltaic battery or other source of electric energy, and with proper appliances over its whole boundary, so regulated as to keep any given constant potential at every point of the boundary; while currents are caused to circulate through the interior by varying currents in circuits exterior to it. There being no *changing electrification* by our supposition of § 4, Ψ can have no contribution from electrification within our conductor; and therefore, throughout our field—

$$\nabla^2\Psi = 0 \; . \; . \; . \; . \; . \; . \; . \; . \; . \; (13)$$

which, with (8) and (11), gives—

$$\frac{d\alpha}{dx} + \frac{d\beta}{dy} + \frac{d\gamma}{dz} = 0 \; . \; . \; . \; . \; . \; . \; (14)$$

Between (12) and (14) we have four equations for three unknown quantities. These, *in the case of homogeneousness* (κ constant), are equivalent to only three, because in this case (14) follows from (12) provided (14) is satisfied initially, and proper surface condition is maintained to prevent any violation of it from supervening. But unless κ is constant throughout our field, the four equations (12) and (14) are mutually inconsistent; from which it follows that our supposition of unchangingness of electrification (§ 4) is not generally true. An interesting and important practical conclusion is, that when currents are induced in any way, in a solid composed of parts having different electric conductivities (pieces of copper and lead, for example, fixed together in metallic contact), there must in general be changing electrification over every interface between these parts. This conclusion was not at first obvious to me; but it ought to be so by anyone approaching the subject with mind undisturbed by mathematical formulas.

(8) Being thus warned off heterogeneousness until we come to consider changing electrification and incomplete circuits, let us apply (10) to an infinite homogeneous solid. As (8) holds through all space according to our supposition in § 4, and as κ is constant, (13) must now hold through all space, and therefore $\Psi = 0$, which reduces (10) to—

$$u = \frac{1}{\kappa}\nabla^{-2}\frac{du}{dt}; \; v = \frac{1}{\kappa}\nabla^{-2}\frac{dv}{dt}; \; w = \frac{1}{\kappa}\nabla^{-2}\frac{dw}{dt} \; . \; . \; (15)$$

These equations express simply the known law of *electro magnetic* induction. Maxwell's equations (7) of § 783 of his "Electricity and Magnetism," become, in this case—

$$\mu\left(4\pi C + K\frac{d}{dt}\right)\frac{du}{dt} = \nabla^2 u, \text{ \&c.} \; . \; . \; . \; . \; . \; (15')$$

which cannot be right, I think (???), according to any conceivable hypothesis regarding electric conductivity, whether of metals, or

[1] From Poisson's well-known elementary theorem, $\nabla^2 V = -4\pi\rho$.
[2] "Electrostatics and Magnetism," § 517 (Postscript) (*c*).
[3] "Electricity and Magnetism," §§ 585, 604.
[4] Maxwell, for quaternionic reasons, takes ∇^2 the negative of mine.

stones, or gums, or resins, or wax, or shell-lac, or gutta-percha, or india-rubber, or glasses, or solid or liquid electrolytes; being, as seems (?) to me, vitiated for complete circuits, by the curious and ingenious, but, as seems to me, not wholly tenable, hypothesis which he introduces, in § 610, for incomplete circuits.

(9) The hypothesis which I suggest for incomplete circuits and consequently varying electrification, is simply that the components of the electromotive due to electro-magnetic induction are still $4\pi\nabla^{-2}du/dt$, &c. Thus for the equations of motion we have simply to keep equations (10) unchanged, while not imposing (8), but instead of it taking—

$$``v"^2\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right)=\frac{d}{dt}\nabla^2\Psi \quad . \; . \; . \; . \; . \; (16)$$

where "v" denotes the number of electrostatic units in the electro-magnetic unit of electric quantity. This equation expresses that the electrification of which Ψ is the potential increases and diminishes in any place according as electricity flows more out than in, or more in than out. We thus have four equations (10) and (16) for our four unknowns, u, v, w, Ψ; and I find simple and natural solutions with nothing vague, or difficult to understand, or to believe when understood, by their application to practical problems, or to conceivable ideal problems; such as the transmission of ordinary or telephonic signals along submarine telegraph conductors and land-lines, electric oscillations in a finite insulated conductor of any form, transference of electricity through an infinite solid, &c. This, however, does not prove my hypothesis. Experiment is required for informing us as to the real electro-magnetic effects of incomplete circuits, and as Helmholtz has remarked, it is not easy to imagine any kind of experiment which could decide between different hypotheses which may occur to anyone trying to evolve out of his inner consciousness a theory of the mutual force and induction between incomplete circuits.

On the Transference of Electricity within a Homogeneous Solid Conductor, by Sir William Thomson.—Adopting the notation and formulas of my previous paper, and taking ρ to denote 4π times the electric density at time t, and place (x, y, z), we have—

$$\rho=\nabla^2\Psi=-``v"^2\int\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right)dt \quad . \; . \; (17)$$

and, eliminating u, v, w, Ψ by this and (16) from (10), we find, on the assumption of κ constant—

$$\kappa\frac{d}{dt}\nabla^2\rho=\frac{d^2\rho}{dt^2}-``v"^2\nabla^2\rho \quad . \; . \; . \; . \; . \; (18)$$

The settlement of boundary conditions, when a finite piece of solid conductor is the subject, involves consideration of u, v, w, and for it, therefore, equations (17) and (12) must be taken into account; but when the subject is an infinite homogeneous solid, which, for simplicity, we now suppose it to be, (18) suffices. It is interesting and helpful to remark that this agrees with the equation for the density of a viscous elastic fluid, found from Stokes's equations for sound in air with viscosity taken into account; and that the values of u, v, w, given by (17) and (10), when ρ has been determined, agree with the velocity components of the elastic fluid if the simple and natural enough supposition be made that viscous resistance acts only against change of shape, and not against change of volume without change of shape.

For a type-solution assume—

$$\rho=\mathrm{A}\epsilon^{-qt}\cos\frac{2\pi x}{a}\cos\frac{2\pi y}{b}\cos\frac{2\pi z}{c} \quad . \; . \; . \; . \; (19)$$

and we find, by substitution in (18)—

$$q^2-\frac{\kappa}{\mathrm{L}^2}q+\frac{``v"^2}{\mathrm{L}^2}=0 \quad . \; . \; . \; . \; . \; . \; (20)$$

where—

$$\mathrm{L}^2=1/4\pi^2\left(\frac{1}{a^2}+\frac{1}{b^2}+\frac{1}{c^2}\right) \quad . \; . \; . \; . \; . \; (21)$$

Hence, by solution of the quadratic (20) for q—

$$q=\tfrac{1}{2}\frac{\kappa}{\mathrm{L}^2}\left\{1\pm\sqrt{\left(1-\frac{4``v"^2\mathrm{L}^2}{\kappa^2}\right)}\right\} \quad . \; . \; (22)$$

[In the communication to the Section numerical illustrations of non-oscillatory and of oscillatory discharge are given.]

Five Applications of Fourier's Law of Diffusion, illustrated by a Diagram of Curves with Absolute Numerical Values, by Sir William Thomson.—(1) Motion of a viscous fluid; (2) closed electric currents within a homogeneous conductor;[1] (3) heat; (4) substances in solution; (5) electric potential in the conductor of a submarine cable when electro-magnetic inertia can be neglected.[2]

1. Fourier's now well-known analysis of what he calls the "linear motion of heat" is applicable to every case of diffusion in which the substance concerned is in the same condition at all points of any one plane parallel to a given plane. The differential equation of diffusion,[3] for the case of constant diffusivity, κ, is—

$$\frac{dv}{dt}=\kappa\frac{d^2v}{dx^2},$$

where v denotes the "*quality*" at time t and at distance x from a fixed plane of reference. This equation, stated in words, is as follows:—Rate of augmentation of the "*quality*" per unit of time is equal to the diffusivity multiplied into the rate of augmentation per unit of space of the "*quality*."

The meaning of the word "*quality*" here depends on the subject of the diffusion, which may be any one of the five cases referred to in the title above.

2. If the subject is motion of a viscous fluid, the "*quality*" is any one of three components of the velocity, relative to rectangular rectilineal co-ordinates. But in order that Fourier's diffusional law may be applicable, we must either have the motion very slow, according to a special definition of slowness; or the motion must be such that the velocity is the same for all points in the same stream-line, and would continue to be steadily so if viscosity were annulled at any instant. This condition is satisfied in laminar flow, and more generally in every case in which the stream-lines are parallel straight lines. It is also satisfied in the still more general case of stream-lines coaxal circles with velocity the same at all points at the same distance from the axis. Our present illustration, however, is confined to the case of laminar flow, to which Fourier's diffusional laws for what he calls "linear motion" (as explained above in § 1) is obviously applicable without any limitation to the greatness of the velocity in any part of the fluid considered (though with conceivably a reservation in respect to the question of stability[4]). In this case the "*quality*" is simply fluid velocity.

3. If the subject is electric current in a non-magnetic metal, with stream-lines parallel straight lines, the "*quality*" is simply current-density, that is to say, strength of current per unit of area perpendicular to the current. The perfect mathematical[5] analogy between the electric motion thus defined, and the corresponding motion of a viscous fluid defined in § 2 was accentuated by Mr. Oliver Heaviside in the *Electrician*, July 12, 1884; and in the following words in the *Philosophical Magazine* for 1886, second half-year, p. 135:—"Water in a round pipe is started from rest and set into a state of steady motion by the sudden and continued application of a steady longitudinal dragging or shearing force applied to *its boundary*. This analogue is useful because everyone is familiar with the setting of water in motion by friction on its boundary, transmitted inward by viscosity." Mr. Heaviside well calls this analogue "useful." It is, indeed, a very valuable analogy, not merely in respect to philosophical consideration of electricity, ether, and ponderable matter, but as facilitating many important estimates, particu-

[1] This subject is essentially the "electro-magnetic induction" of Henry and Faraday. It is essentially different from the "diffusion of electricity" through a solid investigated by Ohm in his celebrated paper "Die Galvanische Kette mathematisch bearbeitet," Berlin, 1827; tran lated in Taylor's "Scientific Memoirs," vol. ii. Part 8, "The Galvanic Circuit investigated Mathematically," by Dr. G. S. Ohm. In Ohm's work electro-magnetic induction is not taken into account. nor does any idea of an electric analogue to inertia appear. The electromotive force considered is simply that due to the difference of electrostatic potential in different parts of the circuit, unsatisfactorily, and even not accurately, explained by what, speaking in his pre-Greenian time, he called "the electroscopic force of the body," and defined or explained as "the force with which the electroscope is repelled or attracted by the body;" the electroscope being "a second movable body of invariable electric condition."

[2] This subject belongs to the Ohmian electric diffusion pure and simple, worked out by aid of Green's theory of the capacity of a Leyden jar (see "Mathematical and Physical Paper," vol. ii. Art. 73).

[3] See "Mathematical and Physical Papers," vol. ii. Art. 72.

[4] See "Stability of Fluid Motion," § 28, *Philosophical Magazine*, August 1887.

[5] It is essentially a mathematical analogy only; in the same sense as the relation between the "uniform motion of heat" and the mathematical theory of electricity, which I gave in the *Cambridge Mathematical Journal* forty-six years ago, and which now constitutes the first article of my "Electrostatics and Magnetism," is a merely mathematical analogy.

larly some relating to telephonic conductors and conductors for electric lighting on the "alternate-current" system. In a short article to be included in vol. iii. of my collected papers, which I hope will soon be published, I intend to describe a generalization, with, as will be seen, a consequently essential modification of this analogy, by which it is extended to include the mutual induction between conductors separated by air or other insulators, and currents in solids of different conductivity fixed together in contact.

4. If the subject is heat, as in Fourier's original development of the theory of diffusion, the "*quality*" is temperature.

5. If the subject is diffusion of matter, the "*quality*" is

$$ON = x$$

$$NP = y$$

$$y = \frac{2}{\sqrt{\pi}} \int_0^{2x/i} \epsilon^{-q^2} dq.$$

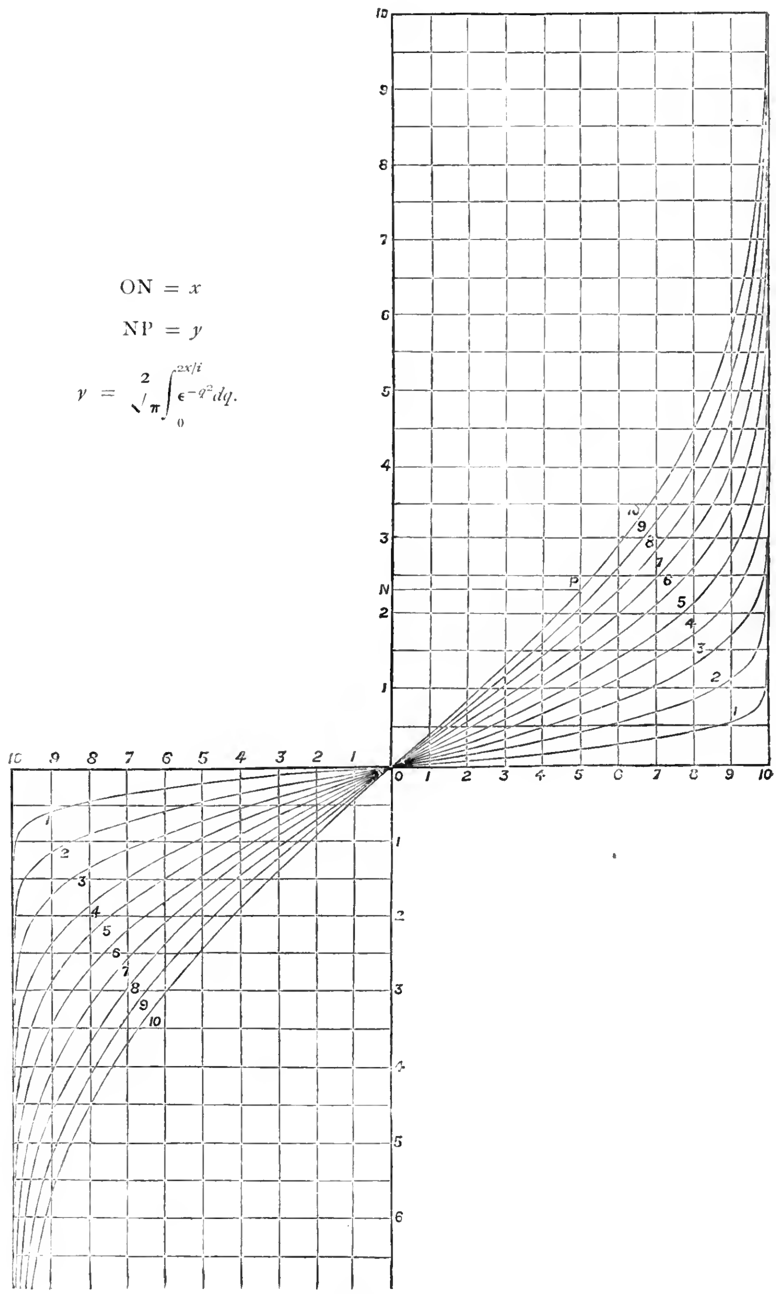


Diagram showing Progress of Laminar Diffusion.

density of the matter diffused, or deviation of density from some mean or standard density considered. It is to Fick, thirty-three years ago Demonstrator of Anatomy, and now Professor of Physiology in the University of Zürich, that we owe this application of Fourier's diffusional theory, so vitally important in physiological chemistry and physics, and so valuable in natural philosophy generally. When the substance through which the diffusion takes place is fluid, a very complicated but practically important subject is presented if the fluid be stirred. The exceedingly rapid progress of the diffusion produced by vigorous up-and-down-stirring, causing to be done in half a minute the diffusional work which would require years or centuries if the fluid were quiescent, is easily explained; and the explanation is illustrated by the diagram of curves, § 7 below, with the time-values given for sugar and common salt. Look at curve No. 1, and think of the corresponding curve with vertical ordinates diminished in the ratio of 1 to 40. The corresponding diffusion would take place for sugar in 11 seconds, and for salt in $3\frac{1}{2}$ seconds. The case so represented would quite correspond to a streaky distribution of brine and water or of syrup and water, in which portions of greatest and least salinity or saccharinity are within half a millimetre of one another. This is just the condition which we see, in virtue of the difference of optic refractivity produced by difference of salinity or of saccharinity, when we stir a tumbler of water with a quantity of undissolved sugar or salt on its bottom. If water be poured very gently on a quantity of sugar or salt in the bottom of a tumbler with violent stirring up guarded against by a spoon—the now almost extinct Scottish species called "toddy ladle" being the best form, or, better still, a little wooden disk which will float up with the water; and if the tumbler be left to itself undisturbed for two or three weeks, the condition at the end of 17×10^5 seconds (twenty days) for the case of sugar, or $5{\cdot}4 \times 10^5$ seconds (six days) for salt, will be that represented by No. 10 curve in the diagram.

6. If the subject be electricity in a submarine cable, the "*quality*" is electric potential at any point of the insulated conductor. It is only if the cable were a straight line that x would be (as defined above) distance from a fixed plane: but the cable need not be laid along a straight line; and the proper definition of x for the application of Fourier's formula to a submarine cable is the distance along the cable from any point of reference (one end of the cable, for example) to any point of the cable. For this case the diffusivity is equal to the conductivity of its conductor, reckoned in electrostatic units, divided by the electrostatic capacity of the conductor per unit length insulated as it is in gutta-percha, with its outer surface wet with sea-water, which, in the circumstances, is to be regarded as a perfect conductor. For demonstration of this proposition see vol. ii. Art. lxxiii. (1855) of my collected papers.

7. *Explanation of Diagram showing Progress of Laminar Diffusion.*—In each curve—

$$\frac{1}{10}NP = \frac{2}{\sqrt{\pi}}\int_0^{2x/i} dq.\,\epsilon^{-q^2},$$

where x denotes the number of centimetres in ON, and i the "curve-number." The curves are drawn directly from the values of the integral given in Table III., appended to De Morgan's article "On the Theory of Probabilities," "Encyclopædia Metropolitana," vol. ii. pp. 483–84.

NP denotes the "*quality*" (defined below) { at distance = ON from initia surface or interface, and at time equal in seconds to ["curve-number"]^2 divided by sixteen times the diffusivity in square centimetres per second.

Subject of Diffusion.	"*Quality*" (represented by $\frac{1}{10}NP$).
Motion of a viscous fluid ...	Ratio of the velocity at N to the constant velocity at O
Closed electric currents within a homogeneous conductor	Current-density
Heat	Ratio of temperature *minus* mean temperature to mean temperature
Substance in solution	Ratio of density *minus* mean density to mean density
Electric potential in the conductor of a submarine cable	Ratio of potential at N to constant potential at end O

EXAMPLES.

"Curve-number."	Time in Seconds.	Case of Diffusion.
1	27056	Zinc sulphate through water
1	25720	Copper sulphate through water
1	17000	Sugar through water
1	5400	Common salt through water
5	1180	Heat through wood
5	118	Laminar motion of water at 10° C.
5	30	Laminar motion of air
5	7·1	Heat through iron
5	1·31	Heat through copper
		Electric current in a homogeneous non-magnetic conductor:
10	0·0488	Copper
10	0·0040	Lead
10	0·0038	German silver
10	0·0023	Platinoid
1,000,000,000	2·15	Electric potential in the Direct U.S. Atlantic Cable

Prof. G. H. Darwin sent a paper *On the Mechanical Conditions of a Swarm of Meteorites and on Theories of Cosmogony.*—This is an abstract of a communication made to the Royal Society, in which the author proposes to apply the principles of the kinetic theory of gases to the case of a swarm of meteorites in space. In the author's theory the individual meteorites are considered to be analogous to the molecules of the gas; and thus a swarm of meteorites, in the course of conglomeration into a star, possesses mechanical properties analogous to those of a gas. Lockyer and others have expressed their conviction that the present condition of the solar system is derived from an accretion of meteorites, but the idea of fluid pressure seems necessary for the applicability of any theory like the nebular hypothesis. The author then proposes to reconcile the nebular and meteoric theories by showing that the laws of fluid pressure apply to a swarm of meteorites. The case of a globular swarm of equal-sized meteorites is considered, and then the investigation is extended to the case in which the meteorites are of various sizes; the latter extension does not affect the nature of the proof, and only slightly modifies the result. In the case of a swarm of meteorites condensing under the mutual attraction of its parts, the author shows that the larger meteorites will tend to settle towards the centre of condensation, and that consequently the mean size of the meteorites will decrease from the centre towards the outside of the swarm.

NOTES.

WE mentioned some time ago that the executors of the late Sir William Siemens, desiring to have his biography authoritatively published, had placed its preparation in the hands of Dr. William Pole, F.R.S., Honorary Secretary of the Institution of Civil Engineers, who had long been a personal friend of Sir William and his family. The work is now finished, and will be published immediately, in one volume, by Mr. Murray. It will be followed by other volumes, containing reprints of Sir William's most important scientific papers, lectures, and addresses, edited by his secretary, Mr. E. F. Bamber.

ALL who take an interest in questions relating to technical education have reason to be grateful to the Goldsmiths' Company for the way in which it has associated itself with the movement for the establishment of technical and recreative institutes in South London. By an act of splendid generosity it has secured that there shall soon be a great centre of technical instruction at New Cross. Subject to the sanction of Parliament, which will of course be readily granted, the following proposal has been accepted. Out of the surplus funds of the

City parochial charities, the Charity Commissioners are to acquire the buildings, with seven acres of land, at present occupied by the Royal Naval School at New Cross; and from the same source they will set apart an endowment of £2500 per annum. This will be met by the Goldsmiths' Company by the appropriation out of their corporate funds (not trust funds, but funds over which they have absolute control) of an annual endowment of a similar amount—a gift equal to a sum of £85,000. It is intended that the new Institute shall be called "The Goldsmiths' Company's (New Cross) Institute."

It is satisfactory to learn that all the scientific work connected with the Fishery Board for Scotland is now absolutely in the hands of a small Committee, of which Prof. Ewart is convener, and that the Board has at last a scientific secretary. A Special Committee on Bait, appointed by the Secretary for Scotland, began its sittings on Monday.

The first meeting of the Council of the Sanitary Institute, which has recently been incorporated, was held at the Parkes Museum last Friday. Sir Douglas Galton, K.C.B., F.R.S., was unanimously appointed Chairman of the Council, and Mr. G. J. Symons, F.R.S., the registrar. The Institute is founded to carry on the work of the amalgamated Sanitary Institute of Great Britain and the Parkes Museum, and it was decided to hold the Institute's first examination for local surveyors and inspectors of nuisances on November 8 and 9. A programme of lectures for the winter session is being prepared. A letter was read from the Charity Commissioners saying that they considered that the new Institute was likely to prove a powerful means for the diffusion of sanitary knowledge, and promising to place at its disposal, for the delivery of lectures, the buildings which the Commissioners propose to establish in various parts of London.

The delegates to the International Bureau of Weights and Measures are hard at work at the Pavillon de Breteuil, near St. Cloud. They are taking steps to verify the "prototype metres" which have been executed at the expense of the French Government, and are to be delivered to the various nations which have ordered them. The expenditure of this establishment, which is supported by contributions from several nations, amounts to £4000. The head of the administration is M. Broch, a Norwegian astronomer and meteorologist. Turkey is nominally one of the subscribing nations, but she has never contributed a farthing to the funds of the Bureau, and some time ago the other nations were obliged to subscribe a supplementary sum to make good the deficiency.

The School of Art Wood-carving, City and Guilds Institute, Exhibition Road, South Kensington, has been re-opened after the usual summer vacation, and we are requested to state that one or two of the free Studentships in the evening classes maintained by means of funds granted to the school by the Institute are vacant. To bring the benefits of the school within the reach of artisans, a remission of half-fees for the evening class is made to artisan students connected with the wood-carving trade. Forms of application for the free Studentships and any further particulars relating to the school may be obtained from the manager.

Ten lectures on "Electricity in the Service of Man" are to be delivered by Mr. W. Lant Carpenter, under the auspices of the London Society for the Extension of University Teaching, at the Chelsea Town Hall. They will be delivered on Fridays at 8 p.m. The inaugural lecture, on electrical energy and its uses, will be given on October 12, when Sir Henry Roscoe will take the chair.

The sixth session of University College, Dundee, was opened by a public address by Prof. Ewing in the College Hall last Saturday evening. Prof. Ewing gave an interesting account of the progress which has lately been made in the teaching of science in Dundee.

Herr Hernsheim, the German Consul at Matupi, one of the South Sea Islands, has presented his native town, Mayence, with an ethnological collection which gives an interesting picture of the manners, customs, and conditions of life of the inhabitants of the Bismarck Archipelago, and the Caroline, Marshall, Pelew, and Solomon Groups.

Towards the cost of the University just opened in Tomsk, Count Demidoff contributed £9000, M. Cybulsky £7500, and the State the balance, £22,000. M. Sibiriakoff has made a donation of £8500 for scientific Scholarships.

The Hon. A. C. Houen, a Norwegian resident at Rome, has presented the Christiania University with £6500 for the purpose of founding scientific Scholarships. He recently gave the same institution £10,000 for a like object.

At a recent meeting of the Geographical Society of Stockholm, Dr. F. Svenonius read a paper on the origin and present state of the glaciers of Europe, dividing them into Alpine, Greenland, and Scandinavian. Referring to the latter, Dr. Svenonius stated that the glaciers of Sweden, to which he had devoted years of study, were far more important than was generally imagined. They could be divided into some twenty different groups, all being situated between 67° and 68½° lat. N., *i.e.* between the sources of the Pile River and Lake Torne. They number upwards of one hundred, and cover a total area of at least 400 square kilometres. The largest is the Sorjik group, the area of which is between 65 and 75 square kilometres.

The great "Bibliography of Meteorology," at which Mr. C. J. Sawyer, of the United States Signal Service, has been working for some years, is now completed. It comes down to the year 1881, inclusive; and Mr. Sawyer estimates that it contains 50,000 independent titles. General Greely, the Chief Signal Officer, is anxious that the work should be printed; and in his last Annual Report he pointed out that, if this were done, future international co-operation would probably secure, by a system of rotation, from the various European Governments, the publication of a series of supplements which would keep the world abreast of the steadily-increasing volume of meteorological publications.

The Administration Report of the Meteorological Reporter to the Government of Bengal for the year 1887–88 states that it has been decided to submit, for two years only, brief accounts of the principal points, while every third year a detailed Report is to be prepared. The present Report is the first of the triennial series. The most important changes during the year have been in the storm-signal service. Until recently, regular storm-signals were not allowed by the port authorities to be displayed in Calcutta, so that ships on several occasions left their safe anchorage in the port, and were proceeding down the river, before they became aware of the display of storm-signals. This condition has, however, been completely changed during the year 1887–88, and signals are now shown, by orders of the Bengal Reporter, in Calcutta, and have been extended to all the ports from the south of Burmah down to the extreme south of the Madras Presidency, or, roughly speaking, he has to warn a coast-line of about 2400 miles in length. His work and responsibility have therefore been very decidedly increased. The observations for the weather service are now taken at 8 a.m. instead of 10 a.m. The advantage of this change, for the issue of storm-warnings in useful time, is obvious.

The Pilot Chart of the North Atlantic Ocean for September shows that the weather during August was generally fine over that ocean. Gales of varying force, however, occurred about once a week over the steam-ship routes. On the 13th and 14th a depression moved along the coast of New England, and reached Newfoundland on the 15th; from this position it moved to the eastward, and appears to have reached this country. No other storm crossed the ocean entirely. Less fog was encountered than is usual during August, and with the exception of a few bergs in the Straits of Belleisle no ice was reported during the month.

M. G. ROLLIN, of the French Meteorological Office, has published in the *Annales* of that institution a valuable article entitled "Remarks on Synoptic Charts." He has carefully examined day by day the movements of the atmosphere, with the view of determining the possibility of predicting the arrival of storms coming from the Atlantic. His experience of the American telegrams coincides with that arrived at in this country, that they cannot at present be turned to practical use in weather prediction. But he has made a serious attempt to render them useful in the future, by the establishment of certain types which connect the weather of the Atlantic with that of the adjacent continents, and he finds that many conditions, without being actually identical, are sufficiently alike to be classified together. His concluding remarks, however, show that much further investigation is necessary before any definite rules can be laid down, and that the atmospheric changes are often so rapid that the difficulties of weather prediction on the exposed coasts of Europe are likely to remain very great for a long time to come.

A BEAUTIFUL crystalline substance of much theoretic interest was exhibited at the recent Bath meeting by its discoverer, Prof. Emerson Reynolds, F.R.S., of Dublin University. Its mode of formation and analysis prove that it is $Si(NHC_6H_5)_4$, or silicotetraphenylamide. It is the first well-defined compound in which silicon is exclusively united with the nitrogen of amidic groups, and is formed by the action of excess of phenylamine on silicon tetrabromide. The new compound crystallizes from carbon disulphide in fine transparent, colourless prisms, which melt sharply at 132°. When heated *in vacuo*, aniline distils over, and a residue is obtained which appears to be the silicon analogue of carbodiphenylimide. Considering the important part which silicon plays in Nature, and its close resemblance to carbon—which affords a large number of important nitrogen compounds—it is surprising that little is yet known of the relations of silicon and nitrogen. The investigation of the new substance is likely to throw much light on this general question.

AT the same meeting Prof. Emerson Reynolds also exhibited a number of new silicon compounds of a different type from that above noticed. They were obtained by the action of silicon tetrabromide on the primary thiocarbamide and some of its derivatives. The products are addition compounds: that obtained with the primary thiocarbamide has the formula $(H_4N_2CS)_8SiBr_4$, and analogous compounds were formed with allyl, phenyl, and diphenyl-thiocarbamides. The allyl product is a colourless and very viscous liquid, the others are vitreous solids at ordinary temperature. When the primary thiocarbamide compound is dissolved by ethylic alcohol, it is decomposed, and affords tetra- and tri-thiocarbamide derivatives free from silicon. The first of these products is a fine crystalline substance, whose formula is $(H_5N_2CS)_4NBr$; the second is a sulphinic compound, $(H_5N_2CS)_3Br \cdot C_2H_5Br$. Prof. Reynolds succeeded in effecting the synthesis of the first compound by the direct union of thiocarbamide with ammonium bromide, and subsequently produced a series of similar bodies by substituting for ammonium bromide the bromides, iodides, and chlorides of ammonium bases. Although the derivatives of thiocarbamide are very numerous, only those were known which result from one or two molecules of the amide; but the existence of the new compounds exhibited by Prof. E. Reynolds proves that thiocarbamide can afford much more highly-condensed products.

AN important quantitative reaction between iodine and arseniuretted hydrogen has recently been investigated by Dr. Otto Brunn. During a series of attempts to completely eliminate arseniuretted hydrogen from sulphuretted hydrogen prepared from materials containing arsenic, it was found that this could be completely effected by passing the mixture over a layer of iodine. The mixed gases were first dried by passage through a calcium chloride tube, and were then led through a tube 12 mm. wide, containing the layer of powdered iodine; a plug of glass wool moistened with potassium iodide to remove vapour of iodine was placed at the end of the layer, and attached to the extremity of the tube were a couple of flasks containing lead acetate solution to absorb the sulphuretted hydrogen. On removing the iodine tube and heating the issuing gas in the usual drawn out form of hard glass tube, a fine mirror of metallic arsenic was deposited, but after insertion of the iodine tube not a trace of deposit was obtained, while a yellow coating of iodide of arsenic was formed upon the surface of the iodine. This led Dr. Brunn to experimentally determine whether the reaction was quantitative or not. Equal volumes of a mixture of hydrogen and arseniuretted hydrogen were passed in two successive experiments through a solution of silver nitrate in the one case, and over a layer of iodine 25 cm. long in the other. As is well known, silver nitrate is quantitatively reduced by the hydride of arsenic to metallic silver, the arsenic being oxidized to arsenious acid. It was found that the amount of arsenic absorbed by the iodine was exactly equal to that absorbed by the silver nitrate, and hence the iodine reaction is happily found to be also a quantitative one. Chemists have therefore a ready means of freeing both hydrogen and sulphuretted hydrogen from the last traces of this most objectionable hydride of arsenic. It was finally shown that hydride of antimony behaves in a precisely similar manner with iodine.

THE Trustees of the Australian Museum have issued their Report for 1887. The total number of visitors was 122,799, as against 127,231 in 1886. This Museum is open on Sundays from 2 o'clock to 5, and the privilege seems to be much appreciated. The average daily attendance throughout the year was 330 on week-days and 709 on Sundays. The collections of the Museum are being steadily increased, mainly by purchases, exchanges, and donations, but also by collecting and dredging expeditions sent out by the authorities of the institution. An expedition, under the charge of Messrs. Cairn and Grant, to the Bellenden Ker Ranges, in Northern Queensland, resulted in obtaining for the Museum about sixty-eight species (198 specimens) of birds, and eleven species (thirty-five specimens) of mammals, seven of which are new to the Museum, and three are new to science; besides a number of insects and other Invertebrates. The Trustees were enabled also during the year to send an Expedition to Lord Howe Island, in company with the Visiting Magistrate, Mr. H. T. Wilkinson. The Ethnological Hall referred to in last year's Report has been fitted up with cases, and the valuable ethnological collections, mostly acquired during recent years, are arranged there. The Trustees anticipate that this will prove to be "not the least interesting portion of the Museum."

AN interesting "Hand-book of Sydney" has been published for the use of the members of the Australasian Association for the Advancement of Science. The editor is Mr. W. M. Hamlet, Government Analyst, Sydney. His object is to give an epitome of the history, meteorology, geology, flora, and fauna of Sydney and the surrounding neighbourhood, together with a brief account of the commerce and industries which have grown up in the mother country of Australia during the first half-century.

The Royal Society of Canada has issued its Proceedings and Transactions during the year 1887. This is the fifth volume of the series. Among the papers (some of which are in French) we may note the following: the Eskimo, by Franz Boas; notes and observations on the Kwakiool people of the northern part of Vancouver Island, and adjacent coasts, made during the summer of 1885, with a vocabulary of about seven hundred words, by George M. Dawson; on the Indians and Eskimos of the Ungava District, Labrador, by Lucien M. Turner; on a specimen of Canadian native platinum from British Columbia, by G. Christian Hoffmann; microscopic petrography of the drift of Central Ontario, by A. P. Coleman; Michel Sarrazin: matériaux pour servir à l'histoire de la science en Canada, by the Abbé Laflamme; a review of Canadian botany from the first settlement of New France to the nineteenth century, by D. P. Penhallow; illustrations of the fauna of the St. John group, by G. F. Matthew; squirrels, their habits and intelligence, with especial reference to feigning, by T. Wesley Mills.

The first volume of the "Geological Record," for 1880–84 (inclusive), has just been published. The second volume is partly in type, and will be ready by the end of the year. The editors are Mr. W. Topley and Mr. C. Davies Sherborn. Three alterations have been made in this issue of the "Record." Titles only are given; physical geology is all included under one heading, instead of three as heretofore; supplements are abolished, titles omitted from previous years appearing in the main text.

According to the Report of the Committee of Council on Education (England and Wales) for the past year, the class subjects under the head of "Elementary Science" have practically not been taught in the elementary schools throughout the country. Only thirty-nine schools have taken up any of these subjects, while geography, for instance, has been taught in 12,035 schools. With regard to the training colleges for teachers it has of late years been arranged that success in the examinations in science held by the Science and Art Department should be reckoned in fixing the students' places in the class list of candidates for certificates as teachers of public schools. It is curious that in the training colleges in Wales—Bangor, Carmarthen, and Carnarvon—not a single student presented himself in mathematics, theoretical mechanics, animal physiology, or inorganic chemistry; and out of 713 male students who passed the examinations in science under the Science and Art Department before entering training colleges in the country only seven passed in applied mechanics, nine in organic chemistry, and six in botany. Amongst the female students who passed the Science and Art Department, animal physiology and physiography were the favourite subjects, while not one passed in applied mechanics, only one in theoretical mechanics, and three in organic chemistry.

We have received a copy of "Rural School Education in Agriculture (Scotland)," the opening lecture delivered to an agricultural class of rural teachers in the University of Edinburgh by Prof. Robert Wallace. At the outset he gives a short history of agricultural education in the University of Edinburgh (the Chair was founded in 1790), and comments on the fact that the students attending his classes are rural schoolmasters from every county in Scotland. Last year a Government grant of £300 to the University enabled the Senate to arrange special classes for his hearers. The students, he says, are not intended to be farmers. They are to be, so to speak, literary experts on agricultural matters, who are to direct the minds of lads in rural districts into proper channels, and to stir up amongst them an intelligent curiosity as to the animal and plant life around them. A suggestion made by Prof. Wallace as to the formation of libraries for the help of the rural teachers is worthy of attention. Each of these libraries should have a cyclopædia of agriculture, and one guinea a year should be expended on each to provide some leading agricultural periodical. This is all that would be absolutely necessary. He also advocates the changing of the text-books at present in use in agricultural classes in Scotland.

The additions to the Zoological Society's Gardens during the past week include a Patas Monkey (*Cercopithecus patas* ♀) from West Africa, presented by Master Lewis Levy; a Drill Baboon (*Cynocephalus leucophæus* ♀) from West Africa, presented by the Rev. G. H. Richardson; a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Miss Jessie Bone; a Common Marmoset (*Hapale jacchus*) from Brazil, presented by Miss Maud Bryden; a Ring-tailed Coati (*Nasua rufa* ♂) from Demerara, presented by Mr. Robert Sentonally; two Grey Ichneumons (*Herpestes griseus* ♂ ♀) from India, presented respectively by Mr. A. Cresser and Miss Alice Rutherford; two West African Love Birds (*Agapornis pullaria*) from West Africa, presented by Miss Ethel Levy; a Salt-water Terrapin (*Clemmys terrapin*) from North America, presented by Mr. Nicholas Fenwick Hele; four Blue-bearded Jays (*Cyanocorax cyanopogon*) from Para, a Violaceous Night Heron (*Nycticorax violaceus*) from South America, purchased; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, deposited.

OUR ASTRONOMICAL COLUMN.

The Light-Curve of U Ophiuchi.—Mr. S. C. Chandler investigated the light-curve of this most interesting variable about a year ago (Nature, vol. xxxvii. p. 36), and found evidence of a slight shortening of the period. Mr. Chandler's light-curve also showed an irregularity in the increase of light after minimum, similar to that which Schönfeld had already exhibited in the light-curves of Algol and S Cancri—a diminution, that is, in the speed of recovery almost amounting to a short halt. It is evident that it is of great importance to decide whether this irregularity is due merely to some personality of the observer, or is truly characteristic of the star's variation, for in the latter case it would be difficult to reconcile it with the view now generally held that the variability of stars of the Algol type is due to the transit of a dark satellite. Mr. Sawyer has recently published (*Gould's Astronomical Journal*, No. 177) the light-curve from his own observations, which are 527 in number, made on 57 nights, and involve 1135 comparisons. Mr. Sawyer's curve shows an irregularity similar to but slighter than that of Mr. Chandler's, but the retardation takes place sooner after the minimum, and the mean of the two curves gives an almost perfectly symmetrical curve for both decrease and recovery. It would seem likely, therefore, that for this star at least this curious irregularity is a purely subjective one, and the regularity of the mean curve would seem to afford confirmation to the satellite theory.

Comets Brooks and Faye.—The following ephemerides are in continuation of those given in Nature, vol. xxxviii. p. 503, and p. 528:—

1888.	Comet 1888 *c* (Brooks). R.A. h. m. s.	Decl. ° ′	Comet 1888 *d* (Faye). R.A. h. m. s.	Decl. ° ′
Oct. 15 ...	16 14 43 ...	5 57·4 N.	7 33 20 ...	11 11 N.
17 ...	16 19 50 ...	4 57·6	7 36 29 ...	10 47
19 ...	16 24 49 ...	4 0·1	7 39 32 ...	10 23
21 ...	16 29 38 ...	3 4·8	7 42 28 ...	9 59
23 ...	16 34 22 ...	2 11·9	7 45 17 ...	9 35
25 ...	16 38 58 ...	1 21·8	7 47 59 ...	9 11
27 ...	16 43 28 ...	0 32·8 N.	7 50 34 ...	8 47 N.

Comet 1888 *e* (Barnard).—Mr. W. R. Brooks discovered this comet independently on the following morning to that on which Mr. Barnard discovered it at Mount Hamilton.

Ephemeris for Berlin Midnight (*continued from* Nature, vol. xxxviii. p. 528).

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Oct. 12 ...	6 23 14 ...	6 59·5 N. ...	0·3523 ...	0·2550 ...	3·51
14 ...	6 19 19 ...	6 39·7			
16 ...	6 14 58 ...	6 18·6 ...	0·3466 ...	0·2265 ...	4·10
18 ...	6 10 12 ...	5 56·2			
20 ...	6 4 57 ...	5 32·4 ...	0·3410 ...	0·1972 ...	4·80
22 ...	5 59 12 ...	5 7·1			
24 ...	5 52 55 ...	4 40·3 N. ...	0·3354 ...	0·1672 ...	5·60

The brightness on September 2 has been taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 OCTOBER 14–20.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 14

Sun rises, 6h. 25m.; souths, 11h. 45m. 54·5s.; sets, 17h. 7m.: right asc. on meridian, 13h. 19·7m.; decl. 8° 25′ S. Sidereal Time at Sunset, 18h. 42m.

Moon (Full on October 19, 21h.) rises, 15h. 33m.; souths, 20h. 13m.; sets, 1h. 1m.*: right asc. on meridian, 21h. 48·6m.; decl. 15° 42′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	8 57	13 15	17 33	19 49·1	19 37 S.
Venus.....	8 49	13 23	17 57	14 57·1	17 4 S.
Mars......	12 15	15 57	19 39	17 31·6	24 53 S.
Jupiter....	10 29	14 40	18 51	16 14·7	20 43 S.
Saturn....	0 22	7 51	15 20	9 24·2	16 7 N.
Uranus...	6 2	11 33	17 4	13 6·9	6 28 S.
Neptune..	18 42*	2 28	10 14	4 0·7	18 53 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Oct.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. °	°
16	74 Aquarii	6	0 57	1 8	66	49
16	B.A.C. 8214	6½	21 21	—	203	—
20	μ Ceti	4	23 39	0 41†	63	338

† Occurs on the following morning.

Oct.	h.	
20	21	Mercury stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′			h. m.	
U Cephei	0 52·4	81 16 N.	Oct.	16,	3 11	*m*
Mira Ceti	2 13·7	3 29 S.	,,	15,		*M*
T Monocerotis	6 19·2	7 9 N.	,,	18,	3 0	*M*
U Geminorum[1]	7 48·5	22 18 N.	,,	15,		*M*
R Camelopardalis.	14 26·1	84 20 N.	,,	17,		*M*
T Ophiuchi	16 27·3	15 54 S.	,,	18,		*M*
U Ophiuchi	17 10·9	1 20 N.	,,	16,	19 46	*m*
Z Sagittarii	18 14·8	18 55 S.	,,	17,	19 0	*M*
β Lyræ	18 46·0	33 14 N.	,,	19,	22 0	*M*
R Lyræ	18 51·9	43 48 N.	,,	18,		*M*
η Aquilæ	19 46·8	0 43 N.	,,	20,	1 0	*M*
T Vulpeculæ	20 46·7	27 50 N.	,,	20,	23 0	*M*
Y Cygni	20 47·6	34 14 N.	,,	14,	3 0	*m*
			,,	17,	3 0	*m*
			,,	20,	3 0	*m*
δ Cephei	22 25·0	57 51 N.	,,	16,	23 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ξ′ Ceti	30°	9° N.	Slow; trained.
,, ε Arietis	42	20 N.	Swift.
,, ν Orionis	90	15 N.	The *Orionids.*
,, ζ Geminorum	105	22 N.	Swift; streaks.

GEOGRAPHICAL NOTES.

We notice in the last number of the *Izvestia* of the East Siberian Geographical Society (vol. xix. 1), a most interesting note, by L. A. Jaczewski, on the geological results of the last Sayan expedition. The immense border-ridge of the great plateau of East Asia, which stretches from the sources of the Iya to Lake Baikal, was very little known. Many explorers have visited the valleys of the Irkut and Oka which flow at its northern base, but very few have crossed it, and if they crossed the huge ridge, it was mostly to the north of Lake Kosogol, where a broad passage is opened from the lowlands to the high plateau. The Expedition of MM. Prein and Jaczews crossed it at three different places, and thus obtained an insight into its geological structure. As to its age, it appears that limestones, most probably Silurian, lie almost undisturbed at its northern base, so that the hypothesis as to the great plateau having been a continent since the Laurentian or Huronian epochs is thus confirmed. We notice also that, besides Munku Sardyk, 3500 metres high, there are in the Sayan at least three or four summits of nearly the same height; and that, viewed from the south on the banks of the Kirlygoi stream, it appears as a massive wall, 700 metres high, having a direction from the north-west to the south-east. As to the complex ramifications of the Sayan, they are chiefly due to a most extensive action of atmospheric agencies, as was foreseen by Tchersky. Most interesting observations were made as to the formerly quite unknown glaciers of the northern slope, where they have the shape of narrow glaciers descending down a very steep slope and taking their origin amidst wide snow-fields. Their lower extremities reach a lower level than on the southern slopes. As to the former extension of glaciers, which was maintained by Kropotkin, but doubted on account of prevailing theoretical conceptions as to the non-glaciation of Siberia, M. Jaczewski found plenty of striæ and striated boulders which made him consider that glaciers formerly extended to a level of 1500 metres on the northern slope, and 1700 metres on the southern slope turned towards the plateau.

The French Maritime Survey is sending a special mission to map the coasts of Madagascar. The officers will leave Paris in a few days, and are busy at the St. Maur Magnetic Observatory regulating their instruments for this purpose.

ELECTRICAL NOTES.

Prof. Fitzgerald (B. A. Address, Section A), in drawing attention to Hertz's experiments, has done the greatest possible service to electrical science. Hertz not only proves the existence of the ether, but the fact that an electric field is due to the oscillatory motions of the ether. Everyone who has the means will probably be repeating these experiments. The *Electrician* is publishing a capital *résumé* of Hertz's work by Mr. De Tunzelmann. Prof. Fitzgerald himself had predicted this result at Southport in 1882, and Prof. Oliver Lodge has actually measured these wave-lengths—the shortest ether wave measured being 3 yards—by extremely simple and beautiful experiments.

Acheson (Nature, July 26, p. 305) is pursuing in Pittsburg his inquiry into the influence of the disruptive discharges of powerful alternating currents. He confirms his formula, $\frac{E^3 \times K}{a} = d$, *d* being the sparking distance in inches and *a* a constant, and finds for

Dielectric.	Sparks between	*a*.
Air	points	135
Air	points and wire	263
Paraffin and cotton	,,	5822
Ozite and cotton	,,	7759

Ozite is a residuum of petroleum.

Lenard and Howard (*Electrotechnik Zeitschrift*, July 1888), have succeeded in making flat spirals of pure bismuth which, in the magnetic field, vary in resistance from 10 to 20 ohms, according to the strength of the field, and form a good practical mode of roughly measuring its intensity as suggested by Leduc.

Dr. Borgman, of St. Petersburg (*Phil. Mag.*, September 1888), has been experimenting on the transmission of electric currents through air when flames or points are used as electrodes. Some years ago, Prof. Hughes showed many of his friends similar experiments with telephones, but for some reason or other he has never published the results. The experiments were extremely interesting, as indeed are those of Borgman, who finds a difference in the surface resistance of the cathode and anode flames. He attributes much to the influence of light as studied by Hertz, Hallwachs, Wiedemann and Ebert, and Arrhenius. These results have a very important bearing on the new views of electrical action that are following from the inquiries of Fitzgerald, Hertz, Lodge, and others.

An extremely suggestive and very original paper was read at the British Association by Prof. Hicks, "On a Vortex Analogue of Static Electricity." Attractions, repulsions, lines of force, charge, positive and negative electrification, induction, strains of dielectrics—all the main phenomena of static electricity admit of explanation on the basis of hollow vortices in the ether. Moreover, the theory is applicable to chemical valency and to Faraday's law of electrolysis. It places Faraday's ideal lines of force on a basis of reality, and it adds one more nail to the coffin of the material theory of electricity which it is to be hoped has now been safely buried.

DURING a thunderstorm which lately burst over Barcelona, the captive balloon in the Exhibition was struck by a lightning-flash and destroyed. The connecting-rope was probably of wire.

THE lightning-conductor discussion at the Bath meeting of the British Association has raised the question of the oscillatory character of the Leyden jar discharge. This was suggested by Helmholtz, in 1852, as an explanation of the fact observed by Faraday, that when electrolysis of water took place through a Leyden jar discharge passing through it, the gases at each electrode were mixed H and O. It was proved by Thomson, in 1853, that if self-induction existed in the discharging circuit it must occur, and the oscillations were actually observed by Feddersen. The fact that needles and iron bars are magnetized militates rather against the theory, but Prof. Ewing (*Electrician*, October 5, p. 712) suggests that oscillations in which the period lengthens while their amplitude decays would account for magnetization in layers.

MOLECULAR PHYSICS: AN ATTEMPT AT A COMPREHENSIVE DYNAMICAL TREATMENT OF PHYSICAL AND CHEMICAL FORCES.[1]

III.

PART II.—ELECTRICITY AND MAGNETISM.

§ 12. *Electrostatic Attraction.*

THOMSON'S investigations, considered in § 1 (August 23, p. 404), rest on the assumption that the diameter of a molecule or atom is indefinitely small in comparison with the wave-length of the light, and therefore the conclusions do not hold good for light-vibrations of such small wave-length as to be comparable with the molecular diameters. The consideration of vibrations of this kind shows that they give rise to what are called electrical phenomena.

These vibrations, like the former, will affect the internal energy of the molecules, and the molecules will also have critical periods with respect to them. But instead of assuming, as before, that within a finite but very short interval, only one wave impinges upon a molecule, it must now be assumed that an indefinitely large number of waves impinge upon the molecule at the same time, and that the effect of these waves is of a constant character. Suppose a sphere of a diameter differing only by an indefinitely small amount from that of a molecule, to be separated from the ether, and let vibrations of short wave-length impinge upon it from a fixed point, P. The first step will be to determine the energy, due to these vibrations, of the ether within the sphere.

Let r_0 be the least and r_1 the greatest distance of P from the spherical surface. The energy will be inversely proportional to the square of the distance, so that, where κ is a constant, the energy of the vibrating ether within the sphere will be—

$$\int_{r_0}^{r_1} \frac{\kappa dr}{r^2} = \kappa\left(\frac{1}{r_0} - \frac{1}{r_1}\right) = \frac{\kappa\delta}{r^2},$$

where $\delta = r_1 - r_0$, and r lies between r_0 and r_1.

Now consider a finite space bounded by spherical surfaces of radii R_0 and R, having their common centre at P, and by a cone with its vertex at P, and suppose it to be filled with spheres of diameters indefinitely near to those of molecules; then a finite number of concentric spherical surfaces may be inserted between the two bounding spheres, at distances equal to the diameter of a molecule. The number of small spheres between any pair of these spherical surfaces will be proportional to the spherical surface included within the cone, so that, if $d\sigma^i$ is the element of surface of the sphere of radius R_i, the total energy of the ether within the space considered will be proportional to—

$$\frac{\delta}{R_0^2}\int d\sigma_0 + \frac{\delta}{R_1^2}\int d\sigma_1 + \ . \ . \ . \ .$$

If, however, we assume that the small spheres are not sufficiently numerous to completely fill the space, but that they may all be arranged along a circular arc of radius R, then R_i^2 in these denominators must be replaced by R_i, so that, writing dR for δ, we find for the total energy—

$$\sum \frac{\delta}{R_i}\int d\sigma_i = \int_{R_0}^{R} \frac{dR}{R}\int d\sigma = \iiint \frac{dx\,dy\,dz}{R},$$

where $dx\,dy\,dz$ represents an element of volume in the most general form. We therefore obtain the following important result:—

If a portion of space infinitely large in proportion to the diameter of a molecule contains a number of spheres of the size of a molecule, so sparsely scattered that they can all be arranged on a surface within the space, then the total energy of the ether within all these spheres will be the same as if the space were completely occupied by the spheres, and the energy of each element of space were inversely proportional to the first power of the distance of the element from the point P.

Now suppose these spheres to be replaced by molecules with a similar scattered distribution, then the vibrations corresponding to their critical periods will increase their energy, while vibrations of different period will traverse the space unaltered, and therefore the molecules may still be regarded as specially susceptible to certain vibrations of very short period, just as in the case of luminous vibrations. Let KR^{-1} be the energy of the ether within the space occupied by the molecules, then the ponderable portions of the molecules will have their energy increased by an amount θKR^{-1}, where θ is a proper fraction—that is to say, a force varying inversely as the square of the distance will act on the ponderable molecules.

Now, it was shown in § 1 that for comparatively slow molecular motions the ether behaves like a perfect fluid, and therefore it follows from the principles of hydrodynamics that the molecules must move in the direction in which the energy of the surrounding ether diminishes most rapidly—that is, towards P; for the increase in the energy of a molecule as it approaches P must be accompanied by a decrease in the energy of the ether surrounding it.

It therefore follows that the vibrations of very short wave-length proceeding from P will have the same effect as if P had a charge of electricity, which suggests that electrostatic phenomena may be due simply to these vibrations in the ether, and it will be found that further investigation confirms this conclusion. For the sake of brevity, the internal energy of a molecule due to vibrations of the short wave-length here considered will henceforth be called electrical energy, and a molecule will be said to be electrically excited when its electrical energy differs from zero. The demonstration given in § 5 (p. 407), that there is a maximum value for the possible internal energy of a molecule, will apply also to the present case, so that there will be a maximum possible value of the electrical energy of a molecule, depending upon the values of the constants which determine its internal constitution. This result leads to the following proposition:—

Two electrically excited particles will attract each other when the electrical energy of either one of them is, under the existing circumstances, susceptible of further increase. In the opposite case there will be repulsion.[1]

The truth of the latter portion of the preceding proposition is easily seen, for if two equally excited particles, or two excited to the maximum amount, were to approach each other, the energy of the intervening ether would increase in the direction of motion, for the ether at a point in the neighbourhood of one of the particles would receive an increase of energy from the approach of the other, while there could be no absorption of energy by the molecule. This would, however, be in contradiction with the law of hydrodynamics according to which the motion takes place in the direction of decreasing energy.[2]

[1] A Paper read before the Physico-Economic Society of Königsberg, by Prof. F. Lindemann, on April 5, 1888. Continued from p. 461.

[1] The action of electrified glass and sealing-wax on each other and on pith-balls is easily explained from this. The difference between positive and negative electricity being merely relative, appears, too, to remove a good many difficulties in the explanation of electrostatic phenomena.

[2] We therefore assume the truth of Maxwell's theory that light-vibrations exert a pressure in the direction of propagation ("Electricity and Magnetism," § 792); this will only be modified when the vibrations are absorbed by the ponderable molecules.

To determine exactly the conditions for attraction and repulsion respectively, let M be the electrical energy, at unit distance, of a vibration proceeding from P, then the energy at the distance R is MR^{-1}, as far as its effect on a molecule is concerned. Suppose a portion, ϵMR^{-1}, of this to be absorbed where ϵ is a proper fraction, then the repulsive force will be proportional to the negative differential coefficients of $(1-\epsilon)MR^{-1}$, and there will be at the same time an attractive force proportional to the differential coefficient of ϵMR^{-1}. The total repulsive force will therefore be proportional to $(1-2\epsilon)MR^{-2}$; its maximum value will be attained for $\epsilon = 0$; it will be zero for $\epsilon = \frac{1}{2}$: it will be attractive for $\epsilon > \frac{1}{2}$, and the attractive force will reach its maximum value for $\epsilon = 1$—that is to say, when the whole of the energy is absorbed. This may take place when the two attracting or repelling particles are of the same substance. The expressions for these forces contain, in addition to R, a factor M depending only on the attracting particle, and a factor $1-2\epsilon$ depending only on the attracted particle. In the same way the second particle will exert upon the first P a force proportional to $(1-2\eta)NR^{-2}$, where η depends only on the first particle, and N only on the second. The electrostatic potential of the mutual action will therefore be—

$$-\frac{(1-2\epsilon)(1-2\eta)MN}{R} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

M and N measure the electricity radiated from the two particles respectively—that is to say, the excess of the internal electrical excitation of the two particles over that of the surrounding ether. This excess may be negative, and therefore two unelectrified particles may repel each other (when $\epsilon = 0$, $\eta = 0$) provided the surrounding medium is excited. The next step would be to determine the further motion of an attracted or repelled electrified particle, but since electricity in motion behaves quite differently from electricity at rest, as will be shown to follow from the author's theory, the consideration of this problem must be postponed, but it may be noted here that an attracted particle can only continue to approach the attracting particle so long as its maximum energy has not been attained. They may therefore either continue to approach until they come into contact, or may cease to approach at a certain critical distance. The latter possibility does not seem allowable according to experience, and in fact is found to be excluded when the motion is more fully considered, and the author merely mentions it in this place to call attention to its relation to the objections brought by von Helmholtz against Weber's theory.

Attempts have already been made to explain Newtonian gravitation from electrostatic actions.[1] The attempt to explain gravitation in this manner derives additional interest from the author's theory of electrostatic action, according to which the earth receives from the sun's rays, not only heat and light, but also electrical energy.

The theory of planetary motion should be capable of being derived from the laws of electro-dynamics, and the author's theory may therefore possibly prove of great value for the explanation of the phenomena of terrestrial magnetism, of meteorology, and may perhaps also throw some light upon the nature of comets.

§ 13. *Electro-dynamic Potential of Two Currents.*

Electrostatic action may be compared, according to the author's theory, with heat radiation, since both series of phenomena are due to the transference of energy from the ether to ponderable molecules. Similarly, heat conduction may be compared with electrical conduction. A body will be defined as a conductor when its molecules, in virtue of specially favourable values of its critical periods and other constants, are so sensitive to electrical energy as to easily absorb the maximum amount of internal energy, after which the centres of gravity of the molecule will begin to execute exceedingly small vibrations, which will be transmitted from molecule to molecule, accompanied by an absorption of electrical energy by each molecule, in exactly the same way that the molecules become luminous by the absorption of energy in the form of heat vibrations. Conduction, then, will take place by electrostatic radiation from molecule to molecule.[1] Those substances, on the other hand, in which the molecules absorb with difficulty the maximum amount of electrical energy, or in which internal electrical vibrations are only excited with difficulty, will be non-conductors.

The energy of an electrical vibration is inversely proportional to the square of the period of vibration, and therefore to the square of the wave-length, λ. A very good conductor (and these alone are considered in electro-dynamics) must have a very large number of critical wave-lengths lying so close together that their sum may be represented by a definite integral. Let λ_1 be the smallest, and λ_2 the greatest, of the electrical wave-lengths to be considered in any given case, then the internal electrical energy of the molecule will be proportional to

$$\int_{\lambda_1}^{\lambda_2}\frac{d\lambda}{\lambda^2} = \frac{1}{\lambda_1} - \frac{1}{\lambda_2} = \frac{\lambda_2 - \lambda_1}{\lambda^{1^2}},$$

where λ^1 is a value of λ lying between λ_1 and λ_2. Owing to the number of critical wave-lengths being necessarily very large, $\lambda_2 - \lambda_1$ will be a finite quantity in comparison with λ^1. We therefore arrive at the conclusion that the total internal electrical energy of a molecule of a good conductor is inversely proportional to a certain mean critical wave-length λ^1.

If we now make the assumption that the electrified particles are moving relatively to each other with a given velocity, their mutual electrostatic action will be modified in the same manner as if the wave-length of the electrical vibration proceeding from each of them were increased or diminished by an amount $\Delta\lambda$. Let c be the velocity of light, and ρ the relative velocity of the two electrified particles, in the direction of the line joining them, then we know that $\Delta\lambda = \lambda\rho/c$. Let r be the initial distance between the particles, and $E/r\lambda = M/r$ the initial electrostatic potential of one due to the presence of the other, then during the motion it will be—

$$\frac{E}{r(\lambda+\Delta\lambda)} = \frac{E}{r\lambda\left(1+\frac{\rho}{c}\right)} = \frac{M}{r}\left(1 - \frac{\rho}{c} + \frac{\rho^2}{c^2} - \ldots\right).$$

Let ds be the element of length of the first conductor, and ds' that of the second, and let θ and θ' be the angles which they make with the joining line, then—

$$\rho = \frac{ds}{dt}\cos\theta - \frac{ds'}{dt}\cos\theta' \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

To determine the mutual action of the two current elements, each element must be assumed to consist of a pair of molecules, one of which has transmitted electrical energy to the other without having itself received a fresh supply, an assumption in complete accordance with the representation of a molecule as consisting of a series of distinct shells, and which takes the place of the assumption usually made that at each moment the quantities of positive and negative electricity on every current-element are equal. The two original elements will repel each other if the internal energy is electrically excited to an equal extent, or to the maximum amount possible in each. In order to fix the ideas this may be assumed to be the case in what follows.

Let 1, 2, represent the two molecules of the element ds, and 1′, 2′, those of ds', then the mutual potential of the two elements will be represented by the sum—

$$P_{22'} + P_{12'} + P_{1'2} + P_{11'};$$

where P_{ik} represents the mutual potential of two molecules i and k. The author takes the potential such that its positive differential coefficient in any direction is equal to the component of force in that direction, and therefore we have—

$$P_{22'} = -\frac{M}{r}\left(1 - \frac{\rho}{c} + \frac{\rho^2}{c^2} \ldots\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

$$P_{12'} = \frac{M}{r\left(1+\frac{ds}{dt}\frac{\cos\theta}{c}\right)} = \frac{M}{r}\left(1 - \frac{ds}{dt}\frac{\cos\theta}{c} + \left(\frac{ds}{dt}\frac{\cos\theta}{c}\right)^2 - \ldots\right) \ldots (31)$$

$$P_{1'2} = \frac{M}{r\left(1-\frac{ds'}{dt}\frac{\cos\theta'}{c}\right)} = \frac{M}{r}\left(1 + \frac{ds'}{dt}\frac{\cos\theta'}{c} + \left(\frac{ds'}{dt}\frac{\cos\theta'}{c}\right)^2 + \ldots\right) \ldots (32)$$

$$P_{11'} = -\frac{M}{r} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

[1] By Mossotti, for example, in 1836; see Zöllner's "Wissenschaftliche Abhandlungen," vol. ii. p. 417 (Leipzig, 1878). On p. 16 *et seq.*, various hypotheses regarding action at a distance are collected together, but the author states that he does not agree with Zöllner's criticisms on them. See also Maxwell's "Electricity and Magnetism," Articles 37, 59, *et seq.*, and 846 *et seq.*

[1] Kundt has recently shown that heat conduction is probably effected in a similar manner (*Sitzungsberichte der Berliner Akademie*, 1888, p. 271).

The constant M, according to (28), depends on the two current elements, and measures the electrical energy of the medium between them.

In (30) $\epsilon = 0$, $\eta = 0$; in (31) $\epsilon = 0$, $\eta = 1$; in (32) $\epsilon = 1$, $\eta = 0$; in (33) $\epsilon = 1$, $\eta = 1$.[1]

Substituting for ρ its value from (29), and neglecting the second and higher powers, we find for the electro-dynamic potential of the two current elements—

$$dV = \frac{2M}{c^2} \cos\theta \cos\theta' \, ds ds' \quad . \quad . \quad . \quad . \quad . \quad (34)$$

which gives for the potential of two closed circuits—

$$V = \frac{2}{c^2} \int\int M \cos\theta \cos\theta' \, ds ds' \quad . \quad . \quad . \quad . \quad (35)$$

where M is an electrostatic constant and c the velocity of light.

In the case of closed circuits we know that the value of V remains unchanged if $\cos\theta \cos\theta'$ is replaced by $\cos(ds, ds')$, and therefore we arrive at Neumann's expression for the mutual potential of two closed circuits, namely—

$$V = \frac{2}{c^2} \int\int M \cos(ds, ds') \, ds ds' \quad . \quad . \quad . \quad . \quad . \quad (36)$$

These expressions for V have been obtained by neglecting the second and higher powers of ρ/c, $1/c \, . \, ds/dt$, and $1/c \, . \, ds'/dt$; moreover, the dependence of the energy on the wave-length was only expressed in terms of a mean value, λ'; so that the expressions are only to be considered as approximately true. It is evident that they cannot hold good if either of the quantities ρ, $\frac{ds}{dt}$, or $\frac{ds'}{dt}$ become equal to or greater than the velocity of light—that is to say, both the relative and absolute velocities of the particles must be less than that of light; and it will be shown in what follows that this limitation is of the utmost importance.[2]

§ 14. *Weber's Fundamental Law.*

von Helmholtz has investigated the mutual potential of two current elements on the assumption that it is of the form—

$$\frac{M}{c^2}\left\{(1 + \kappa) \cos(ds, ds') + (1 - \kappa) \cos\theta \cos\theta'\right\} ds ds'.$$

Putting $\kappa = -1$, this expression agrees with Weber's law and also with (34), showing that the author's theory leads to Weber's law. In fact, putting $\theta = 0$, $\theta' = \pi$, and $ds = ds' = dr/2$, and taking the sum[3] of the electrostatic and electro-dynamic potentials, we arrive at Weber's expression for the potential of the two particles, namely—

$$-\frac{M}{r}\left\{1 - \frac{1}{2c^2}\left(\frac{dr}{dt}\right)^2\right\},$$

and the author's expression for dV leads to Weber's expression for the repulsion between two particles, namely—

$$\frac{M}{r^2}\left\{1 - \frac{1}{2c^2}\left(\frac{dr}{dt}\right)^2 + \frac{r}{c^2}\frac{d^2r}{dt^2}\right\}.$$

von Helmholtz's objections against Weber's law must now be considered, and his own examples may be taken.[4]

[1] All the electric rays proceeding from 2 will not be absorbed by 1′ unless (§ 12) the two conductors are of the same material; if they are of different material, ϵ and η can only approximately assume the value unity, and therefore the expression (35) will only give an approximate value of the mutual potential. From a physical point of view, it would perhaps be more reasonable to assume that the particles in the elements ds and ds' respectively, instead of being, one strongly electrified and one unelectrified, are distributed in an approximately regular manner throughout all the intermediate stages. In this case the sum of the four expressions (30)–(33) will have to be replaced by a double integral, of which this sum will be the mean value.

[2] These conditions are known experimentally to be fulfilled, for while the velocity of light is about 300,000 kilometres a second, that of electricity in wires is, according to Fizeau, Gounelle, Fröhlich, and W. Siemens, from 100,000 to 260,000 kilometres a second. See Sir W. Thomson, "Mathematical and Physical Papers," vol. ii. p. 131, and Wüllner's "Experimental Physik," vol. iv. p. 403, 4th edition. According to the author's theory, the propagation of electric waves *in vacuo* must take place with the velocity of light; but the theory would not be affected if the velocity in air were found to be different. See von Helmholtz, "Wissenschaftliche Abhandlungen," vol. ii. p. 629 *et seq.* In fact, Hertz has found this velocity to be distinctly greater than that of light (*Sitzungsberichte der Berliner Akademie*, February 1888). The increase may be due to the electrical excitation of the air particles, and their consequent repulsive action on one another. With respect to electro-dynamic determinations of the constant c, see Himstedt, *Wiedemann's Annalen*, vols. xxviii. and xxix.

[3] See Riemann, "Schwere, Electricität, und Magnetismus," §§ 96 and 97. It should be noted that Riemann uses c to denote the velocity of light multiplied by $\sqrt{2}$. It may also be noted that the author uses ds/dt and ds'/dt to denote the velocity of propagation of an electrical disturbance, and not directly that of a molecule.

[4] "Wissenschaftliche Abhandlungen," vol. ii. p. 636 *et seq.* The two equations which follow may be interpreted as meaning that the quantity of electricity in motion depends on r, which is in agreement with § 12.

Suppose a ponderable electrified particle of mass μ to be repelled by a stationary quantity of electricity at the origin, in the direction of the joining line r. Let a force R of the ordinary kind act on the mass μ so as to diminish r, then the differential equation of motion of the electrified particle will be—

$$\mu\frac{d^2r}{dt^2} = \frac{M}{r^2}\left\{1 - \frac{1}{2c^2}\left(\frac{dr}{dt}\right)^2 + \frac{r}{c^2}\frac{d^2r}{dt^2}\right\} + R;$$

or, putting $M = \rho\mu c^2$—

$$\mu\left(1 - \frac{\rho}{r}\right)\frac{d^2r}{dt^2} = \frac{M}{r^2}\left\{1 - \frac{1}{2c^2}\left(\frac{dr}{dt}\right)^2\right\} + R.$$

Choosing the initial circumstances, so that $t = 0$, when the velocity and the work done by R are both zero, and supposing that r then has the value $\mathfrak{r}$, the principle of conservation of energy gives—

$$\frac{1}{2}\mu\left(1 - \frac{\rho}{r}\right)\left(\frac{dr}{dt}\right)^2 = M\left(\frac{1}{\mathfrak{r}} - \frac{1}{r}\right) + \Re,$$

where

$$\Re = \int_0^t R\frac{dr}{dt}dt.$$

If, now, $Rr^2 < -M$, von Helmholtz points out that the moving particle must always approach the stationary one; its velocity meanwhile increases without limit until, for a distance $r = \rho$ (the so-called critical distance, see § 12), it becomes infinite, so that a finite force can give an infinitely great velocity to a mass μ by a finite expenditure of work. This impossible result is not, however, a consequence of the author's theory, owing to the limitations stated at the end of § 13. For if the velocity $\frac{dr}{dt}$ increases without limit, it must exceed that of the velocity of light, and then Weber's law ceases to hold good.

It would be easy, by expanding the four previously-considered partial potential expressions, in terms of c/ρ, $c/ds/dt$, and $c/ds'/dt$, to obtain a law for the further motion; but there is no object in doing so, as it will be seen from what follows that this new law would again only hold up to a certain limit not far removed from the first.

In the first place, it is doubtful whether, when moving so rapidly, the ponderable molecules could traverse the ether without resistance. In the second place, the electrical energy transferred from the fixed origin to the moving particle has been assumed to be inversely proportional to the wave-length, and the latter has been regarded as varying gradually within the given limits. This was allowable for good conductors, since their molecules must be specially sensitive to electrical disturbance, and therefore have a very large number of very small critical periods. With the very great velocity assumed, the wave-lengths of the disturbances proceeding from the origin will be greatly shortened before acting on the mass μ. It will follow, therefore, that only such vibrations will cause electrical excitation which already have so great a wave-length that they will really appear as light, or ultra-violet, vibrations, and not as electrical vibrations. Now, in the case of all known substances, these critical wave-lengths do not come together in great numbers, and therefore cannot be treated as forming a continuous series.

If such rays are emitted from the origin, they can only give rise to electrical excitation by separate impulses, and will therefore only cause a slight temporary variation in the acceleration of the particle μ due to the steady action of the force R.

We may therefore conclude that a particle easily susceptible of electric excitation will be electrified if it is made to approach a source of light with very great velocity, and this the more readily, the higher the refrangibility of the light from the source. The requisite velocity must exceed that of light by a definite amount.

The author is not aware that this conclusion has as yet been directly verified by any experimental evidence, unless Hertz's observations of the effect of light on the electric spark[1] may be explained in this way, but it is indirectly supported by the phenomena observed in Geissler tubes, as will be shown below. Consider, moreover, the motion of the particle μ away from the origin at an equally great velocity, then electrical waves proceeding from the origin will be lengthened, and act on the particle as light waves, causing it to glow. This electric glow will first appear of a blue colour, gradually passing through the various colours of the spectrum towards the red, as the velocity further

[1] *Sitzungsberichte der Berliner Akademie*, 1887, pp. 487 and 895.

increases, and of this electric glow many instances could be cited, both in Nature and in the laboratory.

Consider, in the first place, the glow surrounding a point from which an electric discharge is taking place. By means of the electrical repulsion, the density of the air immediately surrounding the point will be so far diminished that a single air-particle will be able to traverse a sensible distance with a very great velocity, and therefore give rise to the glow. Here it is not a question of particles becoming electrically excited by radiation from the point, but of those which are electrified by actual contact with it. As soon as they have lost some of their electrical energy they will again become sensitive to electrical radiation. There must therefore be a dark space immediately surrounding the point, and outside this an electric glow, which explains a well-known phenomenon always observed in the rarefied atmosphere of a Geissler tube. The stratification can also be explained very simply, for the glow causes a diminution in velocity, for when the electrical waves from the positive electrode give rise to luminous instead of electrical vibrations in the particles of gas, the repulsion will be diminished, and therefore the velocity will gradually become less than that of light, when the particle will again become sensitive to the electrical radiation. The velocity will therefore again increase until the glow appears again, thus giving rise to a stratified appearance. The velocity in the glowing layers will naturally be greatest in the neighbourhood of the positive electrode, and here, therefore, light will be given off of all the colours corresponding to the critical periods of the gas contained in the tube, which is in accordance with observation. According to the author's theory, the electrical excitation takes place by the transference of ponderable gas molecules from the positive to the negative electrode. After they have parted with their electrical energy to the latter, they will return in an unelectrified condition to the positive electrode to which they will be attracted, and at the same time repelled from the negative electrode. There will be no dark space surrounding the negative electrode, because the particles leaving it will have little or no electrification. The velocity of the returning molecules will increase as they approach the positive electrode, so that there can be no further transformation of electrical into luminous energy. In very high vacua the velocity of the returning particles may become great enough for electrical energy to be excited in them by the red glow of the positive pole, by which their velocity will be still further increased. The velocity of the returning particles will in this case ultimately become so much greater than that of the luminous molecules moving away from the positive electrode as to cause a sensible increase in the density of the gas surrounding it. The result of this will be to prevent the formation of the positive glow, and the whole tube will become filled by the negative glow. The density in the neighbourhood of the negative electrode will therefore be diminished, and the returning molecules will leave it with still greater velocity. If both electrodes are at one end of the tube, the molecules returning towards the positive electrode will be deflected by the layer of dense gas surrounding it, against the sides of the tube, giving rise to fluorescent phenomena, as explained in § 11 (September 6, p. 461). If the complicated phenomena which have recently been observed in Geissler tubes by Crookes and Hittorf can be thus simply explained, it will afford an important confirmation of the author's theory.

These considerations may be applied to the explanation of many cosmical phenomena, such as the aurora and the light of comets. It is quite possible that the particles of a comet's tail moving with great velocity towards the sun may become electrified by means of the sun's light.

The formulæ previously obtained are applicable to the determination of the motion of an electrified particle, in the case in which no proper luminous vibrations are given off from the origin, or where these may be neglected, for the equations (29) to (33) give in this case for $\frac{dr}{dt} = c$, $r = r_0$, $\mathfrak{R} = \mathfrak{R}_0$, and consequently—

$$\frac{\mu}{2}\left(1 - \frac{\rho}{r_0}\right)c^2 = M\left(\frac{1}{r} - \frac{1}{r_0}\right) + \mathfrak{R}_0.$$

Also—

$$\frac{\mu}{2}\left[\left(\frac{dr}{dt}\right)^2 - c^2\right] = -\frac{2M}{r} + \frac{M}{r_0} + \frac{Mc}{r\frac{dr}{dt}} + \mathfrak{R} - \mathfrak{R}_0.$$

And dr/dt can hence only become infinite when the positive quantity $\mathfrak{R}$ becomes infinite, or $r = 0$. von Helmholtz's objections, therefore, do not apply to this equation.

§ 15.—*Electrical Excitation.*

The foregoing theory easily explains the different methods of electrical excitation.

(1) The friction of two bodies sets their molecules into vibration, which appears in the form of heat. The resulting impacts of neighbouring molecules will most readily excite internal vibrations of the critical periods, for which they are specially sensitive. If the molecules are exceptionally sensitive to vibrations of very short periods, they will be easily electrified, the process being exactly analogous to the production of luminous vibrations by heating gases, as described in § 4 (August 23, p. 407). Electro-positive bodies will be those which are most sensitive, and these will, according to the theory, attract other less electrified bodies. In the ordinary frictional electrical machine the glass will therefore be more strongly excited than the rubber. The explanation of the collecting action of points on the prime conductor is given by the consideration that at a point the molecules are more fully exposed to the electrical radiation from the glass plate, and being electrically excited by this radiation communicate their electrification to the prime conductor by conduction, as explained in § 13.

(2) Electrification by the action of heat takes place in the same manner, and it is clear that the molecules in crystals, being regularly disposed with their axes in definite directions, will be electrified. Thermo-electrical currents are also explained. For if one of the junctions of a circuit consisting of two dissimilar metals is heated, the more sensitive metal will receive more electrical energy than the other, and give rise to a positive current. The potential difference at the junction will depend on the internal constants of the molecules in the two metals, so that we cannot expect to be able to express it by any simple general law.

(3) Electrification by simple contact of two dissimilar metals is not so easily explained if the effects of heat, pressure, and friction are excluded. It is, however, possible that the close contact of differently vibrating molecules may disturb the internal and therefore the external energy, and thus give rise to electrification. The electrification of similar metals by contact could be explained in the same way.

(4) Electrification by chemical action is completely explained by the author's theory, the production of electrical vibrations by this means being exactly analogous to the similar production of heat- and light-vibrations. Such chemical action must, in the author's opinion, play an important part in the galvanic cell, though contact electrification may also have a share in the action. The contact between copper and sulphuric acid, for example, is a very intimate one. At ordinary temperatures the molecules of both substances will be in motion. When two different molecules collide, their internal equilibrium will be destroyed, and they will therefore, according to § 8 (September 6, p. 460) form a chemical compound, provided the critical vibrations of the compound are, at the given temperature, less easily excited than those of the separate elements, which we must assume to be the case, from the strong chemical affinity which is experimentally known to exist between copper and sulphuric acid. During this process electrification will take place if the maximum internal electrical energy is less for the compound than for the constituents, exactly as hydrogen in combining with oxygen to form water produces light, and chlorine in combining with hydrogen to form hydric chloride produces heat. The electricity set free will be carried away by the copper, the latter being a good conductor. The accumulation of electricity in the copper is prevented, however, by its being used up again in forming a chemical compound with the zinc.

G. W. DE TUNZELMANN.

(*To be continued.*)

COMPRESSIBILITY OF WATER, SALT WATER, MERCURY, AND GLASS.[1]

THE pressures employed in the experiments ranged from 150 to 450 atmospheres, so that results given below for higher or lower pressures [and inclosed in square brackets] are extrapolated.

[1] Extracted, with the sanction of Dr. Murray, from a Report by Prof. Tait, now in type for a forthcoming volume of the *Challenger* publications.

A similar remark applies to temperature, the range experimentally treated for water and for sea-water being only 0° to 15° C. Also it has been stated that the recording indices are liable to be washed down the tube, to a small extent, during the relief of pressure, so that the results given are probably a little too *small*.

Compressibility of mercury, per atmosphere	0·0000036
,, ,, glass	0·0000026

Average compressibility of fresh water per atmosphere—

[At low pressures	$520 . 10^{-7}$ –	$355 . 10^{-9}t$ +	$3 . 10^{-9}t^2$]
For 1 ton = 152·3 atm.	504	360	4
2 ,, = 304·6 ,,	490	365	5
3 ,, = 456·9 ,,	478	370	6

The term independent of t (the compressibility at 0° C.) is of the form—

$$10^{-7}(520 - 17p + p^2),$$

where the unit of p is 152·3 atmospheres (1 ton-weight per square inch). This must not be extended in application much beyond $p = 3$, for there is no warrant, experimental or other, for the minimum which it would give at $p = 8·5$.

The point of minimum compressibility of fresh water is probably about 60° C. at atmospheric pressure, but is lowered by increase of pressure.

As an *approximation* through the whole range of the experiments we have the formula—

$$\frac{0·00186}{36 + p}\left(1 - \frac{3t}{400} + \frac{t^2}{10000}\right);$$

while the following formula exactly represents the average of all the experimental results at each temperature and pressure—

$$10^{-7}(520 - 17p + p^2) - 10^{-9}(355 + 5p)t + 10^{-9}(3 + p)t^2.$$

Average compressibility of sea-water (about 0·92 of that of fresh water)—

[At low pressures ...	$481 . 10^{-7}$ –	$340 . 10^{-9}t$ +	$3 . 10^{-9}t^2$]
For 1 ton ...	462	320	4
2 ,, ...	447·5	305	5
3 ,, ...	437·5	295	5

Term independent of t—

$$10^{-7}(481 - 21·25p + 2·25p^2).$$

Approximate formula—

$$\frac{0·00179}{38 + p}\left(1 - \frac{t}{150} + \frac{t^2}{10000}\right).$$

Minimum compressibility point, probably about 56° C. at atmospheric pressure, is lowered by increase of pressure.

Average compressibility of solutions of NaCl for the first p tons of additional pressure at 0° C.:—

$$\frac{0·00186}{36 + p + s},$$

where s of NaCl is dissolved in 100 of water.

Note the remarkable resemblance between this and the formula for the average compressibility of fresh water at 0° C., and $p + s$ tons of additional pressure.

[Various parts of the investigation seem to favour Laplace's view that there is a large molecular pressure in liquids. In the text it has been suggested, in accordance with a formula of the kinetic theory of gases, that in water this may amount to about 36 tons-weight on the square inch. In a similar way it would appear that the molecular pressure in salt solutions is greater than that in water by an amount directly proportional to the quantity of salt added.]

Six miles of sea, at 10° C. throughout, are reduced in depth 620 feet by compression. At 0° C. the amount would be about 663 feet, or a furlong. (This quantity varies nearly as the square of the depth). Hence the pressure at a depth of 6 miles is nearly 1000 atmospheres.

The maximum-density point of water is lowered about 3° C. by 150 atmospheres of additional pressure.

From the heat developed by compression of water I obtained a lowering of 3° C. per ton-weight per square inch.

From the ratio of the volumes of water (under atmospheric pressure) at 0° C. and 4° C., given by Despretz, combined with my results as to the compressibility, I found 3°·17 C.; and by direct experiment (a modified form of that of Hope) 2°·7 C. The circumstances of this experiment make it certain that the last result is too small.

Thus, at ordinary temperatures, the expansibility of water is increased by the application of pressure.

In consequence, the heat developed by sudden compression of water at temperatures above 4° C. increases in a higher ratio than the pressure applied; and water under 4° C. may be heated by the sudden application of sufficient pressure.

The maximum density coincides with the freezing-point at – 2°·4 C., under a pressure of 2·14 tons.

SCIENTIFIC SERIALS.

In the *Journal of Botany* for August and September, a considerable portion is occupied by the continuation of papers, to which reference has already been made—Messrs. Britten and Boulger's biographical index of British and Irish botanists, and Mr. G. Murray's catalogue of the marine Algæ of the West Indian region.—Mr. W. H. Beeby records an addition to the British Phanerogamic flora in *Callitriche polymorpha*.—Mr. A. Fryer has some critical remarks on *Potamogeton fluitans*.—A number of new ferns from Western China, and from Manipur, in India, are described by Mr. J. G. Baker and Colonel Beddome.

The numbers of the *Botanical Gazette* for June–August contain quite an unusual number of articles of general interest. Bryologists will find a description of eight new species of moss from North America, each illustrated by a plate; in fact, the plates in these three numbers are very numerous and excellent.—Mr. Chas. Robertson discusses the origin of zygomorphic flowers from the point of view of evolution.—Of flowering plants, we have descriptions of new species from Western America (chiefly Umbelliferæ) and from Guatemala, by Messrs. Coulter and Rose and Mr. J. D. Smith.—Mr. F. C. Newcombe describes the mode of dissemination of the spores of Equisetum in the splitting of the sporange and the carriage of the spores by means of the elaters.—Mr. A. F. Förste describes (with a plate) the adaptation to cross-fertilization in various species.

American Journal of Mathematics, 1888 (Baltimore, Johns Hopkins University).—The object of M. R. Liouville's paper, "Sur les lignes géodésiques des surfaces à courbure constante," with which vol. x. No. 4 opens, is stated by him to be "d'indiquer la signification géométrique des équations différentielles du second ordre ayant leur intégrale générale linéaire par rapport aux constantes arbitraires, et de former leurs invariants pour toutes les substitutions qui ne changent point, soit l'inconnue, soit la variable indépendante" (pp. 283–292).—The following memoir, on the primitive groups of transformations in space of four dimensions, by James M. Page, is likely to be very serviceable, as it is the first continuous account in English of the researches of Sophus Lie on the theory of groups of transformations. Lie himself has developed the theory in a series of papers which date from 1873, and has not published any connected work on the subject (pp. 293–346).—W. C. L. Gorton writes on line congruences. He treats the subject by quaternions, and obtains all Kummer's results (*Crelle*, vol. lvii.), and is enabled by his method to carry out certain steps which are only indicated by this writer (pp. 346–367).—The volume closes with a notelet by Prof. Franklin, entitled "Some Theorems concerning the Centre of Gravity." This contains "almost instantaneous" proofs of Lagrange's two theorems on the centre of gravity.

With vol. xi. No. 1, we have what strikes us as being an admirable likeness of the great French mathematician, Charles Hermite. We have previously expressed our pleasure at this new departure of the editors of this journal, and hope their catering for mathematicians will meet with material approval.—The first communication is a memoir on a new theory of symmetric functions, by Captain P. A. Macmahon, R.A. This prolific young mathematician is doing excellent work, and the pages of the journal are just suited to present his results in the most effective form. The paper is intimately connected with a recent one, by the same writer, communicated to the London Mathematical Society, in which he gives a sketch of an extension of the algebra of the theory of symmetrical functions, and establishes the basis of a wide development. "The main object of the memoir is to show clearly the proper place of the 'symmetric function tables' as studied by Hirsch, Cayley, Durfee, and others, in the algebra of such functions; to point out that the fact of their existence depends upon a wide theorem of algebraic reciprocity which leads to an equally wide theorem of algebraic expressibility, and that they are a particular case, and not the most important case from the point of view of application, of a system of such tables" (pp. 1-36).—Prof. W. W. Johnson contributes a paper on the integrals in series of binomial differential equations (pp. 37-54). "Binomial equation" is here used in Boole's sense.—Some interesting geometrical results are given in the next paper, by M. d'Ocagne, "Sur certaines courbes qu'on peut adjoindre aux courbes planes pour l'étude de leurs propriétés infinitésimales" (pp. 55-70).—Prof. Cayley closes the number with an instalment on the surfaces with plane or spherical curves of curvature (pp. 71-98). The paper is a reproduction in a compact form, with additional developments, of papers by Bonnet (*Journal de l'École Polyt.*, t. xx., 1853, pp. 117-306), and Serret (Liouville, t. xviii., 1853, pp. 113-162).

Engler's Jahrbücher, vol. viii. Part 5, contains:—Contributions to the knowledge of the Cupuliferæ, by K. Prantl. The author concludes that the segments of the cupule are not themselves leaves, but outgrowths of the axis covered with leaves, and that, with the exception of this peculiarity, the male and female catkins are similarly constructed. His views will be stated in Engler's "Die Naturliche Pflanzenfamilien," for which this paper was a preparatory study.—A revision of Bentham and Hooker's "Genera Plantarum," and "Floræ Columbiæ specimia selecta," by H. Karsten.—The rest of the number is taken up with abstracts of botanical papers, and the list of the more important works on classification and geographical botany published in the year 1886.

Vol. ix. contains the following articles:—On the roots of the Araceæ, by Max Lierau. An investigation of the roots of about 130 species from 46 genera of this natural order, leads the author to the result that those histological characters by which the stem and leaf of the several sub-orders of Engler are distinguished recur also in the roots, and thus these organs, though performing the most various physiological functions, have constant characters of systematic value.—In his contributions to the knowledge of the Capparidaceæ, Dr. Ferd. Pax discusses the questions of (1) the part taken by the axis in the construction of the flower; (2) the relation of the Capparidoideæ to the Cleomoideæ, in respect of the andrœcium. He concludes that the disk, androphore, and gynophore, are of axial nature, and not the result of coalescence of sporophylls; further, that the construction of the andrœcium is uniform throughout the order, being based upon the presence of two dimerous whorls, increased often very greatly by duplication.—Observations on the organization and biological conditions of northern trees, by F. W. C. Areschoug.—Specilegium canariense, by H. Christ.—Dr. Marloth gives an interesting account of the morphology, anatomy, and biology of the Naras (*Acanthosicyos horrida*, Welw.) of the south-west coast of Africa, and of observations of the peculiar property of the fruit in promoting the coagulation of milk.—On the flora of the German East-Asiatic Protectorate, by K. Schumann.—Contributions to the morphology and classification of the Ranunculaceæ, by K. Prantl. The author distinguishes "honey-leaves" (*Honigblätter*) from the perianth, defining them as "floral leaves, the chief function of which is the secretion of honey, and which have been produced from stamens independently of the differentiation of the perianth into calyx and corolla": thus he would describe the corolla of Ranunculus as consisting of such "honey-leaves," while the calyx would be regarded as a simple perianth. The greater part of the paper is occupied by the classification of the species within the genera.—New contributions to the flora of Greenland, by Eug. Warming.—Contributions to the knowledge of the walnut (*Juglans regia*, L.) by Dr. M. Kronfeld, with two plates.—A posthumous paper, by Dr. Hillebrand, descriptive of the vegetation of the Sandwich Islands.—Orchidaceæ herbarii Dom.-J Arechavatetæ det. et descr., by F. Kränzlin.—Dr. A. Breitfeld, in a paper on the anatomical structure of the leaves of the Rhododendroideæ, attempts to rank anatomical details with the characters of flower and fruit in the classification of the group, and finds the most useful characters in the epidermis.—On continuous and saltatory variation, by Franz Krašan.—Biographical notices on some of the collectors and authors named in the "Plantæ Ryddeanæ," by F. von Herder.—Marine Algæ of Puerto-Rico, by Dr. F. Hauck.—In addition to the above original treatises, the volume for the year contains a list of the papers of 1887 on the classification, description, and geological distribution of plants, as well as abstracts of the most important of these.

SOCIETIES AND ACADEMIES.

Sydney.

Linnean Society of New South Wales, July 25.—Dr. J. C. Cox, Vice-President, in the chair.—The following papers were read:—The insects of King's Sound and its vicinity, part 2, by William Macleay. This paper contains a list of all the Lamellicorn insects in the collection made by Mr. Froggatt in the West Kimberley district. Of the seventy-six species recorded, fifty-nine are described as new, but are all referable to known genera. The genera most numerous in species are *Onthophagus* and *Heteronyx*. The sub-family *Cetoniides* is represented by four species only.—Catalogue of the known Coleoptera of New Guinea, &c., part 2, by George Masters, Curator of the Macleay Museum. Part 2 of this catalogue, comprising the Tetramerous and Trimerous divisions, amounting to about 1100 species, completes the list of Coleoptera hitherto described from the region under consideration. The total number of species recorded is 2079.—Malaysian land and fresh-water Mollusca, by Rev. J. E. Tenison-Woods. After some introductory remarks on the extent and physical geography of the region under consideration, and on the characteristic features of its land and fresh-water Mollusca, the author gives a list of about 400 species indigenous to the Malay Peninsula in the States south of Keddah, and the Indian Archipelago, not including the Philippines and New Guinea. A bibliographical list is appended.—Mr. Ogilby exhibited a specimen of a deep-sea fish (*Chlorophthalmus nigripennis*), originally described by Dr. Günther in the *Ann. of Nat. Hist.*, 1878, and figured in vol. xxii. of the "*Challenger* Reports." The original specimens were taken by the *Challenger* naturalists off Twofold Bay, in 120 fathoms; the specimen exhibited was captured quite recently off Port Jackson in 70 fathoms, the only other occasion on which the species has been met with since its discovery.—Mr. Ogilby also exhibited a photograph of *Acanthias Blainvillii*, not hitherto recorded from New South Wales, and one of a variety of *Acanthoclinus littoreus*, originally described by Forster in "Cook's Voyage," the former having been taken in deep water off Port Jackson, the latter under stones between tide-marks at Lord Howe Island.—Mr. Brazier exhibited a spherical stone, about ½ inch in diameter, found in the crop of a Goura pigeon (*G. Albertisi*, Salvad.), from Hall Sound, New Guinea. Also a tube of fresh-water shells (*Segmentina australiensis*, E. A. Smith), from Waterloo Swamps.—Mr. MacDonald showed under the microscope an interesting exhibit of Rotifers (*Megalotrocha* sp.), living in clusters on pond weed.—Mr. Burnell exhibited two living slow-worms (*Typhlops nigrescens*), from Wentworthville, near Parramatta.—Mr. Deane exhibited a remarkable excrescence on a root of *Monotoca elliptica*, found by Mr. J. F. Fitzhardinge in the neighbourhood of Sydney; a specimen of an apodal lizard (*Delma impar*) found by Mr. C. F. Price, of Arable, near Cooma, where the species is said to be abundant in basaltic country; and examples of concretionary nodules occurring abundantly in a slaty rock in a cutting near Bredbo on the Goulburn to Cooma Railway.

Paris.

Academy of Sciences, October 1.—M. Des Cloizeaux in the chair.—Relative values of the two constituents of the force displayed in the stroke of a bird's wing, deduced from the direction and insertion of the fibres of the great pectoral muscle, by M. Marey. Of the forces in question, one, as shown in previous communications, equals the weight of the bird and enables it to resist gravitation, the other is horizontal and enables it to resist the air. From a study of the disposition of the muscular fibres of the breast, the author now infers that the latter force, contrary to the general opinion, is much greater, and may even be double that of the former.—Positions of Barnard's comet (September 2, 1888) measured at the Observatory of Besançon with the 0·22 m. equatorial, by M. Gruey. The observations cover the period from September 5-15.—Observations of Sawerthal's comet (1888, I.) made with the 0·38 m. equatorial at the Observatory of Bordeaux, by MM. G. Rayet and Courty. The observations range from April 4 to July 12.—Potential energy of the gravitation of a planet, by M. O. Callandreau. The object of this note is to show that the potential energy of a planet's gravitation—in other words, the power of attraction displayed in drawing the molecules from boundless space to their present position—may be approximately calculated if its dimensions, mass, and angular velocity of rotation be known, irrespective of the law of internal densities.—On actino-electric phenomena, by M. E. Bichat. The passage of electricity of high or feeble tension is known to be greatly facilitated when the electrified body is illumined by very refrangible radiations. In a previous communication it was shown that in Stoletow's experiment the substitution of a sheet of water for the metallic plate produces no deviation of the galvanometer, which seems to prove that the electricity is not transmitted by conduction. This inference is confirmed by the experiments here described. —On some new electric phenomena produced by radiations, by M. Auguste Righi. In continuation of previous researches, the author here reports a series of further results connected with the same order of phenomena.—On the employment of the sulphite of soda in photography, by M. Paul Poiré. The process here described has the advantage of avoiding the cloudiness produced by the prolonged action of the carbonate. Plates left forty-five minutes in the bath acquire a continual increase of intensity without presenting the least appearance of cloudiness. —On the land locomotion of reptiles and four-footed Batrachians compared with that of Mammalian quadrupeds, by M. G. Carlet. The locomotion of frogs, toads, lizards, and the like is described as a peculiar action, somewhat analogous to the trot of quadrupeds, and exactly like that of two men walking one behind the other with *contrary* step. It is a sort of slow trot, without any suspension of the body in the air.—M. Carlet communicates a supplementary paper in illustration of the same subject, on the locomotion of an insect rendered tetrapod by deprivation of the two middle legs. The experiment explains the persistence in all these organisms of the six legs, which appear to be not merely useful, but even necessary to secure stability and rapid locomotion.—A series of papers are contributed by MM. Philippe Thomas, P. Fliche, and Bleicher, on the petrified vegetation of Tunis. These fossils are shown to belong to the same Pliocene formation, and to be otherwise closely analogous to the well-known petrified forests in the neighbourhood of Cairo. Specimens of a like character have been picked up in Algeria and other parts of Mauritania, rendering it highly probable that the whole of North Africa, from the Mediterranean to the verge of the Sahara, was covered with a somewhat uniform vegetation in Pliocene times.

STOCKHOLM.

Royal Academy of Sciences, September 12.—Demonstration of a proposition, which touches upon the question of the stability of the planetary system, by Prof. Gyldén.—The same exhibited a calculating machine made by Herr Sörensen.—On a paper by Baron von Camerlander in Vienna, on the fall of meteoric dust in some parts of Austria in February this year, by Baron Nordenskiöld.—The same exhibited a new mineral from Pojsberg, which he had named Brandtit.—On crystals of native lead from Pojsberg, by Herr A. Hamberg.—On two new chlorides of indium, and on the density of the vapour of the chlorides of indium, gallium, iron, and chromium, by Profs. Nilsson and Pettersson.—On the theory of the numbers and functions of Bernoulli, based on a system of functional equations, by Dr. Berger.—On change of the sea-level at Altenfiord, by Commodore Littiehöök.—On some definite integrals, by Dr. C. F. Lindman.—Contributions to the theory of a singular solution of a partial differential equation with two independent variables, by Dr. J. Möller.—Observations on the condensation of the vapour of water in a humid, electrical atmosphere, by Herr G. A. Andrée.—On a species of Annelida living with hermit crabs, by Dr. Wirén.—On some derivates of α-β-dichlor-naphthaline, by Herr P. Hellström.—On the former occurrence of *Felis catus* in Scania, by Prof. Qvennerstedt.—On Dahllit, a new mineral from Bamle, in Norway, by Prof. W. C. Brögger and Herr H. Bäckström.—On the freezing-point of dilute aqueous solutions, by Dr. S. Arrhenius.—Galvanometric measurements on the influence that is exercised by an electric spark on another spark, by Dr. C. A. Mebius.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Untersuchungen zur Morphologie und Systematik der Vögel; I. Specieller Theil, II. Allgemeiner Theil: Max Fürbringer (T. Van Holkema, Amsterdam).—Fossils of the British Islands; Vol. i. Palæozoic: R. Etheridge (Clarendon Press).—A Class-book of Elementary Chemistry: W. W. Fisher (Clarendon Press).—General Report on the Operations of the Survey of India Department during 1886-87 (Calcutta).—Fourfold Root and Will in Nature: A. Schopenhauer (Bell).—University College, Liverpool, Calendar for the Session 1888-89 (Holden, Liverpool).—Papers and Proceedings of the Royal Society of Tasmania for 1887 (Tasmania).—Laboratory Manual of General Chemistry: R. P. Williams (Ginn, Boston).—An Introduction to Practical Inorganic Chemistry: W. Jago (Longmans).—Les Formes du Terrain, Texte et Planches: G. de la Noë and E. de Margerie (Paris).—The International Annual of Anthony's Photographic Bulletin (Greenwood).—A Catalogue of the Moths of India, Part 3: E. C. Cotes and C. Swinhoe (Calcutta).—Sixth Annual Report of the Fishery Board for Scotland, for the year 1887; Three Parts (Edinburgh).—Instruction in Photography; eighth edition: Captain W. de W. Abney (Piper and Carter).—The Metallurgy of Gold: M. Eissler (Lockwood).—Key to Lock's Arithmetic for Schools: Rev. R. G. Watson (Macmillan).—Report on the Eruption of Tarawera and Rotomahana, N.Z.: A. P. W. Thomas (Wellington, N.Z.).—Die Schwankungen der Hocharmenischen Seen Seit 1800: Dr. R. Sieger (Wien).—Bulletin du Comité International Permanent pour l'Exécution Photographique de la Carte du Ciel, 2e Fascicule (Gauthier-Villars, Paris).—Die Fossile Pflanzen-Gattung Tylodendron: H. Potonié (Berlin).—Ueber den Einfluss niederer Sauerstoffpressungen auf die Bewegungen des Protoplasmas: J. Clark (Berlin).—Der Feuerstoff: L. Mann (Berlin).—The Minerals of New York County, U.S.A. (New York).—Journal of the Chemical Society, October (Gurney and Jackson).—Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg, tome xxxii. Nos. 2 and 4.

CONTENTS.

THURSDAY, OCTOBER 18, 1888.

APPLICATIONS OF DYNAMICS TO PHYSICS AND CHEMISTRY.

Applications of Dynamics to Physics and Chemistry. By J. J. Thomson, M.A., F.R.S., Cavendish Professor of Experimental Physics, Cambridge. (London: Macmillan and Co., 1888.)

THIS is one of the most original books on mathematical physics which has appeared for a long time. Prof. J. J. Thomson has elaborated a method of very wide scope, and has applied it to a large number of problems of very different kinds. A reader of the work must perforce be struck not only with the mathematical ability of the author, but with the wide extent of learning which enables him to illustrate his theme by recent researches in nearly every branch of physics and physical chemistry.

The method employed is so essentially mathematical that it is not easy to describe it without the use of symbols. As, however, it is a matter of considerable importance that those who are studying by means of experiment the phenomena discussed by Prof. Thomson should have some idea as to the progress already made in their theoretical explanation, it may be well to give an account of the general principles which he has used.

In ordinary dynamics it is necessary to specify the positions of the members of a system of bodies of which the movements or mutual actions are under consideration. This is done by means of co-ordinates, which define their positions at a given time with respect to certain given lines or surfaces. If the system is in motion, the values of these quantities change with the time, and thus the co-ordinates may be regarded as possessing velocities.

The difference between the kinetic and potential energies of the system (which is called the Lagrangian function) can be expressed in terms of the co-ordinates and their velocities, and if this is done the magnitude of the force which is acting on the system and tending to increase the value of any particular co-ordinate can be deduced from it. If no such force is acting, it follows that a certain relation between the co-ordinates and their velocities must be satisfied.

This is a perfectly general dynamical method, which could be directly applied to the complex system of atoms and ether by which the physical phenomena displayed by any given body are produced, if it were not for difficulties which Prof. Thomson has attempted, as far as may be, to overcome.

In the first place, the dynamical method presupposes a knowledge of the relative positions of the members of the system, *i.e.* of its geometry, and we cannot at present express "such things as the distributions of electricity and magnetism, for example," in terms of the relative positions or movements of atoms and ether.

In the next place, even when we can express certain physical states in terms of quantities which completely describe all that we can observe, it is certain that in general they would not suffice to describe completely the state of the body if we had the power of noting every detail of its molecular structure.

Using theorems due to Thomson and Tait and to Larmor respectively, Prof. J. J. Thomson shows that the second difficulty may be overcome if the co-ordinates of the values of which we are ignorant do not enter into the expressions for the kinetic energy of the system. It then, however, becomes necessary to modify the Lagrangian function, but this new form is such that when it is expressed in terms of any variable quantities and their "velocities" they satisfy the mathematical condition to which a true geometrical co-ordinate and its velocity are subject. If L′ is the modified Lagrangian function, and q any one of a series of quantities, $q_1, q_2, \ldots$, in terms of which and of their velocities it can be expressed, then—

$$\frac{d}{dt} \cdot \frac{dL'}{d\dot{q}} - \frac{dL'}{dq} = 0.$$

The term co-ordinate is thus used by Prof. Thomson in the generalized sense of any quantity in terms of which and of its velocity the modified Lagrangian function can be expressed, and he assumes that as far as the phenomena under consideration are concerned the state of a body may be described by four different types of co-ordinates. These specify (1) the position in space of any bodies of finite size which may be in the system; (2) the strains in the system; (3) its electrical, and (4) its magnetic state.

The most general expression for the Lagrangian function is then formed. It may contain terms of various kinds. Prof. Thomson goes through them one by one, determines what the physical consequences of the existence of each would be, and if these are found to be contrary to experience concludes that the term in question does not exist.

Thus, for instance, it can be shown that if there were a term containing a product of the velocities of a geometrical and an electrical co-ordinate, an electrical "current would produce a mechanical force proportional to its square, so that the force would not be reversed if the direction of the current was reversed." As this and other similar deductions are all opposed to experience, no such term can exist.

A similar method is applied to the coefficients of the terms which are shown to be possible. Thus a term exists which contains the squares of the velocities of the geometrical co-ordinates. It corresponds to the expression for the ordinary kinetic energy. Prof. Thomson inquires whether the kinetic energy depends only on the geometrical co-ordinates, or whether it also varies with the electrical state of the various members of the system. The answer is given by means of an investigation of his own (*Phil. Mag.*, April 1881), in which he has shown that the kinetic energy of a small sphere, of mass m, and radius a, charged with e units of electricity, and moving with a velocity v, is—

$$\left\{\frac{1}{2} m + \frac{2}{15} \frac{\mu e^2}{a}\right\} v^2,$$

where μ is the magnetic permeability of the dielectric surrounding it. The effect of the electrification is therefore the same as if the mass had received an increment, which, however, numerical calculation shows is too small to be observed. It is, nevertheless, important that the mutual relationship between ordinary kinetic energy and electrification should be recognized, as it follows that the speed with which electrical oscillations are propagated across any medium will be diminished by the presence of conductors moving about in it. "Thus, if the electromagnetic theory of light is true, the result we have been discussing has an important bearing on the effect of the molecules of matter on the rate of propagation of light."

It would take too long to follow the author in detail through the interesting discussion which is pursued on these lines. Another example must therefore suffice. The specific inductive capacity of a dielectric depends upon the strain, and it follows that the distribution of stress which Maxwell supposes to exist in the electric field is supplemented by another, which is due to the relation between inductive capacity and strain. Maxwell's distribution will be the same for all dielectrics, but Quincke has shown that though most dielectrics expand when placed in an electric field, the fatty oils contract. In these cases the effects of what may be called the supplementary distribution are contrary to, and greater than, those produced by Maxwell's stress.

Phenomena which depend on temperature are specially discussed, and an interesting conclusion with respect to thermo-electricity may be noted. It is that from the heat developed by a current at a junction of two dissimilar metals we can derive information as to that part only of the electromotive force which depends upon the temperature. Hence the Peltier effect can throw no light upon the absolute difference of potential between two different metals.

A chapter is devoted to the calculation of "the Lagrangian function in the simplest case, when the body is in a steady state, when it is free from all strain except that inseparable from the body at the temperature we are considering, and when it is neither electrified nor magnetized." Two forms are found, which hold for the gaseous and the liquid or solid states respectively. The general principle is also laid down, that, "when the physical environment of a system is slightly changed, and the consequent change in the mean Lagrangian function increases as any physical process goes on, then this process will have to go on further in the changed system before equilibrium is reached than in the unchanged one, while if the change in the mean Lagrangian function diminishes as the process goes on it will not have to proceed so far."

As an example of this we may take the effect of a charge of electricity on the vapour-pressure of a liquid. If a spherical drop, of radius a, surrounded by a medium of specific inductive capacity K, is charged with e units of electricity, its potential energy is increased by $e^2/2Ka$, and thus electrification changes the mean Lagrangian function by the amount $-e^2/2Ka$. Prof. Thomson quotes experiments by Blake to prove that when an electrified liquid evaporates the vapour is not electrified, so that the charge e is unaffected by evaporation, while the radius a of course diminishes. On the whole, then, evaporation algebraically diminishes the term $-e^2/2Ka$, and therefore it will not proceed so far as before the liquid was electrified. Thus electrification diminishes the vapour-tension by an amount which is limited by the insulating power of the air. The maximum effect is about equal in magnitude, though opposite in sign, to that due to a curvature of a quarter of a centimetre. The suggestion is made that we should therefore expect an electrified drop of rain to be larger than an unelectrified one, so that this effect may help to produce the large drops of rain which fall in thunderstorms. The principle also leads to the conclusion that the density of saturated aqueous vapour in the presence of air is greater than if no other gas is present, and thus, apart from other causes, rain-drops would form more easily when the barometer is falling than when it is rising.

The properties of dilute solutions are discussed at length, and the Lagrangian function is calculated in accordance with the views of Van 't Hoff on the assumption that the molecules of a salt in a dilute solution behave as though they were in the gaseous state.

The results obtained cannot be considered favourable to the view that the effects of solution are capable of being stated in such simple terms. Röntgen and Schneider's experiments on the compressibility of saline solutions prove that the decrease in the compressibility is sometimes more than a hundred times greater than that calculated on the above assumptions. The author also points out that the rise in the osmometer, which is explained as due to the pressure of the dissolved salt, may be capable of other interpretations, and that at present the indications of the instrument must be considered ambiguous.

Enough has perhaps been said to give an idea of the method and scope of Prof. J. J. Thomson's work.

It is possible that some of the experimental results which are quoted require fuller confirmation than they have as yet received, but if the work is regarded as a text-book of mathematical physics this is a very minor defect. The author has developed a method of wide scope, and it is important that its applications should be fully illustrated, even if the data assumed are not in all cases unexceptionable.

The book literally bristles with novel suggestions and points of interest. An explanation of the fact recently discovered by Mr. Shelford Bidwell, that iron becomes shorter when the magnetizing force is very great; the effect of surface-tension on electromotive force; chemical action in thin films; the effect of a neutral gas on dissociation—these are some of the subjects, in addition to those which have already been mentioned, upon which we light on turning over the pages haphazard.

That it will make the study of physics and chemistry easier is only in one sense true. *Nihil tetigit quod non ornavit* may, as applied to Prof. J. J. Thomson, be freely translated, that he hardly mentions any law of physics except to complicate it with correction terms.

From a more serious point of view, however, it is difficult to over-estimate the value of the establishment of the less obvious connections between phenomena.

On many points, such as Quincke's and Bidwell's observations on the changes of magnitude produced in the electric and magnetic fields respectively, experiment needed the support of theory, and Prof. J. J. Thomson points out causes to which the observed effects may be due. Almost daily, conscientious experimentalists are spending time and ability in the detailed examination of facts which they cannot explain, and which they can only hope to explain by the most minute investigations. In the cases just mentioned the labour was well spent, but in others it is practically thrown away in the attempt to pierce a labyrinth the clue to which can be found only by mathematics. Prof. J. J. Thomson's book ought to be carefully studied by all physicists, and especially investigators who have discovered what they believe to be a new fact. In many cases it will suggest possible explanations which may prevent long and wearisome groping in the dark.

The author is to be warmly congratulated on his work, which is an achievement of a high order, and which will add to his already great reputation as a mathematical physicist.

RECENT WORKS ON ORNITHOLOGY.

Argentine Ornithology. By P. L. Sclater, M.A., F.R.S., &c., and W. H. Hudson, C.M.Z.S. Vol. I., pp. i.–xvi., 1–208, pls. i.–x. (London: R. H. Porter, 1888.)

British Birds: Key List. By Lieut.-Colonel L. Howard Irby. Pp. 1–58. (London: R. H. Porter, 1888.)

Birdsnesting and Bird-skinning: A Complete Description of the Nests and Eggs of Birds which breed in Britain. By Edward Newman. Second Edition. Revised and re-written, with directions for their collection and preservation; and a chapter on Bird-skinning, &c. By Miller Christy. Pp. i.–xii., 1–138. (London: T. Fisher Unwin, 1888.)

DR. SCLATER AND MR. HUDSON have combined their forces to produce one of the best books ever written on South American ornithology. Each is a master of his own portion of the subject, for no one is better acquainted with neotropical ornithology than Dr. Sclater, and Mr. Hudson has been known for many years as one of the best living observers of the habits of birds in the field. The scheme of the book, therefore, leaves nothing to be desired, and the whole of the "get-up," as regards paper, print, and illustrations (the latter a matter of course when Mr. Keulemans is the artist), is about as good as it is possible to be, and reflects the greatest credit on the publisher.

One of the most interesting features of the work will doubtless be the introduction, which will appear in the second volume, when it will be possible to form some accurate notion of the relations of the avifauna of the Argentine Republic with that of the neighbouring States, a comparison which will doubtless be of importance to all naturalists who are interested in the somewhat complicated natural areas of the neotropical region. At present the genera and species peculiar to the region treated of by the authors seem to be few in number, and they would appear to be limited to the more western portions of the country, especially the district of Tucuman.

It would be easy to give many extracts from Mr. Hudson's charming writings on the habits of the birds, with the life-history of many of which he is as familiar as we are in England with that of many of our British birds, while his travels have enabled him in many instances to give an account of species both in their summer and winter homes. To any naturalist visiting Argentina this book will be of the highest value, the descriptions given by Dr. Sclater being short and concise, but sufficient for the identification of species, while he is to be congratulated also on the success with which he has contrived to attach an English name to each bird. Everyone who has tried to do this, when writing on exotic birds, knows how difficult it is to invent English titles for species which have no counterpart in European nomenclature; and we must acknowledge that the names are a great improvement on some of the zoology "as she is taught" at our Zoological Gardens. Should some of the names bestowed upon animals in the "Zoo" ever be adopted in general works of travel, we might expect to find such truthful anecdotes as the following:—

"The insolent behaviour of one of the animals considerably annoyed us, from its persistent habit of making 'long noses' at us. On shooting a specimen we discovered that it was a Rude Fox (*Canis rudis*)," &c., &c.

"Some interesting little creatures now came in sight, dancing, apparently in perfect time, across the glade. They proved to be Pleasant Antelopes (*Tragelaphus gratus*)," &c., &c.

"Just as I was emerging from a thicket I managed to trip over something which brought me heavily to the ground. I fancied that I had fallen over a tree-stump, but on careful examination, it proved to be an Inconvenient Curassow (*Crax incommoda*) which had somehow got in my way," &c., &c.

In his useful little work, a "Key List to British Birds," Colonel Irby has supplied a real want—a handy pocket-book, giving just the diagnostic characters of every species. It is a desirable supplement to the "List of British Birds" published by the British Ornithologists' Union, which dealt with the nomenclature of the various species, but which might also with advantage have contained diagnoses, such as Colonel Irby's industry has now supplied.

What Colonel Irby has done for the birds, Mr. Miller Christy does for the eggs of British birds, and it is certain that with this little work in his hands the young student can gain a very good idea of the eggs which are likely to be met with in England. The call for this second edition of the late Mr. Newman's work shows apparently that there are a good many egg-collectors in this country, notwithstanding the prohibitions of an Act of Parliament; nor can we state with truth that there is any falling off in the number of students of the egg-collection in the British Museum since the Wild Birds Preservation Act became law. To the chapter on bird-skinning we would add a practical hint that before commencing operations a tiny wisp of wool should be inserted into the palate of the specimen. This greatly prevents the risk of discharge from the nostrils, and saves many a skin from being draggled and spoilt. The American method of enveloping the prepared skin in wadding is also far preferable to our method of fastening a paper band round the specimen.

R. BOWDLER SHARPE.

OUR BOOK SHELF.

Mechanics. By Edward Aveling, D.Sc. (London: Chapman and Hall, Limited, 1888).

THIS is the first of four treatises on mechanics and experimental science, published to meet the requirements of candidates in the matriculation examination of London University. The volume before us contains a great number of numerical examples and exercises for students, and twenty pages are devoted to specimen examination papers of various kinds. The author's language is very inexact if compared with the language of Thomson and Tait's "Natural Philosophy," or Dr. Lodge's text-book. It reads as if a shorthand-writer had taken notes of lectures, and the lecturer had published them after hasty correction. This inexactness is visible in almost every definition in the book. We read of velocities acting and accelerations working. New magnitudes are introduced; thus, "the intensity of a force is like the temperature of a body. It is measured by the velocity communicated, apart altogether from the mass to which it is communicated." "But the quantity of a force is like the amount of heat in a body. Force-quantity is measured by the product of the velocity communicated *and* the mass to which it is communicated" (p. 103). In defining, if he can be said to define, "impressed force," the author uses expressions such as "so that when we speak or read of an accelerating force, f or g, or 9·8 or 32·2, or a per second per second."

This book would certainly not be recommended by us to any student who is desirous of obtaining a knowledge of mechanics; but, for all we know, it may very well serve the purpose for which its author has designed it. It is a book written for candidates in certain examinations by a successful candidate. The author has introduced side lines to catch a student's eye, and we think this a very clever contrivance. Thus there is the side line "Pressure" (p. 2), and the student is directed to get off by memory: "When a body is prevented from falling towards the earth by the hand or by a table, *e.g.*, the body exerts a certain pressure upon the hand or the table." It is interesting to know from such an authority as Dr. Aveling that this is the sort of definition which satisfies an examiner, and it seems to us that a study of this book by examiners would lead to very useful results.

Solutions of the Examples in an Elementary Treatise on Conic Sections. By Charles Smith, M.A. (London: Macmillan, 1888.)

MR. SMITH has been well advised in drawing up this collection of elegant solutions to the examples in his "Conics." His treatise is just now in the full tide of success, and seems likely to maintain its position for some time yet before a better one drives it into the background. This, then, is just the time when such aid as is here furnished is most acceptable to teachers, "many of whom," as we have more than once stated in these columns, and as the author here testifies, "can ill afford time to write out detailed solutions of the questions which prove too difficult for their pupils." We have compared many of the solutions here given with our own (in manuscript), and find that new light is thrown on some by Mr. Smith's thorough command of the latest methods. We have detected here and there a trifling error, which may perhaps cause momentary trouble to a self-taught student, but there is sufficient detail given to enable the reader, on careful perusal, to make the required correction. In some cases more than one solution is given: this is a good feature. The possessor of the text-book and of the "Solutions" occupies a strong position, and should be able to attain considerable skill in this particular branch of mathematics.

The Beginner's Guide to Photography. By a Fellow of the Chemical Society. (London: Perken, Son, and Rayment, 1888.

THIS is a second edition, revised and enlarged, of an elementary guide for those commencing the art of photography. In it will be found practical hints as regards the choice of apparatus, and a good explanation of the whole process of photographic manipulation, written in a manner which for beginners leaves nothing to be desired.

An article on "Exposure" has been added by Mr. H. S. Platts, including tables and directions, and the latter, if carried out by the amateur, ought to give him good results.

There are, also, chapters on the production of lantern-slides, enlarging, and photomicrography, and the book concludes with a collection of the illustrations referred to in it.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Prophetic Germs.

IN his letter of October 8, the Duke of Argyll says that he sees great value in my statement (which he improperly terms an "admission"), that natural selection cannot act upon any structure which is not already developed up to the stage of actual use. He says, "This is really all I want for my previous argument, because all organs whatever do actually pass through rudimentary stages in which actual use is impossible." Here we have the Duke's case in a nutshell. It is easily dealt with. Firstly, what the Duke terms an "admission" on my part is an essential and explicitly stated element of Mr. Darwin's own exposition of his theory. Secondly, it is necessary for the Duke to demonstrate not that "all organs whatever," but that *some* organs "do actually pass through rudimentary stages in which actual use is impossible."

The stages here alluded to are—if I understand correctly—ancestral stages, not stages in the embryological development of the individual.

I feel bound to state that I do not know of any facts in the history of either animals or plants which lead me (or, I may say, which have led any important number of the vast army of writers and observers on these subjects) to the conclusion that any existing active organ has passed through rudimentary stages in which actual use is impossible, if we set aside such cases as may be explained by correlation of growth or by the persistence of vestiges of formerly useful structures.

If the Duke of Argyll can show that any one organ has or "must have" passed through such useless stages (not explicable as due to correlation of growth nor as inherited vestiges), he ought at once to do so. Mr. Darwin, in his severe testing of his own theory, tried to find such cases, and did not find them. What are they? My own opinion is that they do not exist, and that the Duke's case collapses. E. RAY LANKESTER.

A New Australian Mammal.

A FEW days ago, through the kindness of Mr. A. Molineux, of Adelaide, a small mole-like animal, which appears to be new to science, was forwarded to the South Australian Museum. It was found on the Idracowie cattle-station, at a distance, I understand, of about 100 miles from the Charlotte Waters Telegraph Station, on the overland line from Adelaide to Port Darwin; but the exact circumstances of its capture are not yet to hand. The collector, however, reports that it must be of rare occurrence, as, on questioning the aboriginals of the locality, there was only one old woman who said she had seen it before, and that upon a single occasion.

It is evidently an underground burrowing animal resembling somewhat the Cape mole (Chrysochloris) in its general external appearance, but differing in many respects.

The total length is 13 cm., inclusive of the tail, which is 2 cm. long. The head, relatively shorter than Chrysochloris, has a rounded muzzle, the dorsal surface of which is covered by a horny shield. Nostrils transversely slit-like. No eyes visible, the skin passing uninterruptedly over the ocular region; but on reflecting the skin on one side of the face a small circular pigment spot is visible in the position of the eye. No apparent bony orbit. Tongue fleshy, broad at the base, and tapering to a blunt point. No external ears; but the ear-openings distinct, 1 mm. wide, and covered over with fur.

The fore-limbs are short, resembling somewhat those of a mole; but the manus is folded, so that the large nails of the fourth and fifth digits only are visible in the natural position of the limbs. Of these nails the fourth is 15 mm. long and of a uniform width of 4 mm., ending very bluntly; the fifth is very slightly shorter than the fourth, broad at the base (8 mm.), tapering rapidly to a blunt point, the two together forming an outline rather like that of a goose-mussel (Lepas). The nails of the third, second, and first digits, very much smaller, form a series gradually diminishing in size in the order named, and constitute a second row on the inside of the fourth and fifth, by which, as stated, they are completely concealed from view. What corresponds to the palm is the cleft between the two rows of digits.

The hind-limbs are also short, with the soles turned outwards. What appears to be the fifth (anterior) digit is very short, with a short, broad, and strong nail; the fourth is armed with a long (7 mm.) narrow, curved, and sharp claw, while the claws of the third, second, and first are broad, flat, rounded at their points, and joined together by a membrane which extends nearly to their points. On the sole there is a hard, elongated, horny tubercle crossing it transversely.

The tail, 2 cm. long, and 5 mm. wide at the insertion, tapers to 3mm., and terminates in a knob-like tip.

About 15 mm. in front of the vent (? cloaca) there is a pouch in the integument about 4 mm. wide, with the opening directed backwards and having a depth in a forward direction of from 4–5 mm. The surface of this pouch is devoid of hair, but the bare area is surrounded by thick fawn-coloured fur, with a slightly reddish tint; it is possible, however, that this reddish tint is due wholly or in part to some ferruginous-looking sand which is much mixed up with the fur. The body generally, with the exception of the lower two-thirds of the tail, which is bare, is covered with fur of a rather lighter tint.

With regard to the internal parts, it is unfortunate that the specimen came to us completely eviscerated, and in a bad state of preservation generally; but in a small part of the lower bowel which was left, remains of ants were found. The bowel terminates at a wide vent (? cloaca), and I can find no trace of a separate genital aperture, nor of such openings into the supposed cloaca.

I have hot yet had time to examine with minuteness the skeleton, which unfortunately is also considerably damaged, especially about the occipital region; but from a cursory examination of the recently-skinned body, I can note the following points, with, I believe, accuracy:—

Cranium relatively large; no bony orbits; zygomatic arches present; well-developed shoulder-girdles with slender clavicles; pectoral muscles large; pelvis large and strong, with a rather wide symphysis pubis, but no epipubic bones, either actual or rudimentary; ribs, 14; angle of lower jaw markedly inflected.

The teeth are peculiar, and require a more extended description than I can give at present, but the formula appears to be—

$$i\,\frac{3}{3},\ c\,\frac{1}{1},\ m\,\frac{6}{5}\left(?\ \frac{p\,2,\ m\,4}{p\,1,\ m\,4}\right).$$

This, however, may require some modification, as just posterior and external to the premolar (or first molar) of the right ramus of the mandible there is a small rudimentary conical tooth, which is not to be found on the opposite side, nor at corresponding positions in the maxilla.

I do not pretend to be a zoological expert, but I cannot help being struck with the resemblance both of the lower jaw and of the general characters of the teeth to the pictures of the jaws of Amphitherium as figured in various osteological works. I am now endeavouring to obtain other specimens, and meanwhile am having careful drawings made of the various parts of the present example of what appears to be a remarkably curious and interesting animal even in this land of strange and antique types.

E. C. STIRLING.

The University, Adelaide, South Australia, August 29.

Nomenclature of Determinants.

NATURE of October 4 opens with a review of a book on "Determinants" by two pupils of Prof. Valentin Balbin, whose energy and enthusiasm have done so much for mathematics in the University of Buenos Ayres. In regard to the naming of the various special forms of determinants, your reviewer says:—"The nomenclature adopted in the second book differs in some particulars from that employed by Muir. Thus, our authors do not follow him in substituting 'adjugate' for the more euphonious and more familiar adjective 'reciprocal,' and they agree with Scott and others in calling those determinants 'orthosymmetrical' which Muir names 'persymmetric.' We think that their name 'determinante *hemisimétrica*' is a distinct improvement on the old '*zero-axial skew* determinant,' but . . ."

Now, as I have gone on a definite principle in the selection of the technical terms used in my book, and as I believe that this principle is one which receives very general approval among students of science, I shall be glad if you will allow me to direct attention to it. It is that, unless very strong reasons to the contrary can be adduced, the first name given to a scientific object or concept should not be departed from.[1] In more aphoristic form, the multiplication of synonyms is a great evil. Judged by this principle, the terms "adjugate," "persymmetric," and "skew" deserve the place I have given them. "Adjugate," as applied to a determinant, was a generation old before "reciprocal" was proposed; and—what is no mean additional recommendation—it carries with it the sanction of the highly-honoured names of Gauss and Cauchy. To outweigh these claims there is very little to be said for the rival word. It is not more appropriate; indeed, the kind of connection to be indicated does not involve the idea of reciprocity at all. It is true, as your reviewer says, that "reciprocal" is a more familiar word; but the use of a familiar word in a new and therefore unfamiliar sense is surely not to be commended. In regard to "persymmetric," similar language may be employed. It was proposed by Sylvester, and was in use for years by him and others before "orthosymmetric" made its appearance. The latter is not an etymological mongrel, but it is also not one whit more appropriate than the word it seeks to supplant, and it is the unfortunate parent of the monster "doppelt-orthosymmetrisch." It never was heard in England until 1880, and I regret that my friend Mr. Scott should have seen cause to introduce it. As for the third word, "skew," the arguments in its favour are still stronger. The determinant in question was called "skew" in English and "gauche" in French, by Cayley, as far back as 1846; and these words, and their German and Italian equivalents, are to be found employed in scores of original memoirs by the highest mathematicians. "Hemisymmetric" is but of yesterday, and, so far as I know, has never been used by any mathematician of note.

Is it merely a proof of the decadence of our insularity to find a welcome given by Englishmen to terms of foreign coinage which have been wantonly proposed to displace the original words of Cayley and Sylvester? and what does it prove to find Germans, who at first derided the tropical luxuriance of Sylvester's nomenclature, now out-Heroding Herod without having Sylvester's exculpating accompaniment of tropical luxuriance of *ideas?*

Your reviewer's protest against Dostor's introduction of "multiple determinants" I cordially support, and only wish that he had taken space to show the numerous absurdities connected therewith.

THOMAS MUIR.

Beechcroft, Bothwell, N.B.

A Shadow and a Halo.

"E. W. P." may see the phenomenon he describes any sunshiny morning or bright moonlight night, when the dew is heavy on the grass. The halo being caused by reflection at a small angle of the sun or moonlight from the wet surfaces of the blades of grass, enhanced by contrast with the dark shadow (and having nothing to do with moist air), its brightness would no doubt be increased by the foreshortening and consequent apparent compression of the reflecting surfaces on the slope. The neighbourhood of a high hedge would diminish it by lessening radiation, and the consequent cooling of the grass and deposition of dew upon it.

NATURE naturally takes no account of moral analogies, of which Nature herself is full. Else one might note that a man never sees a halo round his own head unless he turns his back to the light.

B. W. S.

Hampstead Heath, October 6.

OFTEN and often in walking or riding over the chalk downs of Wiltshire or Hertfordshire I have observed a bright halo surrounding the shadow of my head. This is usually cast by sun

[1] The introduction of "continuant" may seem to do violence to this principle; but the letter referred to by your reviewer will show the opposite.

or moon in bright clear weather, and extends with a radius of about three times the shadow's diameter around the head alone. It is probably due to diffraction of light-waves, an explanation of which at length may be read in Glazebrook's "Optics" and in other text-books. But your correspondent omits the most extraordinary character of the phenomenon. It is a curious fact that any man can see the light around the shadow of his own head, but *never* about the shadow of another. Few people notice this halo, but when once pointed out to them, they tell me they frequently observe it. It is particularly clear when thrown across a valley from one ridge to another on the opposite side. I have puzzled over this spectral brightness for five years, and never found an explanation of the fact that no one can see anyone's halo but his own. I have delayed writing to NATURE until cause and effect could both be given, but they are not forthcoming.

Another curious appearance is a rainbow thrown by sunlight on black sound ice, probably due to polarization by crystals. On the one occasion when I saw it on a pond, I had no time to observe details. Has anyone seen the like? A. S. EVE.

Marlborough College.

Nesting Habit of the House Sparrow.

I SHOULD be glad to know if any of your correspondents have noticed a nesting habit of the house sparrow (*Passer domesticus*) which I have very frequently observed in this part of New Zealand. In many of the deep cuttings in our roads and in the cliffs upon our river-banks, where the formation is a light pumiceous sand, these birds are in the habit of burrowing holes similar to those of the sand-martin (*Hirundo riparia*). In some cases I have found these burrows by measurement to be as much as 6 feet in depth.

Can this be a recently acquired habit, and will it not have an influence upon the anatomical development of the bird?

Waihou, Auckland, N.Z., September 5. G. L. GRANT.

Sonorous Sands.

I NOTICE a letter from my friend Mr. A. R. Hunt in your issue of last week, and add a line to say that the sand which our common friend, the late Admiral Bedford, gave him was, *probably*, of my collecting.

I found that the sand in Studland Bay is sonorous, during a visit to Swanage, in 1869, and was, for many years, in such constant communication with the late Admiral Bedford, exchanging notes and specimens, that I think I must have given him the sonorous sand in question, though I cannot remember the circumstance.

Anyway, there is no doubt that the dry sands of Studland Bay are powerfully sonorous. Walking with my son and a young friend of his across the bay in July 1869, we all amused ourselves by kicking the musical dust before us, the two younger pairs of heels getting quite a volume of sound out of the performance. D. PIDGEON.

Holmwood, Putney Hill, October 6.

A Shell Collector's Difficulty.

IF Mr. Layard will discard "tightly-corked tubes" altogether, and keep his minute shells in open-ended sections of glass tube, lightly closed, at top and bottom, with cotton-wool, he will have no more trouble from "milky efflorescence," which will not form in presence of the "thorough draft" he will thus establish in his cabinet. D. PIDGEON.

Holmwood, Putney Hill, October 13.

Yorkshire Geological and Polytechnic Society.

IN accordance with a request made by the Council of the Yorkshire Geological and Polytechnic Society, I am compiling a history of the past fifty years' work of the Society, and including in it biographical notices of some of its principal members. Amongst the latter was the Rev. W. Thorp, who for several years held the office of Honorary Secretary, and took great interest in the Society. He was at one time vicar of Womersley, and afterwards removed to Misson. Unfortunately I can obtain no records of his life. Can any of your readers assist me? Any information will be gratefully received and duly acknowledged. I believe Mr. Thorp died about 1857.

Chevinedge, Halifax, October 15. JAMES W. DAVIS.

MODERN VIEWS OF ELECTRICITY.[1]

PART IV.—RADIATION.

XI.

WE have next to consider what happens when electrical waves encounter an obstacle.

Mechanism of Electric Radiation.

In forming a mental image of an electrical wave, we have to note that three distinct directions are involved. There is (1) the direction of propagation—the line of advance of the waves; (2) the direction of the electric displacements, at right angles to this; and (3) the direction of the magnetic axis, at right angles to each of the other two.

One may get a rough mechanical idea of the process of electrical radiation (at any rate in a plane) by means of the cog-wheel system already used in Part III. Imagine a series of elastic wheels, in one plane, all geared together, and let one of them be made to twist to and fro on its axis; from it, as centre, the disturbance will spread out in all directions, each wheel being made to oscillate similarly and to transmit its oscillation to the next. Looking at what is happening at a distance from the source, we shall see the pulses travelling from left to right; the electrical displacement, such as it is, being up and down; and the oscillating axes of the wheels being to and fro, or at right angles to the plane containing the wheels. The electric displacement is small, because the positive and negative wheels gearing into one another move almost equally, and accordingly there is the merest temporary balance of one above the other, due to the elastic "give" of the wheels. The magnetic oscillations, on the other hand, are all in one sense, the positive wheels rotating one way and the negative the other: all act together, and so the magnetic oscillation is a more conspicuous fact than the electric oscillation. Hence it is often spoken of as electro-magnetic radiation rather than as electric radiation. But the energy of the electrostatic strain is just as great as that of the electro-magnetic motion; in fact the energy alternates from the potential to the kinetic form, or *vice versâ*, at every quarter swing, just like every other case of vibration.

Prof. Fitzgerald, of Dublin, has devised a model of the ether, which by help of a little artificiality represents the two kinds of displacement—the electric and magnetic—very simply and clearly.

His wheels are separated from one another by a certain space, and are geared together by elastic bands. They thus turn all in one direction, and no mention need be made of positive and negative electricity as separate entities.

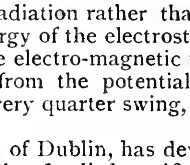


FIG. 48.—Fitzgerald's Ether Model. A set of brass wheels connected by common elastic bands. If the bands are taken off any region, it becomes a perfect conductor, into which disturbances cannot penetrate.

But, the wheels being massive, a rotatory disturbance given to one takes time to spread through the series, at a pace depending on the elasticity of the bands and the inertia of the wheels; and during the period of acceleration one side of every elastic is stretched, while the other side is relaxed and therefore thickened. This thickening of the elastics goes on in one direction, and corresponds to an electric displacement in that direction; the direction being perpendicular both to the direction of advance of the disturbance and to the axes of the wheels. A row of wheels corresponds to a section of a wave-front; the

[1] Continued from p. 419.

displacements of india-rubber and the rotating axes, *i.e.* the electric and the magnetic disturbances, both lie in the wave-front.

Clerk Maxwell's originally suggested representation was not unlike this.[1] It consisted of a series of massive wheels, connected together not by a series of elastic bands but by a row of elastic particles or "idle wheels." These particles represented electricity; their displacement during the period of acceleration corresponding to the one-sided thickening of the elastic bands in Fitzgerald's model.

I have proposed to contemplate a double series of wheels geared directly into one another, and representing positive and negative electricity respectively, because it seems to me that so many facts point to the existence of these two entities, and because then no distinction has to be drawn between one part of the medium which is ether, and another part which is electricity, but the whole is ether and the whole is also electricity; while, nevertheless, a much-needed distinction can be drawn between a motion of the ether as a whole, and a relative motion of its component parts—a distinction between forces able to move ether, *i.e.* to displace the centre of gravity of some finite portion of it, and forces which shear it and make its components slide past each other in opposite senses: these latter forces being truly electromotive.

If it be asked how the elasticity of the ether is to be explained, we must turn to the vortex sponge theory, suggested by Mr. Hicks[2] (principal of Firth College, Sheffield), and recently elaborated by Sir William Thomson.[3] But this is too complicated a matter to be suited for popular exposition just at present. It must suffice to indicate that the points here left unexplained are not necessarily at the present time unexplainable, but that the explanations have not yet been so completely worked out that an easy grasp can be obtained of them by simple mechanical illustrations and conceptions. At the same time, the general way in which motion is able to simulate the effects of elasticity will be found popularly illustrated in Sir William Thomson's article "Elasticity" in the "Encyclopædia Britannica"; and the fact that elastic rigidity of a solid can be produced by impressing motion on a homogeneous and otherwise structureless fluid must be regarded as one of the most striking among his many vital discoveries.

We have seen that to generate radiation an electrical oscillation is necessary and sufficient, and we have attended mainly to one kind of electric oscillation, viz. that which occurs in a condenser circuit when the distribution of its electricity is suddenly altered—as, for instance, by a discharge. But the condenser circuit need not be thrown into an obviously Leyden-jar form; one may have a charged cylinder with a static charge accumulated mainly at one end, and then suddenly released. The recoil of the charge is a true current, though a weak one; a certain amount of inertia is associated with it, and accordingly oscillations will go on, the charge surging from end to end of the cylinder like the water in a tilted bath suddenly levelled.

In a spherical or any other conductor, the like electric oscillations may go on; and the theory of these oscillations has been treated with great mathematical power both by Mr. Niven and by Prof. Lamb.[4]

Essentially, however, the phenomenon is not distinct from a Leyden jar or condenser circuit, for the ends of the cylinder have a certain capacity, and the cylinder has a certain self-induction; the difficulty of the problem may be said to consist in finding the values of these things for the given case. The period of an oscillation may still be written $2\pi\sqrt{(LS)}$; only, since L and S are both very small, the "frequency" of vibration is likely to be excessive. And when we come to the oscillation of an atomic charge the frequency may easily surpass the rate of vibration which can affect the eye. The damping out of such vibrations, if left to themselves, will be also a very rapid process, because the initial energy is but small.

But whether the charge oscillates in a stationary conductor, or whether a charged body vibrates as a whole, it equally constitutes an alternating current, and can equally well be treated as a source of radiation. Now, when we were considering the subject of electrolysis we were led to think of molecules as composed of two atoms or groups of atoms, each charged with equal quantities of opposite kinds of electricity. Under the influence of heat the components of the molecules are set in vibration like the prongs of a tuning-fork, the rate of vibration depending on and being characteristic of the constants of the particular molecule. The atoms being charged, however, their mechanical oscillation is necessarily accompanied by an electric oscillation, and so an electric radiation is excited and propagated outwards. These vibrations would appear to be often of the frequency suited to our retina, hence these vibrating atoms indirectly constitute our usual source of light. The "frequency" of the visible radiation can be examined and determined by optical means (some form of interference experiment, usually a diffraction grating), and hence many of the rates of vibration possible to the atoms of a given molecule under given circumstances become known, and this is the foundation of the science of spectroscopy.

It is possible that the long duration of some kinds of phosphorescence may be due to the atoms receiving indirectly some of the ethereal disturbance, and so prolonging it by their inertia, instead of leaving it to the far less inertia of the ether alone. It is possible also that the definite emissivity of some fluorescent substances is due to periods of vibration proper to their atoms, which, being disturbed in an indirect way by receipt of radiation, re-emit the same radiation in a modified, and, as it were, laden manner.

To get some further idea concerning the way in which an oscillating charge or an oscillating charged body can propagate radiation, refer back to Fig. 39, Part III. (NATURE, vol. xxxvii. p. 346), and imagine the rack oscillating to and fro. It will produce rotatory oscillation in the wheels gearing into it, these again in the next, and so on. If the wheel-work were rigid, the propagation would go on at infinite speed to the most distant wheels, but if it be elastic then the pace of propagation depends on the elasticity and the density in a way we have already said enough about. The line of rack is the direction of electric oscillation, the axes of the wheels the direction of magnetic or rotatory oscillation, and at right angles to both these is the direction of advance of the waves. True, the diagram is not a space representation, it is a mere section, and a very crude suggestion of a mechanical analogy to what may be taking place.

The wheels being perfectly geared together and into the rack represent an insulator or dielectric: there is no slip or frictional dissipation of energy—in other words, there are no true electric currents. The electric oscillation is a mere displacement oscillation due to elasticity and temporary give of the elastic wheels, whereby during each era of acceleration they are thrown slightly into the state represented in Fig. 46 (vol. xxxvii. p. 367) as contrasted with Fig. 37 (*ibid.* p. 345).

Effects of encountering a New Medium.

Now contemplate an advancing system of waves, and picture their encounter with an obstacle; say, a medium of greater density, or less elasticity, or both. If the new medium is a perfect insulator, it must be considered as

[1] *Phil Mag.*, April 1861.
[2] Brit. Assoc. Report, 1885, Aberdeen, p. 930.
[3] B.A. Report, 1887, Manchester, p. 486. Also *Phil. Mag.*, October 1887.
[4] Phil. Trans., 1881 and 1883. Also by Prof. J. J. Thomson, Math. Soc. Proc., April 1884.

having its wheels thoroughly geared up both with themselves and with those of the initial medium, so that there is no slip or dissipation of energy at the surface. In this case none of the radiation will be lost: some will be reflected and some transmitted according to ordinary and well-known mechanical laws. The part transmitted will suddenly begin to travel at a slower pace, and hence if the incidence were oblique would pursue a somewhat different path. Also, at the edges of the obstacle, or at the boundary of any artificially limited portion of the wave, there will be certain effects due to spreading out and encroaching on parts of the medium not lying in the direct path. These refraction and diffraction effects are common to all possible kinds of wave propagation, and there is nothing specially necessary to be said concerning electrical radiation on these heads which is not to be found in any work on the corresponding parts of optics.

Concerning the amount and direction of the reflected vibrations there is something to be said however, and that something very important; but it is no easy subject to tackle, and I fear must be left, so far as I am concerned, as a distinct, but perhaps subsequently-to-be-filled-up, gap.

If the gearing between the new medium and the old is imperfect, if, for instance, there were a layer of slippery wheels between them, representing a more or less conducting film, then some of the radiation would be dissipated at the surface, not all would be reflected and transmitted, and the film would get to a certain extent heated. By such a film the precise laws of reflection might be profoundly modified, as they would be also if the transition from one medium to another were gradual instead of abrupt. But all these things must remain for the present part of the unfilled gap.

Electric Radiation encountering a Conductor.

We will proceed now to the case of a *conducting* obstacle—that is, of waves encountering a medium whose electrical parts are connected, not by elasticity, but by friction. It is plain here that not only at the outer layer of such a medium, but at every subsequent layer, a certain amount of slip will occur during every era of acceleration, and hence that in penetrating a sufficient thickness of a medium endowed with any metallic conductivity the whole of the incident radiation must be either reflected or destroyed: none can be transmitted.

Refer back to Fig. 43 (vol. xxxvii. p. 347), and think of the rack in that figure as oscillating. Through the cog-wheels the disturbance spreads without loss, but at the outer layer of the conducting region A B C D a finite slip occurs, and a less amount of radiation penetrates to the next layer, E F G H, and so on. Some thickness or other, therefore, of a conducting substance must necessarily be impervious to electric radiation: that is, it must be opaque.

Conductivity is not the sole cause of opacity. It would not do to say that all opaque bodies must be conductors. But conductivity is a very efficient cause of opacity, and it is true to say that all conductors of electricity are necessarily opaque to light; understanding, of course, that the particular thickness of any homogeneous substance which can be considered as perfectly opaque must depend on its conductivity. It is a question of dissipation, and a minute but specifiable fraction of an original disturbance may be said to get through any obstacle. Practically, however, it is well known that a thin, though not the thinnest, film of metal is quite impervious to light.

When one says that conductivity is not the sole cause of opacity, one is thinking of opacity caused by heterogeneity. A confused mass of perfectly transparent substance may be quite opaque; witness foam, powdered glass, chalk, &c.

Hence, though a transparent body must indeed be an insulator, the converse is not necessarily true. An insulator need not necessarily be transparent. A homogeneous flawless insulator must, however, be transparent, just as a homogeneous and flawless opaque body must be a conductor.

These, then, are the simple connections between two such apparently distinct things as conducting power for electricity and opacity to light which Maxwell's theory points out; and it is possible to calculate the theoretical opacity of any given simply-constructed substance by knowing its specific electric conductivity.

Fate of the Radiation.

To understand what happens to radiation impinging on a conducting body it is most simple to proceed to the limiting case at once and consider a perfect conductor. In the case of a perfect conductor the wheels are connected not even by friction; they are not connected at all. Consequently the slip at the boundary of such a conductor is perfect, and there is no dissipation of energy accompanying it. The blank space in Fig. 38 (vol. xxxvii. p. 345), represented a perfectly conducting layer. Ethereal vibrations impinging on a perfect conductor practically arrive at an outer confine of their medium: beyond there is nothing capable of transmitting them; the outer wheels receive an impetus which they cannot get rid of in front, and which they therefore return back the way it came to those behind them with a reversal of phase: the radiation is totally reflected. It is like what happens when a sound-pulse reaches the open end of an organ-pipe; like what happens when sound tries to go from water to air; like the last of a row of connected balls along which a knock has been transmitted; and our massive elastic wheels are able to represent the reversal of phase and reflection quite properly.

The reflected pulses will be superposed upon and interfere with the direct pulses, and accordingly if the distances are properly adjusted we can have the familiar formation of fixed nodes and stationary waves.

The point of main interest, however, is to notice that a perfect conductor of electricity, if there were such a thing, would be utterly impervious to light: no light could penetrate its outer skin, it would all be reflected back: the substance would be a perfect reflector for ethereal waves of every size.

Thus with a perfect conductor, as with a perfect non-conductor, there is no dissipation. Radiation impinging on them is either all refracted or some reflected and some transmitted. It is the cases of intermediate conductivity which destroy some of the radiation and convert its ethereal vibrations into atomic vibrations, *i.e.* which convert it into heat.

The mode in which radiation or any other electrical disturbance diffuses with continual loss through an imperfect conductor can easily be appreciated by referring to Fig. 43 again. The successive lines of slip, A B C D, E F G H, &c., are successive layers of induced currents. An electromotive impulse loses itself in the production of these currents, which are successively formed deeper and deeper in the material according to laws of diffusion.

If the waves had impinged on one face of a slab, a certain fraction of them would emerge from the other face—a fraction depending on the thickness of the slab according to a logarithmic or geometrical-progression law of decrease. OLIVER J. LODGE.

(*To be continued.*)

PRESENT POSITION OF THE MANUFACTURE OF ALUMINIUM.

THE recent opening of new works for the manufacture of aluminium at Oldbury, near Birmingham, is distinctly an epoch in the history of this interesting metal.

The first practical steps for the manufacture of aluminium were taken in France, following the discoveries of Wöhler and of Deville, and that country has retained the monopoly of its production up to the present time. Aluminium was first obtained in a pure state in the year 1854 by St. Claire Deville whilst working in the laboratory of the Normal School, Paris, with a totally different object. Some pounds of this metal which were shown at the Paris Exhibition of 1855 had been made at the chemical works of Javel; subsequently larger plant was put up at some works at Glacière; later on we find the manufacture in an improved form transferred to Nanterre; and soon afterwards it was removed to the position in which it has ever since remained, viz. at Salindres, at the works at that time belonging to Messrs Merle and Co., but now carried on by Messrs. Pechiney and Co.

Shortly after Deville obtained aluminium by reducing the chloride with sodium, he also succeeded in isolating it by electrolyzing the double chloride of aluminium and sodium in a state of fusion. Many attempts have been made to improve this method, but although within the last year or two works have been put up both in Germany and in France which are stated to be able to produce aluminium at a comparatively cheap rate, there is no trustworthy evidence to show that they can compete with the sodium process. On the face of it there appears no reason why aluminium should not be economically manufactured in this way, since it is an undoubted fact that it can be done in the case of magnesium. There are, however, difficulties in getting aluminium to deposit in a satisfactory condition which do not occur with magnesium.

Recently, by applying electricity in a totally different way, alloys of aluminium have been manufactured on a comparatively large scale in America by Messrs. Cowles Bros. Works for the purpose are also being opened by them in England. This process, it will be remembered, consists in passing a powerful current between two carbon electrodes embedded in a mixture of alumina, charcoal, and the other metal required for the alloy. By this process aluminium in an unalloyed form has not yet been obtained, at any rate commercially.

Some fourteen years ago, Messrs. Bell Bros., of Newcastle-on-Tyne, erected works to manufacture aluminium by means of sodium; but, after incurring great expense, they abandoned the attempt, partly owing to difficulties experienced in obtaining it sufficiently pure for the manufacture of alloys, and partly because they were unsuccessful in getting it used on a sufficiently large scale. Another factory put up in Berlin was similarly abandoned, almost as soon as erected.

In America, a few years ago, Colonel Frismuth sold aluminium, which, he stated, was made by an improved sodium process of his invention; he did not, however, reduce the price, and his claims have not been substantiated. The same thing may be said of the Aluminium Company which was started about the same time in this country to work the patents of Mr. Webster, of Birmingham. It is, however, by this Company, after having undergone reconstruction, that the process is now being worked which warrants our opening statement that a fresh epoch has been reached in the manufacture of aluminium.

The process in question is the outcome of experiments commenced some six or seven years ago by Mr. H. Y. Castner in New York. He appears to have come to the conclusion that aluminium could only be satisfactorily produced by means of sodium, and he accordingly commenced work to try and improve and cheapen the manufacture of sodium. Having obtained what he considered sufficiently satisfactory results, he came over to this country about two years ago, and erected experimental works at Lambeth, where, after further trials, he succeeded in demonstrating that he was really able to produce sodium at a much cheaper rate than had before been possible; in fact, it appears he is able to produce sodium at less than 1*s.* a pound, whereas it had previously cost about 4*s.* This success led to the erection of works at Oldbury, which have been recently completed, and are now in successful operation.

In the process hitherto employed to produce sodium, an intimate mixture of carbonate of soda, lime, and charcoal is first calcined at a red heat, and this having been transferred to small wrought-iron cylinders (mercury-bottles or large gas-piping being commonly used), it is heated to about 1500° C., when the metal, having become reduced to the metallic state, distils over, and is condensed in a flat iron mould. In practice, this method is found to be defective both mechanically and chemically.

At least half the ultimate cost of production results from the wear and tear of the furnace, and the destruction of the retorts or cylinders by the comparatively high temperature. Looking at it from the chemical point of view, we find the condition of things almost as bad; little, if any, more than 40 per cent. of the sodium actually in the charge being obtained in the metallic state.

All these difficulties arise from the presence of lime in the charge, the lime being added to stiffen the mixture, and so prevent the charcoal from separating from the soda. But the thickening of the charge, which for one reason is so desirable, is equally objectionable for others. It is the thickening of the charge which necessitates the use of small cylinders and a high temperature: the material being a bad conductor, it could not otherwise be sufficiently heated. Another important difficulty in the old process arose from the presence of carbonic oxide in the gases produced in the reactions. Sodium vapour, when near its condensing-point, reacts upon carbonic oxide, forming a black refractory material which is exceedingly explosive. This is particularly the case with potassium, and is the principal reason why potassium is so much dearer than sodium.

Mr. Castner originated the idea of weighting the particles of carbon, thus doing away with the necessity of adding lime. The practical results of this modification in the method of manufacturing sodium are very far-reaching and important. The charge being perfectly fluid, it is no longer necessary to employ such a high temperature, since there is a continuous circulation of fresh material to the sides of the crucible, where the temperature is sufficiently high to set up the reactions by which the sodium is reduced to the metallic state. For the same reason large crucibles can be used instead of small cylinders. Also, the temperature of the operation being reduced from about 1400° C. to about 800° C., cast iron or cast steel may be used for the containing vessels instead of wrought iron.

The carbon particles are weighted by means of iron. The iron is first obtained in a fine state of division by reducing oxide of iron—"purple ore" being generally used for the purpose—in "producer gas," a mixture of carbonic oxide and hydrogen. The finely-divided iron thus obtained is stirred into molten pitch, which is then cooled and broken up into lumps. The next operation consists in heating these lumps in crucibles, whereby a coke is produced containing carbon and iron in the proportion of about 30 : 70; this material, technically called "carbide," having been ground up very fine, is incorporated with certain proportions of caustic soda and carbonate of soda, and the mixture is charged into large crucibles, where it is heated until the violent effervescence, due to the escape of carbonic acid and hydrogen, which takes place at first, has subsided. These crucibles are provided with holes at the bottom, closed by movable plugs. When the effervescence has ceased, the charge, in a liquid state, is run out into smaller crucibles and transferred to the furnace in which the distillation of the sodium is to take place. The preliminary heating takes about half an hour, and the actual distillation about an hour and a half.

The lid of the crucible, to which is attached the condensing arrangement consisting of an iron pipe dipping into an iron box, is fixed in the furnace; it has a convex rim which makes a joint with the grooved top of the crucible, with the assistance of a little powdered lime. The crucibles are raised and lowered by means of hydraulic power, the work of removing a crucible from the furnace and replacing it by another being done with great rapidity.

The reaction which takes place may be represented by the formula—

$$6NaHO + FeC_2 = 2Na_2CO_3 + 6H + Fe + 2Na.$$

This formula is made up in reality of several taking place *pari passu*. The main point is that it clearly expresses the final result. It will be observed that no carbonic oxide is given off, and the difficulties already referred to, caused by the presence of that gas, are got rid of. The iron is recovered, and used over and over again by coking it with fresh tar.

It is unnecessary to refer here to the arrangements for the production of the double chloride of aluminium and its reduction by sodium, as no special novelty is claimed for them.

Mr. Castner has shown great technical skill in devising the plant used throughout the works, and they are in every way a great advance on anything of the kind attempted before.

A novel feature is that hydrochloric acid, for the manufacture of the double chloride, is obtained direct by means of pipes from Messrs. Chance's glass-works, which are contiguous, and the carbonate of soda resulting from the operation in which sodium is produced is similarly conveyed to Messrs. Chance's, to be there purified and crystallized.

The estimated possible output of these works is stated to be 500 pounds of aluminium and 1500 pounds of sodium per day. The cost of manufacture of aluminium has hitherto been between 30*s*. and 40*s*. per pound. By Castner's process it is stated that it can be produced at 15*s*. That this is so there is but little reason to doubt; and it is a substantial and important reduction, which will enable aluminium to be used much more largely than has hitherto been possible. Still, before it can be very largely used, the price will have to be further considerably brought down; and it is much to be hoped that Mr. Castner's success will stimulate him and others to work with this end in view.

THE QUEEN'S JUBILEE PRIZE ESSAY OF THE ROYAL BOTANIC SOCIETY OF LONDON.

PROBABLY the last of the Jubilee productions has seen the light by the appearance of an article in the Quarterly Record of the Royal Botanic Society of London for the three months ending March last under the title of "Fifty Years of Economic Botany." The article in question forms the essay to which the Council of the Royal Botanic Society has awarded its gold medal and a purse of fifty guineas. The author is Mr. John W. Ellis, L.R.C.P. It needs only a casual glance to discover how deficient this short essay is, not only in consequence of the numerous omissions of very important plants and products, but also on account of the imperfect information given under many of the headings. Thus the writer tells his readers that China grass and rhea are two distinct fibres furnished by allied plants, the former by *Bœhmeria nivea* and the latter by *B. tenacissima*, while the fact is that China grass and rhea are one and the same thing, *B. tenacissima* being a synonym of *B. nivea*. In a casual reference to "Moong" fibre the author is apparently quite ignorant of the fact that its botanical source is *Saccharum munja*, Roxb. New Zealand flax (*Phormium tenax*) is introduced under textiles, but why is not apparent, for the author concludes his paragraph as follows—"Not having been introduced during the period to which this essay refers, any further mention of this interesting fibre—for which it has frequently been attempted to find a place in the British market—is unnecessary." Why "gun cotton and its derivatives" should occupy a special chapter it is difficult to say, seeing that this explosive substance is not a direct product of the vegetable kingdom; the author however apparently looks upon it as a much more important vegetable product than the species of cinchona, the ipecacuanha, coca, jalap, or the multitude of new drugs that have occupied such a prominent place in men's minds for the last twenty years. The success that has attended the acclimatisation of the cinchonas in our Indian possessions, whither they were introduced some twenty or thirty years since, when there was a great fear lest the supply of bark from South America should fail because of the great demand, and the consequent reduction in the price of quinine from a guinea to its present price of two shillings per ounce, are facts of sufficient importance, one would think, to be noted in any record of the progress of useful plants. And the same might also be said with regard to *Erythroxylon Coca*, considering to what purpose cocaine is now being put, but the author—a member of the medical profession—has apparently a wholesome dread of drugs, and for once has ignored all consideration of them. He seems to have been content to consult very old books for his facts throughout and to have completely passed over modern authorities; consequently his statements are both antiquated and incorrect.

The old name of *Siphonia elastica* is quoted for the Para rubber plant instead of the now better known name of *Hevea brasiliensis*. Balata is referred to *Sapota Mulleri* instead of *Mimusops globosa*, and we read that Mr. Jenman's report on the Balata Forests of British Guiana issued in 1885 "will probably assist in developing a demand for this material," while the fact is that balata has been going down in the estimation of manufacturers since that date in consequence of it having been found not to be durable when exposed to the air; manufactured articles made from it cracked on the surface, and the inner portion lost its tenacity, so that some manufacturers have given up its use entirely. The Dika plant of W. Trop. Africa, which has long been identified with the Simarubeous plant (*Irvingia Barteri*), is referred to under the very old name of *Mangifera Gabonensis*, a genus belonging to the natural order Anacardiaceæ. Again carapa or croupee oil of West Africa is said to be obtained from the seeds of *Carapa guineensis* and crab oil of British Guiana from *Carapa guianensis*. These two were combined by Prof. Oliver under *C. guyanensis* in the "Flora of Tropical Africa" so far back as 1868.

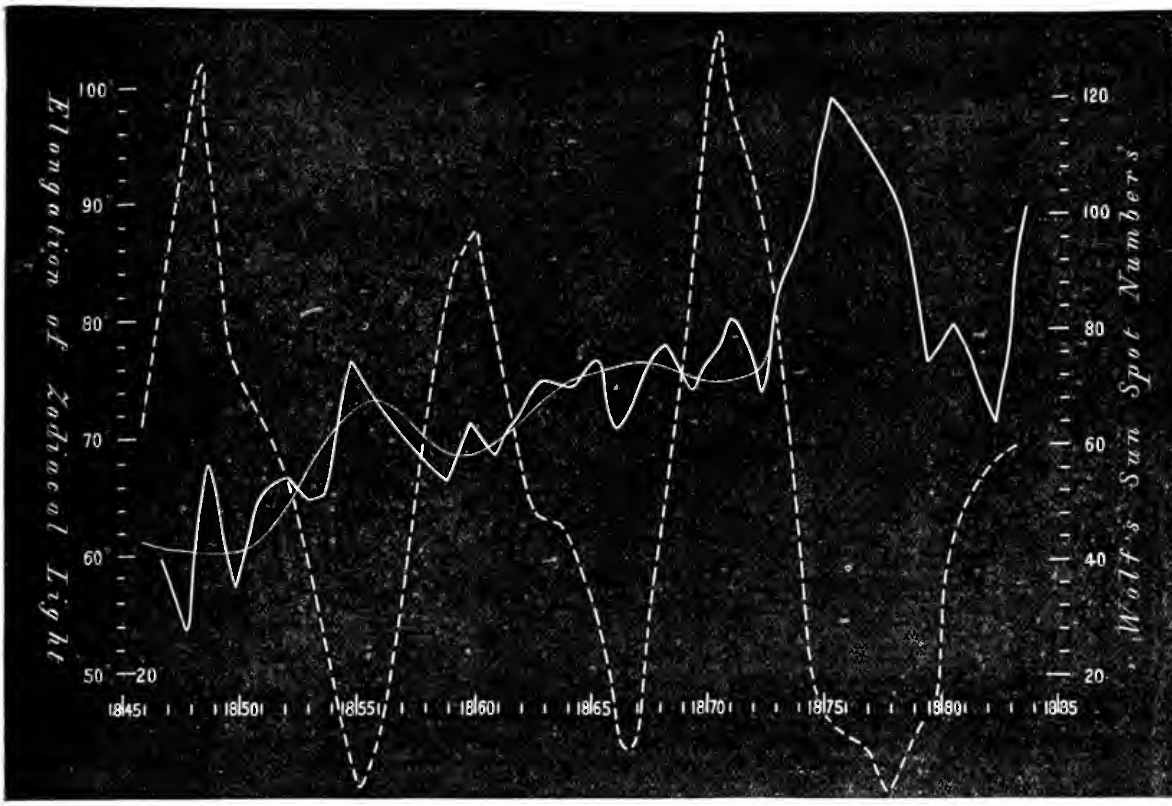
These are only a few illustrations of the general untrustworthiness of the essay, the circulation of which, it is hoped, will not be large.

THE ZODIACAL LIGHT.

FROM the days of Cassini a connection between the zodiacal light and sun-spots has been suggested. In some recent discussions it is denied. But, so far as I am able to discover, the long series of observations by Heiss and Weber, extending from 1847 to 1883, afford the first opportunity to attack the question.

The result is in the diagram before you. The broken line represents Wolf's well-known series of relative sun-spot numbers, the jagged full line the mean elongations of the apparent apex of the zodiacal light from the sun. It will be seen that each sun-spot minimum corresponds with a maximum of the zodiacal light, and each sun-spot maximum with a minimum of the zodiacal light. The minimum in 1870 must be considered as masked by the forces tending to produce the enormous maximum of 1876. It will be noticed, too, that when the sun-spot phenomena are more extensive, as in 1850 and 1870, the following zodiacal light phenomena are also more extensive; where the sun-spot phenomena are less, as in 1860, the following zodiacal light phenomena are less extensive; and *per contra*, when the zodiacal light phenomena are extensive, as in 1880, the sun-spot phenomena are less extensive. As far as this series goes, the correlation seems to be complete.

We may gain some insight into the relation by tabulating the various spectroscopic observations in their order in the sun-spot cycle. Thus we have Lias, for four years during the rise in the sun-spot period, observing only a faint continuous spectrum; Respighi and Lockyer, just after sun-spot maximum, one bright line; Vogel, the same; Smyth, Secchi, Pringle, about the same date, no spectrum, or only a continuous spectrum; Tacchini, possibly a bright line; Wright, three years after maximum, generally only a continuous spectrum,—three times a bright line; Burton, fourth year after sun-spot maximum, continuous spectrum; generally a bright line; Arcimis, five years after sun-spot maximum, continuous spectrum and *two* bright lines (1480 K and 2270 K). It would seem, therefore, that the zodiacal light is more gaseous at sun-spot minimum, and only slightly, if at all gaseous, at and near sun-spot maximum.



Comparison of zodiacal light elongations with Wolf's relative sun-spot numbers.

The same story is told by the disturbances suffered by Encke's comet.[1]

We would consider, therefore, the zodiacal light a locus of condensation.

One may notice, too, that the light appears, in common with the frequency of auroræ and the diurnal range of the declination-needle, to be affected by a disturbance of longer period. But for the present we must restrain ourselves from the connections with terrestrial and cosmical physics with which the matter teems, and ask—what is the principal object of this communication—that those who are not observing will observe, and that those who have, or know of the places of concealment of, any observations, will kindly call them to our attention.

Baltimore, Md. O. T. SHERMAN.

[1] *Gould's Astronomical Journal.*

CHEMISTRY AT THE BRITISH ASSOCIATION.

It was hardly to be expected that the proceedings of the Chemical Section of the British Association would be as remarkable at Bath as at Manchester. Nevertheless, at Bath some interesting discussions took place, and some valuable papers were read.

The President's Address was listened to with great interest, and formed a fitting introduction to the discussion, which afterwards took place, on the teaching of chemistry.

In the "Report of the Committee on the Action of Light on the Hydracids, in Presence of Oxygen," read by Dr. Richardson, some experiments were described, in continuation of those read before the Association last year. The influence of traces of free chlorine and of moisture on the course of the reaction was investigated.

In connection with the "Report of the Committee on the Properties of Solutions," read by Dr. Nicol, a new apparatus for determining solubilities at temperatures below 100° was shown. Excellent results had been obtained, owing to the very intimate mixture of the salt and solvent.

Dr. Johnstone Stoney exhibited to the Section a diagram illustrating the logarithmic law of the atomic weights. Many curious relations are brought out by its means. If, as seems probable, the logarithmic law be a law of Nature, there appear to be three elements lighter than hydrogen.

Prof. Sterry Hunt, in his paper on "The Study of Mineralogy," advocated a system of mineralogy, based on the successive forms which are imposed upon matter: (1) the chemical form or composition; (2) the mineralogical form, or physical state; (3) the crystalline form, being the most accidental.

Some speculations suggested by Van 't Hoff's hypothesis were put forward by Mr. J. E. Marsh, attention being drawn to certain compounds, which appear to be *geometrical* isomers.

The same author, in another paper, suggested a new constitutional formula for camphoric acid.

On the Friday morning an interesting and well-attended discussion (at which the members of Section D were present) was opened by Prof. Michael Foster, on the "Chemical Problems presented by Living Bodies." In the course of his remarks he suggested several subjects for chemical investigation, such as the exact chemical difference of certain proteids, the changes which occur in the curdling of milk and the clotting of blood, and, to the biologist, the all-important question of the relation in which water stands to the organism.

An animated discussion followed, in which several chemists and biologists took part. In reply to Prof. Thiselton Dyer's question, as to whether the processes employed by chemists had any connection with those which take place in Nature, Prof. Armstrong cited several cases in which the chemical changes occurring in Nature bore a suggestive relation to those brought about in the laboratory.

In their paper on the "Incompleteness of Combustion on Explosion," Prof. H. B. Dixon and H. W. Smith show that, on exploding a mixture of oxygen and hydrogen in a long tube, a considerable residue of gas is obtained, which is still explosive. Experiments were made to arrive at the cause of the phenomenon, and an explanation is suggested.

A new gas-analysis apparatus was shown by Dr. Nicol, which combined the advantages of the Hempel apparatus with the means of using mercury and of readily performing explosions.

Dr. Bott exhibited a modification of a vapour-density apparatus, previously described, which can be employed at any temperature or pressure.

On the Saturday morning Prof. Dunstan read the "Report of the Committee on the Teaching of Chemistry," which was followed by a paper on "Chemistry as a School Subject," by the Rev. A. Irving.

In the ensuing discussion, which was confined to the teaching of chemistry in schools, many of the speakers seemed to agree with the opinions quoted in the report, viz.—

(1) That chemistry should be taught in schools, first, and mainly, on account of the mental training it affords; and, secondly, for the sake of its applications, and its direct bearing on the facts of every-day life.

(2) The chief difficulties met with in teaching seem to be those which arise from (i.) defective organization and considerations of expense; (ii.) the lower value attached to chemistry in comparison with other subjects of the school curriculum; (iii.) the time which is devoted to the subject; (iv.) preparation for various examinations; (v.) absence of good text-books; (vi.) dearth of properly-qualified teachers.

(3) The older plans of teaching are felt to require modification.

The Committee ask for reappointment.

A discussion on "Valency" was opened on Monday by Prof. Armstrong. The question of constant and variable valency was referred to in connection with such compounds as chloroplatinic acid, &c., and a few new terms were introduced. The constitution of such bodies as tetra-methyl-ammonium iodide was considered. Dr. Morley drew attention to the influence which one element in a compound often has in modifying the properties of another not immediately adjacent to it. Chemists were advised to study the facts connected with the question carefully before speculating.

Later on, Mr. Veley described an ingenious arrangement he had invented for studying the action of acids on copper, under simple conditions.

The closing sitting was opened by Prof. Armstrong, who read the "Report of the Committee on Isomeric Naphthalene Derivatives." It was shown that the existence of all the known dichlor-naphthalenes can only be explained by the use of space-formulæ.

In a "Note on the Molecular Weight of Caoutchouc and other Bodies," Dr. J. H. Gladstone and W. J. Hibbert attempted to apply Raoult's method to the determination of very high molecular weights, with fair results.

Some interesting compounds of silicon with thio-carbamide and with aniline were exhibited and described by Prof. Emerson Reynolds, together with several other new thio-carbamide compounds. An account of these exhibits was given in NATURE last week (p. 575).

Dr. Richardson, in his paper on "The Action of Light on Water-colours," drew attention to the very important part played by moisture in assisting their decomposition. Colours are divided into two groups: (1) those which bleach under the combined influence of light, air, and moisture; (2) those on which light exerts a reducing action, which is independent of the air, and in some cases takes place in the absence of moisture.

A paper on "Pyrocresols," by Dr. W. Bott and J. B. Miller, was illustrated by specimens of a large number of derivatives of α-pyrocresol, amongst them being two new azo colouring-matters.

With the reading of this paper the proceedings terminated.

By the courtesy of several chemical manufacturers in the neighbourhood, the members of the Section were enabled, during the course of the meeting, to visit several works where interesting operations were being carried on.

GEOLOGY AT THE BRITISH ASSOCIATION.

THE most important geological work done at Bath this year related to volcanic and earthquake phenomena. Dr. Johnston-Lavis gave an account of the recent eruption in Vulcano, and read the letter which has already appeared in the *Times* from Mr. Narliau, a deeply interested and much-injured witness of the whole occurrence. The chief features seem to have been the ejection of very large blocks to a great distance—one, measuring 10 yards in length, having been found three-quarters of a mile from the crater—and the occurrence of flames, probably caused by the combustion of sulphur deposits. This paper was illustrated by lantern photographs taken by Dr. Tempest Anderson three months before the event. The latter gentleman also exhibited photographs of Vesuvius, Stromboli, and Etna, showing different phases of eruption.

Dr. Lavis presented a report on Vesuvius, describing various new sections cut through the tuffs and lavas of Vesuvius and the Phlegrean fields. The report announced the completion of the author's map of Vesuvius, and claimed to have established that the volcanic activity of the mainland had followed a regular course southwards. The same author announced the discovery of leucite in a lava from Etna, and in another paper attributed the conservation of heat in volcanic chimneys to latent heat set free on the passage of magma from a vitreous to a crystalline condition. Among the other papers were one by Dr. Claypole, who pointed out that in many places, and notably in the Appalachians, strata had been forced up from a depth greater than five miles, the supposed depth of the "layer of no strain"; and one by Mr. Logan Lobley, who attributed (1) the formation of lava to heat in the earth's interior inducing chemical action, (2) its ejection to the expansion due to change from a solid to a fluid state, and (3) explosive eruption to the access of sea- and land-water to the volcanic focus. In the discussion a good deal of misunderstanding seemed to arise from the confusion of "zone of no strain" with "zone of no cooling."

Prof. J. Milne gave tables to show the distribution of Japanese earthquakes in connection with years, seasons, months, and hours of the day. Further tables proved that the majority of earthquakes coincide with a high barometer, and that they are more frequent when the glass is falling or rising, than when it is steady. Earth-tremors are almost always associated with strong wind.

The local interest centred round papers on the Oolitic and Carboniferous rocks. Mr. Horace Woodward united the Cotteswold, Bridport, and Yeovil Sands under the name of Midford Sands; thought that the fullers' earth should be grouped with the Great Oolite which its upper beds sometimes replaced; and preferred to divide the Portlandian in Britain into an upper division, including the Portland, Tisbury, and Swindon *stone*, and a lower division, to hold the Portland Sand and Hartwell Clay.

A very interesting communication from Mr. Whitaker described the occurrence of the Bath Oolite at a depth of 1081 feet in the Streatham boring, the author hoping that the boring would be continued on the chance of meeting some porous rock under this which might have tapped off the Lower Greensand waters. Even if this did not take place, he trusted that the boring might be continued for purely scientific purposes, and as another opportunity of testing the question of coal under London.

Mr. Wethered correlated the Lower Carboniferous limestone of Gloucestershire with the Tuedian and Calciferous series of the north of England; and Mr. Handel Cossham described a series of trial shafts and headings which proved the existence of a reversed fault with very low hade on the northern part of the Bristol coal-field: the effect of the faulting of the strata was nearly to double the known coal resources in the western part of the field. A similar overthrust, bringing Carboniferous limestone to rest in dolomitic conglomerate at Tytherington was described by Mr. Winwood; and Mr. Ussher called in similar faults to explain the position of the Vobster limestone patches in Somerset. The latter author considered the Watcombe terra-cotta clay to be of Triassic age.

There were a few papers on the Archæan rocks, but little that was new was brought forward. Dr. Persifor Frazer considered that the central rocks of the nucleal ranges of the Antilles were Archæan; and Dr. Irving summed the evidence for life in this system, and found it wanting.

Mr. Bell's "Report on the Manure Gravels of Wexford" concluded that these were immediately pre-glacial in age, and that the Killiney gravels, and the marls, clays, and brick-earths of the coast were of later date. Mr. Clement Reid recorded *Betula nana*, *Salix polaris*, and *S. myrsinites* from the lacustrine deposit of Hoxne, to prove that it was formed in a severe climate preceded by a warmer one in which yew, bur-reed, and cornel flourished. A lengthy report from Dr. Crosskey on new erratic blocks in Yorkshire, Essex, Lancashire, and Leicestershire, was followed by a paper on a high-level boulder-clay (700 feet) in the Midlands, in which the same author inclined to the theory that it was floated from the nearest glacier and deposited by ice-foot and ice-bergs. Mr. Shore recorded Neolithic flakes and a hammer-stone found in peat below the tidal alluvium at the Southampton new dock excavation; and Mr. Lamplugh's report on the old subglacial sea-beach at Bridlington gave proof of some remarkable changes in the physical geography of the Yorkshire coast since the time of its formation.

Amongst the palæontological work was Prof. Rupert Jones's "Report on the Palæozoic Phyllopods"; and Prof. Williamson's on the Carboniferous flora, in which the author showed that the central vascular bundle of the Carboniferous Cryptogams contained a germ which developed into a persistent pith, while portions of the medullary tissue assumed the functions of a cambium. Dr. Irving described experiments to show that the vigour of plant life is increased until the percentage of CO_2 in the atmosphere equals the oxygen; and Mr. Whidborne briefly described many new species of Cephalopoda, Gasteropoda, Crustacea, and Conchifera from the Devonian of various localities. An important communication was made by Mr. H. F. Osborn, who traced back the Mammalian teeth to the tritubercular and thence to the triconodont type, and proposed a new nomenclature based on this principle. Prof. Gaudry commented on the gigantic size of some Tertiary Mammalia, Prof. Seeley on an Ichthyosaurus from Africa, and Prof. Marsh on the classification of the Dinosaurs. Mr. Smith Woodward and Prof. Bassani dealt with fish-remains from the Chalk, London Clay, and Lower Miocene.

Among the petrological papers we may note:—Dr. Sterry Hunt on mineralogical evolution, in which the author attempted to correlate chemical resistance with hardness, and this with condensation, in minerals; and to show that the greater stability of those (silicates) which belong to the more condensed types was shown in their superior resistance to decay. Dr. Sterry Hunt concludes that the great successive groups of stratiform crystalline rocks mark necessary stages in the mineralogical evolution of the planet. Mr. Joly decolorized beryl at 357° C., and has discovered twelve-sided basal prisms of iolite in the Dublin granite. Prof. Seeley raised a discussion on Oolitic structure, in which Dr. Gilbert instanced the formation of recent Oolites in the Great Salt Lake. Prof. Blake presented a long report on the Anglesey rocks, in which he described the passage of dolerites into hornblende and glaucophane schists, and then into slate-like rocks; and of gabbros into talcose schists. Mr. Watts described an igneous succession in Shropshire from old acid andesites through younger dolerites into picrites, without any break in the sequence; and Dr. Persifor Frazer exhibited and described some curious specimens of glassy and spherulitic oligoclase and quartz with peculiar optical properties. Though not precisely belonging to this Section, some clay models exhibited and described by Dr. Ricketts may here be mentioned, in which, by vertical pressure in the centre, reversed folds and inverted faulting had been produced. The author attempted to apply this method to explain the folding and cleavage of the Silurian slates in Wales.

NOTES.

SOME time ago Lord Crawford offered to present to Scotland his valuable collection of astronomical instruments at Dun Echt, on condition that suitable accommodation should be provided for it, and that it should be managed for the public benefit. The Secretary for Scotland, we are glad to learn, has accepted Lord Crawford's offer; and the Treasury has agreed to provide means for the erection of the necessary buildings. A committee of scientific men is engaged in examining different sites around Edinburgh which seem suitable for the erection of a national Observatory; and, according to the Edinburgh Correspondent of the *Times*, the choice seems to lie between the Braid Hills and the Blackford Hill, both of which are on the south side of the city. The same writer says that two proposals have been made for utilizing the old Observatory on the Calton Hill—the one that, after the instruments have been repaired, the place should be used as a popular Observatory; the other, that it should be attached to the Heriot-Watt Technical College for class-work in connection with the lectureship on astronomy there.

THE Mercers' Company, one of the oldest and wealthiest of the City Companies, is thinking of establishing an Agricultural College. A correspondent of the *Times* says it proposes to devote £60,000 to this object. According to the same authority, the intention is that the College shall be in Wiltshire, and that there shall be attached a farm of considerable extent, in which the pupils may practically apply the knowledge they gain, the institution being intended to benefit the sons of farmers and others who will be dependent on the successful culture of land for their future livelihood. The sum of £60,000 contributed by the Company would, it is hoped, be supplemented by a liberal donation from the Charity Commissioners, and the Company would of necessity be prepared to provide an adequate endowment.

THE new laboratories at Trinity College, Dublin, which are now open to all students of chemistry, comprise general laboratories for instruction in elementary chemistry, and quantitative and research laboratories. The latter are provided with all modern appliances, and have special rooms attached for analysis of gas and water, for assaying, and for ultimate organic analysis. The laboratories are under the general direction of Prof. Emerson Reynolds, F.R.S.

A STATUE of Ampère was unveiled on October 9, at Lyons, his native place. The ceremony took place before the President of the French Republic; and M. Cornu, a member of the French Academy of Sciences, delivered an elaborate address, in which he spoke of the importance of Ampère's discoveries.

THE Council of the Institution of Civil Engineers has issued a list of subjects upon which, among others, original communications are invited for reading and discussion at the ordinary meetings, and for printing in the minutes of proceedings of the Institution. For approved papers the Council has the power to award premiums, arising out of special funds bequeathed for the purpose.

THE Society of German Engineers offers a prize of 5000 marks (£250) for the best essay containing a critical estimate of experimental investigations concerning the passage of heat through heated surfaces, in its relation to material, form, and position of the latter, as well as to the kind, temperature, and motion of the heated substances. Competitors are to forward their treatises to the General Secretary of the Society by December 31, 1890.

THE Tokio Mathematical and Physical Society proposes, in order to commemorate the tenth anniversary of its foundation, to award a prize not exceeding 20 *yen* (£4) in value for the best original paper on the properties of the so-called asymptotic curves, and the relations (if any) existing between these curves and straight lines on a surface—in particular, an algebraic surface.

SEVERAL influential Chinese have subscribed large sums of money to aid in establishing a zoological garden at Shanghai. At present the institution will be merely a commercial undertaking, but it is hoped that ultimately the State will take it over. Amongst others, the Governor of Formosa has promised his help in the collection of specimens.

IN the last issue of the Journal of the Russian Chemical and Physical Society there is an interesting article on Prof. S. A. Wroblewski, whose death at Cracow we lately recorded. While a student of the Kieff University, Wroblewski took part in the Polish insurrection of 1863, and was exiled to Siberia, where he had to remain for six years. During his term of exile he elaborated a new cosmical theory, which on his return he hastened to submit to German men of science. Helmholtz received the young man cordially, but advised him to make at the Berlin laboratory certain experiments which would convince him of the erroneousness of his ideas. Wroblewski at once began earnest physical and chemical work, and never afterwards spoke of the theory of his youth. In 1874 he went to Strasburg, and there he published his first serious work, "Ueber die Diffusion der Gase durch absorbirende Substanzen." The flattering opinion expressed about this work by Maxwell in NATURE encouraged Wroblewski to continue physical work on the same lines. He was offered the Chair of Physics at the Cracow University, and the authorities of that institution gave him permission to spend a year at Paris in the laboratory of Sainte-Claire Deville, before beginning his University teaching. There Wroblewski discovered, in the course of his work on the saturation of water with carbonic anhydride under strong pressures, the hydrate of carbonic oxide, and that discovery became the starting-point of a series of works on the condensation of gases. His capital discoveries, made in association with M. Olszanski, which resulted in the condensation of oxygen, azote, and hydrogen, are well known. He was making preparations for an elaborate volume on the condensation of hydrogen, when he perished by accident. While working late in the night in his laboratory, he fell asleep, and in his sleep he overthrew a kerosene lamp. His clothes began to burn, and the wounds thus received resulted four days later in death. The Journal gives a complete list of Wroblewski's works.

AN interesting archæological discovery has been made in the tidal river Hamble, near Botley, Hants. A boathouse is being built at the point of the junction of the Curdridge Creek on the river, some distance above the spot where there is a still existing wreck of a Danish man-of-war. While the mud and alluvial soil were being removed to make sufficient waterway, something hard was encountered, which on being carefully uncovered proved to be a portion of a prehistoric canoe. It is about 12 feet long by 2½ feet wide, beautifully carved, and in a fairly good state of preservation.

THE other day a peasant at Vestervang, in West Jutland, found a splendid piece of amber in a marl pit, weighing 1½ pound.

M. HALLEZ has published, in the first number of the *Revue Biologique du Nord de la France*, an interesting paper on the natural scavengers of various beaches of Northern France. At Boulogne, the species *Nassa*, which is very abundant, performs the useful office of destroying all dead animal relics. At Portel, *Nassa* is scarce, but *Eurydice pulchra* is very abundant, and takes the business in hand. At Cape Alprech, there are neither *Eurydice* nor *Nassa*, but *Ligia oceanica* fulfils their duties. At Equihen, these duties are undertaken by numerous *Orchestiæ*. It is worth noting that these four points are quite close to each other.

THE chemistry of the modern advantageous method of manufacturing chloroform from acetone and bleaching-powder has formed the subject of the successful researches of Messrs. Orndorff and Jessel. The first stage of the reaction is found to consist in the formation of methyl chloral, $CH_3 . CO . CCl_3$, which is subsequently acted upon by the hydrate of calcium formed at the same time, with production of chloroform and calcium acetate. The changes are expressed by the following equations:—

$$2CH_3 . CO . CH_3 + 6CaOCl_2 = 2CH_3 . CO . CCl_3 + 3Ca(OH)_2 + 3CaCl_2;$$
$$2CH_3 . CO . CCl_3 + Ca(OH)_2 = 2CHCl_3 + Ca(C_2H_3O_2)_2.$$

Calculated from these equations, the yield of chloroform should be 206 per cent. of the weight of acetone employed. As a matter of fact, the process has now reached such a state of perfection that as much as 188 per cent. is actually obtained in the best manufactories.

ANOTHER rich yield of new organic compounds has been obtained by M. Paul Adam by an application of the famous aluminium chloride reaction to the hydrocarbon diphenyl, $C_6H_5 . C_6H_5$. The number of new substances which have been synthesized by use of this reaction since its introduction by Messrs. Friedel and Crafts must now be enormous, and its value in assisting the completion of the fabric of descriptive organic chemistry cannot be over-estimated. The method of treatment consists in mixing the substance to be acted upon, in this case diphenyl, with aluminium chloride in a flask connected with an inverted condenser, to the end of which is attached a bent tube arranged so as to dip beneath the surface of mercury. If necessary, just sufficient heat is applied in order to keep the mixture in the liquid state; when this is effected the haloid compound of the radical to be introduced is allowed to slowly enter from a dropping funnel. Hydrochloric or hydrobromic acid is at once disengaged, the gas coming off steadily and readily indicating the progress of the reaction, which results in the substitution of the radical for hydrogen of the original substance. On completion of the reaction it is only necessary to place the mass in water, so as to decompose the aluminium chloride, when the black liquid becomes decolorized and the new substance separates as a colourless liquid or crystalline solid. When methyl chloride, CH_3Cl, is allowed to act in this way upon diphenyl in presence of aluminium chloride, M. Adam finds that the chief product is methyl-diphenyl, $C_6H_5 . C_6H_4 . CH_3$, in which the methyl group occupies the meta position. This new body is a highly refractive colourless liquid, boiling about 271°–277° C., and remaining liquid as low as −21°. It is isomeric with Dr. Carnelley's para-compound, the only one hitherto known. The ethyl and methyl ethers were readily obtained by the usual methods, and from the latter yellow syrupy substance, $C_6H_5 — C_6H_4 — CH_2 — OCH_3$, was obtained by the action of gaseous hydriodic acid, a highly interesting body, $C_6H_5 — C_6H_4 — CH_2 — OH$, the alcohol of the series, phenyl-benzyl alcohol, a very viscid liquid which eventually crystallized. The mono-methyl derivative, however, was not the only product of the primitive reaction, for M. Adam also succeeded in isolating a dimethyl phenyl, $C_{12}H_8(CH_3)_2$, boiling at 284°–290°. Moreover, a similar series of derivatives were next obtained containing ethyl instead of methyl; and finally the synthesis of the hydrocarbon fluorene, $\left.\begin{array}{l} C_6H_4 \\ | \\ C_6H_4 \end{array}\right> CH_2$, discovered by Berthelot in coal-tar, was effected by acting in a similar manner with methylene dichloride, CH_2Cl_2, upon diphenyl in presence of the accommodating aluminium chloride.

AN interesting discussion on "Bird Pests of the Farm" is printed in the current number of the *Zoologist*. All the writers who take part in the discussion agree that the habits of rooks have for some time been undergoing a remarkable change. Formerly, rooks lived chiefly on grubs and worms. Their supply of this kind of food has been greatly diminished by better farming, draining, and other improvements; and at the same time the birds have largely increased in numbers. Consequently they have been obliged to look for new sources of food-supply. They do very serious injury to cultivated crops, and devour enormous quantities of the eggs of game-birds. Mr. H. H. Scott says that during nesting-time, in districts where there are large rookeries, the heather on the moors and the fences in the fields are searched by rooks, yard by yard, for these eggs. Mr. Gilbert Millar, head-keeper to Mr. Creswell, of Harchope Hall, Alnwick, testifies that twenty-five or thirty years ago rooks were rarely known to take eggs; "but," he adds, "they have turned gradually worse every year since then, and now they have become a perfect pest and take all the early nests. Not one out of every twenty early nests that I have known of, these last few years, has escaped them." Pheasants' nests are sometimes built in rookeries, but, oddly enough, they are safer there than outside, as rooks never seem to look for them under their own nests.

AT the general meeting of the Council of the French Meteorological Office, Admiral Cloué, Vice-President, stated that the service of weather forecasts during the past year had reached 90 per cent. of successes, a figure never before surpassed. The number of climatological stations from which reports are regularly received is 143. Among the foreign stations we observe that two are being established in Madagascar. As an encouragement to observers on board ship, sixteen gold medals were presented during the year, for the best log-books received. Telegrams from America are regularly received, and include reports of storms, &c., met by ships in the Atlantic. M. Mascart stated that the work of the Departmental Commissions continued to improve each year, and that now there were only six departments which had not special Commissions. M. Vaussenat gave an interesting account of the observation of thunderstorms and of the photography of clouds and lightning on the Pic-du-Midi, and M. Janssen urged the importance of cloud photography at regular intervals, and of a systematic study of cloud formations and modifications.

THE Meteorological Report of the Straits Settlements for the year 1887 contains, in addition to the usual monthly and annual summaries at the four principal Observatories: (1) a tabular statement of the mean annual and monthly rainfall at Singapore from 1869 to 1887; and (2) charts showing the mean annual range of various elements at Singapore from 1870 to 1887. The year 1887 has presented little that is striking or anomalous. The rainfall of the colony, which is represented by thirty-nine stations, has been more than in the previous year.

THE Royal Society of Tasmania has issued its Papers and Proceedings for 1887. Among the papers we may note the following: description of new rare Tasmanian Hepaticæ, by B. Carrington and W. H. Pearson; on the acclimatization of the salmon (*Salmo salar*) in Tasmanian waters, by W. Saville-Kent; a first list of the birds of Maria Island, by Colonel W. V. Legge; observations with respect to the nature and classification of the rocks of the Tertiary period, more particularly relating to Tasmania, by R. M. Johnson.

MESSRS. MACMILLAN AND CO. have just published the third edition of Lock's "Arithmetic for Schools." Simultaneously with this edition, a key to the work, by the Rev. R. G. Watson, has been issued. Mr. Lock explains that the solutions have been very carefully worked under his superintendence.

THE "Hand-book of Jamaica" for 1888–89, by A. C. Sinclair and L. R. Fyfe, has been issued. It is compiled from official and other trustworthy sources, and includes ample historical, statistical, and general information concerning the island.

A GUIDE to the Caucasus, by E. Weidenbaum, has been published at Tiflis by order of the Governor-General. It contains much archæological information.

We have received the tenth volume of the third series of the Memoirs, and the first volume of the fourth series of the Memoirs and Proceedings, of the Manchester Literary and Philosophical Society.

The University College of Liverpool, and the University College of Wales, Aberystwith, have each issued a calendar for the session 1888–89.

Messrs. Longmans and Co. have in the press the following works:—"A Hand-book of Cryptogamic Botany," by A. W. Bennett and George R. Milne Murray; "A Text-book of Elementary Biology," by R. J. Harvey Gibson; "Force and Energy: a Theory of Dynamics," by Grant Allen; and Part I of "Graphics; or, the Art of Calculation by Drawing Lines, applied to Mathematics, Theoretical Mechanics and Engineering, including the Kinetics and Dynamics of Machinery, and the Statics of Machines, Bridges, Roofs, and other Engineering Structures," by Prof. Robert H. Smith.

Messrs. Chapman and Hall will shortly publish "Thirty Thousand Years of the Earth's Past History," by Major-General A. W. Drayson; and "Marine Engines and Boilers," by Mr. George C. V. Holmes.

Among the works announced by Messrs. Sampson Low and Co. are the following:—"Metallic Alloys; a Practical Guide for the Manufacture of all kinds of Alloys, Amalgams, and Solders used by Metal-workers, especially by Bell-founders, Bronze-workers, Tinsmiths, Gold and Silver Workers, Dentists, &c., &c., as well as their Chemical and Physical Properties," edited chiefly from the German of A. Krapp and Andreas Wildberger, with many additions by William T. Brannt; "The American Steam Engineer: Theoretical and Practical, with Examples of the latest and most approved American Practice in the Design and Construction of Steam-Engines and Boilers," for the use of engineers, machinists, boiler-makers, and engineering students, fully illustrated by E. Edwards, C.E.; "Science and Geology in Relation to the Universal Deluge," by W. B. Galloway, M.A., Vicar of St. Mark's, Regent's Park; "Technology of Textile Design: being a Practical Treatise on the Construction and Application of Weaves for all Textile Fabrics, with minute Reference to the latest Inventions for Weaving," containing also an appendix showing the analysis and giving the calculations necessary for the manufacture of the various textile fabrics, by E. A. Posselt, Head Master, Textile Department, Pennsylvania Museum and School of Industrial Art, Philadelphia, Pa.

Dr. Birkbeck Hill, the editor of Boswell's "Johnson," has nearly ready for publication through the Clarendon Press a collection of letters from David Hume to William Strahan, hitherto unpublished. In the preface he recounts the circumstances under which Lord Rosebery purchased the originals when the authorities of the Bodleian and of the British Museum had declined them. A "Life of Hume" has been prefixed, and the letters have been fully annotated.

We have received a copy of a pamphlet entitled "The Technical Education of Engineers," a course of technical study recommended by the Manchester Association of Engineers to youths engaged in engineering workshops and other mechanical trades. There are practical hints as to the course to be pursued in each subject, and the names of books recommended by the Association are given. The little work, which only costs two-pence, should be in the hands of all those for whose aid it was compiled.

The Botanical Exchange Club of the British Isles has issued its Report for 1887. Mr. Arthur Bennett indicates the new county records in the plants contributed.

Mr. Saville-Kent, at present engaged in officially investigating and reporting upon the fish and fisheries of various of the Australian colonies, has accepted an invitation from Captain the Hon. F. C. Vereker and other officers of H.M.S. *Myrmidon*, to join that ship at Port Darwin and to take part in the marine natural history exploration of the northern and north-western Australian coast in association with the survey work now being conducted. Mr. Saville-Kent proceeds *viâ* Brisbane and Thursday Island, taking with him trawls, dredges, and other apparatus suited for the projected work.

The Committee of the Sunday Lecture Society have decided that during the winter a course of twenty-one lectures shall be given in St. George's Hall, London, on Sunday afternoons, at 4 p.m., as in former years, beginning on October 21.

The next ordinary general meeting of the Institution of Mechanical Engineers will be held on Wednesday, October 24, and Thursday, October 25, at 25 Great George Street, Westminster. The chair will be taken at 7.30 p.m., on each evening, by Charles Cochrane, Esq., Vice-President, in the absence of the President, Edward H. Carbutt, Esq., who is travelling in America. The discussions will be resumed on the following papers read at the last two meetings in May and August: description of Emery's testing machine, by Mr. Henry R. Towne, of Stamford, Connecticut, U.S.A.; description of the compound steam turbine and turbo-electric generator, by the Hon. Charles A. Parsons, of Gateshead. The following papers will be read and discussed, as far as time permits: description of the Rathmines and Rathgar township water-works, by Mr. Arthur W. N. Tyrrell, of London; supplementary paper on the use of petroleum refuse as fuel in locomotive engines, by Mr. Thomas Urquhart, Locomotive Superintendent, Grazi and Tsaritsin Railway, South-East Russia.

The additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♂) from India, presented by Miss Kate Marion Pope; a Brush-tailed Kangaroo (*Petrogale penicillata* ♂), a Laughing Kingfisher (*Dacelo gigantea*) from New South Wales, presented by Captain Philp; a Gazelle (*Gazella dorcas* ♀) from North Africa, presented by Mrs. Eugenio Arbib; a Brazilian Hangnest (*Icterus jamaicai*) from Brazil, presented by Mr. T. R. Tufnell; five —— Francolines (*Francolinus* —— 2 ♂ 3 ♀) from South Africa, presented by Captain Larmer; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Mr. H. Butler; two Slowworms (*Anguis fragilis*), British, presented by Mr. Cecil L. Nicholson; two Alpacas (*Lama pacos*) from Peru, two Upland Geese (*Bernicla magellanica* ♂ ♀) from the Falkland Islands, three Crested Pelicans (*Pelecanus crispus*), South European, deposited; four Esquimaux Dogs (*Canis familiaris*, var.), a Bennett's Wallaby (*Halmaturus bennetti* ♀), a Vulpine Phalanger (*Phalangista vulpina*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

The Solar Parallax from Photographs of the Last Transit of Venus.—A preliminary value of the solar parallax, as obtained from the measurement of the photographs of the sun taken at the different American stations during the transit of Venus, of December 1882, has been recently published. This value is based upon the measured distances of the centres of the sun and of Venus on 1475 photographs, taken at ten stations, six in the United States, two in South America, and the remaining two at Wellington, South Africa, and Auckland, New Zealand. It compares as follows with the values deduced from the American and French photographs respectively of the transit of 1874:—

American 1882	$\pi = 8''.847 \pm 0''.012$
American 1874	$\pi = 8''.883 \pm 0''.034$
French 1874	$\pi = 8''.80$

The value now found, though probably a close approximation to that which will be afforded by the complete discussion of all the photographs, cannot be regarded as final, since, amongst other reasons, the reduction of the position-angles of Venus is yet unfinished.

THE MARKINGS ON MARS.—Observations of Mars more recently published tend to throw doubt upon the "inundation of Libya," which M. Perrotin reported some four or five months ago. Not only were Prof. Schiaparelli and Dr. Terby unable to confirm his statement, but M. Niesten at Brussels, and Prof. Holden at the Lick Observatory, failed to remark this change. The observations of Prof. Holden and his assistants did not begin until July 16, and were continued until August 10. The planet was therefore very unfavourably situated when they were made, since the diameter of the planet was always less than 9", and its zenith distance about 60°. Several of the more important canals were seen, but they were not seen double, but appeared rather "as broad bands covering the spaces on M. Schiaparelli's map which are occupied by pairs of canals, and by the space separating the members of each pair." M. Niesten also seems to have failed to see the gemination of the canals, but, in common with other observers, was much struck by the whiteness and brilliancy of some portions of the planet, particularly of *Elysium* or *Fontana Land*, as it is called by Mr. Green. The brightness of Fontana Land has been commented on both by M. Perrotin and Prof. Schiaparelli, and the former observer has recently delineated an intricate network of canals between that district and the north pole, and another yet more complicated on the *Madler Continent*. Prof. Schiaparelli has had to chronicle still stranger changes in this last-named district, which he observed on May 20 under specially favourable circumstances, having been able to distinguish the two banks of some of the canals, the one from the other, and to detect very small undulations in them. He speaks also of the ordinary markings, of gulfs, canals, &c., as disappearing at a given moment, for their places to be taken by grotesque polygons and geminations "which evidently approximately represent the earlier state; but it is a gross, and, I should say, an almost ridiculous mask."

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *OCTOBER* 21–27.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 21

Sun rises, 6h. 37m.; souths, 11h. 44m. 35·2s.; sets, 16h. 52m.: right asc. on meridian, 13h. 46·0m.; decl. 10° 57′ S. Sidereal Time at Sunset, 18h. 54m.

Moon (Full on October 19, 21h.) rises, 17h. 43m.*; souths, 0h. 40m.; sets, 7h. 50m.: right asc. on meridian, 2h. 39·3m.; decl. 10° 28′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	8 41	12 56	17 11	14 57·7	19 59 S.
Venus ...	9 13	13 30	17 47	15 31·9	19 38 S.
Mars ...	12 11	15 52	19 33	17 54·1	25 2 S.
Jupiter ...	10 10	14 19	18 28	16 20·4	20 58 S.
Saturn ...	23 58*	7 26	14 54	9 26·3	18 58 N.
Uranus...	5 37	11 7	16 37	13 8·5	6 37 S.
Neptune..	18 14*	2 0	9 46	4 0·1	18 50 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich).

Oct.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
23	δ^2 Tauri	6	2 53	3 59	156 274
24	χ^1 Orionis	4½	21 2	21 56	39 264
27	B.A.C. 2854	6	21 59	22 51	46 239

Meteor-Showers.

	R.A.	Decl.	
Near ν Orionis	90°	15° N.	The *Orionids.*
From Canis Minor	105	12 N.	Swift; streaks.
,, Cancer	133	21 N.	Very swift.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·4	81 16 N.	Oct. 21,	2 51 *m*
			,, 25,	2 30 *m*
Algol	3 0·9	40 31 N.	,, 25,	2 51 *m*
			,, 27,	23 40 *m*
S Aurigæ	5 19·7	34 3 N.	,, 24,	*M*
R Canis Majoris	7 14·5	16 12 S.	,, 23,	1 59 *m*
S Hydræ	8 47·7	3 30 N.	,, 23,	*M*
U Ophiuchi	17 10·9	1 20 N.	,, 21,	20 32 *m*
			,, 26,	21 19 *m*
R Scuti	18 41·5	5 50 S.	,, 25,	*m*
η Aquilæ	19 46·8	0 43 N.	,, 24,	9 0 *m*
S Sagittæ	19 50·9	16 20 N.	,, 23,	1 0 *m*
			,, 26,	1 0 *M*
S Delphini	20 37·9	16 41 N.	,, 26,	*M*
T Vulpeculæ	20 46·7	27 50 N.	,, 23,	0 0 *m*
Y Cygni	20 47·6	34 14 N.	,, 23,	3 0 *m*
			,, 26,	3 0 *m*
R Vulpeculæ	20 59·4	23 23 N.	,, 23,	*m*
T Cephei	21 8·1	68 2 N.	,, 23,	*m*
δ Cephei	22 25·0	57 51 N.	,, 23,	23 0 *M*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES.

To the October number of *Petermann's Mitteilungen*, Dr. J. Hann contributes an important paper, containing a *résumé* of data on the temperature and rainfall of the Japanese islands, and Dr. F. Boas a paper of a similar character on the ice conditions of the south-west of Baffin's Bay.

CAPTAIN WIGGINS has failed to accomplish the voyage to the Yenissei along the north coast of Europe and Asia—mainly, it would seem, on account of the delay caused by his having to wait for another vessel from Europe. Dr. Torell, the well-known Swedish Arctic explorer, who is well acquainted with these seas, maintains that there should be no difficulty in establishing a regular communication between Europe and Siberia along the north-east passage, though he admits that it would be liable to interruption about once in five years. But in order to insure success he states that vessels should be built specially for the work, and that they should go out early in summer and take up their post on the west side of Matotshkin Scharr, in Novaya Zemlya, to be ready to enter the Kara Sea as soon as ever it begins to clear of ice. A railway across Siberia, however, should serve to render any such hazardous trade-route unnecessary, and such a railway is sure to be constructed soon.

A CENSUS of the illiterates in the various countries of the world recently published in the *Statistische Monatschrift*, places the three Sclavic States of Roumania, Servia, and Russia, at the head of the list, with about 80 per cent. of the population unable to read and write. Of the Latin-speaking races, Spain heads the list with 63 per cent., followed by Italy with 48 per cent., France and Belgium having about 15 per cent. The illiterates in Hungary number 43 per cent., in Austria 39, and in Ireland 21. In England we find 13 per cent., Holland 10 per cent., United States (white population) 8 per cent., and Scotland 7 per cent. unable to read and write. When we come to the purely Teutonic States, we find a marked reduction in the percentage of illiterates. The highest is in Switzerland, 2·5; in the whole German Empire it is 1 per cent.; in Sweden, Denmark, Bavaria, Baden, and Würtemberg, there is practically no one who cannot read and write.

IN the October number of the Proceedings of the Royal Geographical Society, the Shah of Persia appears as a geographer. In a paper, annotated by General Houtum-Schindler, His Majesty describes simply, but clearly, the results of his own observations on a new lake, between Kom and Teheran, or rather the reappearance of an old lake, which is said to have dried up in 1357. Whatever may be the history of the lake, there seems little doubt that at one time a large part of Central Persia was covered with water. Mr. H. H. Johnston contributes a short study, from his own observations, of what he calls the Bantu Borderland in West Africa, which is accompanied by a map showing the boundaries of the Bantu and Semi-Bantu races, and also the courses of migration of the two. Another important paper, accompanied by a map, is a translation, by Miss Hay, of Tashkent, of a description of the destructive earthquakes of May and June 1887, in the Vernoe district of Russian Turkestan. Captain Wharton's paper on Christmas Island is given at length.

NOTES ON METEORITES.[1]

V.

We shall see next that another line of thought and inquiry was required to completely establish the cosmical hypothesis by giving us data as to the *velocities* of the meteorites.

This was that the sporadic meteors, those which made their appearance by chance, so to speak, were always more numerous in the morning than in the evening hours, and further that the numbers seen in the northern hemisphere in one half year was greater than that seen in the other. These facts, although at first they seemed to connect these phenomena with our terrestrial hours, and therefore were at first considered to militate against the cosmical hypothesis, were subsequently shown, by Bompas, A. S. Herschel, H. A. Newton, and Schiaparelli, to be a distinct proof that the bodies were moving in space with a velocity not incomparable with, but at the same time somewhat greater than, that of the earth itself; that therefore they were moving with planetary velocities, and therefore were truly members of the solar system.

The work of M. Coulvier-Gravier[2] was the first to indicate the extreme regularity with which the numbers increased from sunset to sunrise, as will be seen in the accompanying table:—

Time of Observation.	Number seen per hour.	Time of Observation.	Number seen per hour.
5 p.m.– 6 p.m.	7·2	12 –1 a.m.	10·7
6 p.m.– 7 p.m.	6·5	1 a.m.–2 a.m.	13·1
7 p.m.– 8 p.m.	7·0	2 a.m.–3 a.m.	16·8
8 p.m.– 9 p.m.	6·3	3 a.m.–4 a.m.	15·6
9 p.m.–10 p.m.	7·9	4 a.m.–5 a.m.	13·8
10 p.m.–11 p.m.	8·0	5 a.m.–6 a.m.	13·7
11 p.m.–12	9·5	6 a.m.–7 a.m.	13·0

It was the dependence of these phenomena upon certain terrestrial hours which made that eminent observer decline to consider their origin as in any way cosmical.

Mr. Bompas,[3] commenting on the numbers obtained by Coulvier-Gravier, wrote—

"The part of the heavens towards which the earth is moving at any time is always six hours from the sun. At 6 a.m. the observer's meridian is in the direction of the earth's motion; and at 6 p.m. in the opposite.

"Thus the greatest number of meteors are encountered when the observer's meridian is in the direction of the earth's motion, and the number diminishes from thence to 6 p.m., when he looks the opposite way."

The accompanying wood-cut will make this clear. The front half of the earth ploughing its way through space is unshaded; an observer is being carried along the line of the earth's motion at sunrise, the earth is behind him, so to speak, and the point towards which the earth is travelling lies 90° in longitude behind the sun.

Combining these facts, Bompas explained the results on the principle that if the meteors be distributed equally in space they would converge to the earth, if at rest, equally on all sides. But if the earth be in motion, and with a velocity one-half the average velocity of the meteors, they would converge to it more on the side towards which it is moving than the other: and in the proportion of nearly two-thirds of the number, would have an apparent motion more or less opposed to that of the earth, and apparently diverging from the point towards which the earth is moving, with a gradual increase in number from 6 p.m. to 6 a.m.

Before we proceed to show the bearing of this matter, a word must be said with regard to the actual conditions under which these bodies reach us from space, and how the fall of these bodies upon the earth and their appearance in the heavens even in the case of no fall have been investigated.

To approach the proof of the cosmical hypothesis afforded by these observations, we may begin by supposing the earth at rest. If the movements of the cosmical particles are in all directions, they will fall equally on all parts of the earth, and even the earth's *rotation* will make no difference. But if we assume the earth's movement in its orbit to be much more rapid than the movements of the meteorites, it is clear that its forward half will receive blows while the hinder half cannot.

Suppose that all the regions of space swept through by the earth in its orbit round the sun were occupied here and there by meteorites, also like the earth moving in orbits round the sun, and let us assume for the moment that they are pretty nearly equally distributed and are moving in all directions.

Under these circumstances the earth in movement in its orbit, at the rate of about 1000 miles a minute, would be sweeping through them all the year round, and we should get the appearance of a shooting-star or the fall of a meteorite every day in the year. Careful observations in climates most convenient for these researches, where the sky is freest from cloud and is purest, show, as we have seen, that there is not only no night but no hour without a falling star. We are therefore justified in considering that practically the part of the solar system which is swept through by the earth is not a vacuum, not empty space, but space peopled with meteorites here and there.

If these meteoritic bodies are equally distributed and are going in the same direction as the earth, but moving more quickly, they would follow and catch the earth; if they were travelling in the same direction as the earth, but more slowly, we should overtake them, and the two sides of the earth separated by a plane at

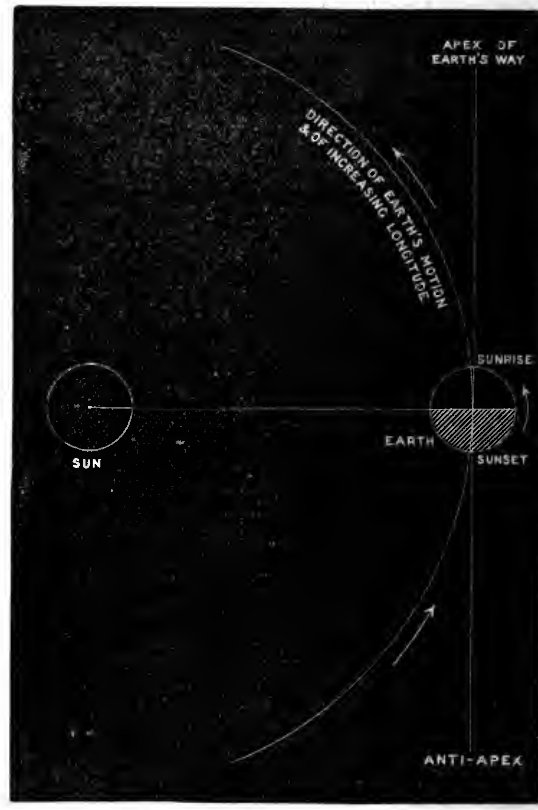


FIG. 10.

right angles to the tangent to the part of the orbit along which it is moving at the time (see Fig. 10) would experience a different condition. One side would be bombarded by the greater number of meteorites in the former case, while in the latter the forward half only would be affected. The assumption, however, is that they are travelling in all directions; hence the numbers which fall on the front hemisphere compared with those that fall on the opposite one—in other words, the numbers seen at sunrise as compared with those seen at sunset—must depend wholly on the velocity of the earth as compared with the mean velocity of the meteorites.

The point of space towards which the earth is travelling at any moment, shown in Fig. 10, has been called "the apex of the earth's way"; the point of space it is leaving the "anti-apex."[1]

[1] Continued from p. 559.
[2] "Recherches sur les Météores," p. 219 (Paris, 1859).
[3] *Monthly Notices*, vol. xvii. p. 148.

[1] These terms were suggested by Prof. Pritchard. In 1866, Schiaparelli suggested *point de mire*. Quite recently, Prof. Newton, of Yale, has suggested "goal" and "quit."

The apex of the earth's way is always 90° of longitude behind the sun.

Having, then, this general view of the movement of the earth in her orbit, we are in a position to discuss Mr. Bompas's argument, and we cannot do better than use the explanation given by Prof. Pritchard to the Royal Astronomical Society in 1864,[1] which really possesses an historical interest.

"Our object is to show that this hypothetical uniformity of distribution, combined with the direction and amount of the earth's motion, will have a very sensible effect on the number of meteors actually visible at a given place, at a given hour of the night, as explained (in a somewhat different way) by Mr. Bompas [and at a given season of the year, as extended by Mr. Herschel].

"For the purpose of illustration, suppose H O R (Fig. 11) to represent a flat umbrella, of which N O is the stick; suppose, also, rain to fall upon it equally, and in all directions: then, if the umbrella be at rest, as much rain will fall upon its front, looking from Z, as on its back, from N.

"But now suppose the umbrella itself has a motion from O to E in a given time, and, for the simplicity of first conception, let O E represent also the uniform velocity of the rain: very much more rain will now fall on the front of H O R, and much less on the back of H O R, than before. In fact, if O *m* be taken = O E,

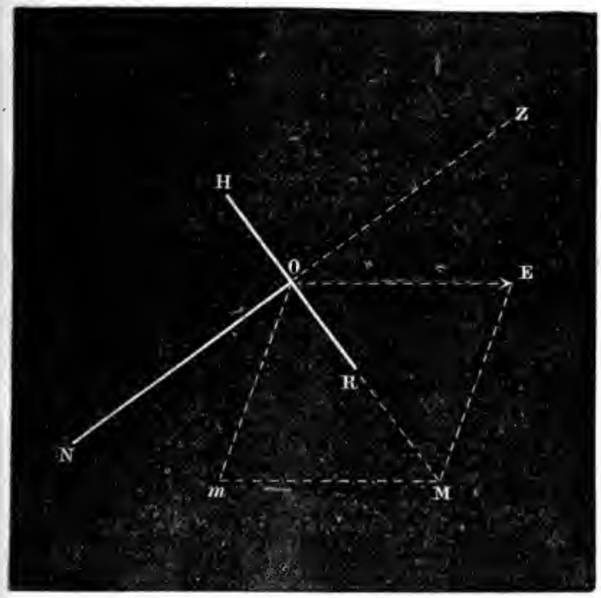


FIG. 11.

and the angle *m* O R be made = R O E, and the parallelogram *m* E be completed, then a raindrop, of which *m*'s real path is M E, would, by the motion of H O R, just graze along the front surface of H O R in the direction M R O, when it arrives at E. Moreover, all the rain which at the beginning of the motion was moving within the angle *m* O R, which would have fallen on the back of H O R at rest, will now fall on the front of H O R, if in motion.

"The application of this hypothetical case to that of meteors is obvious. H O R now represents the horizon, Z the zenith of an observer, and O E the direction and magnitude of the earth's orbital motion. The earth's diurnal motion of rotation is comparatively too small to be taken into account for our present purposes. So long, then, as O E, the direction of the earth's orbital motion, is in front of the horizon of an observer, there will thereby occur to him an additional flow (and partial combustion) of meteors against the earth's atmosphere above him; and this increased flow will become the greater as the angle R O E becomes greater. If O E be below H O R, then the number of visible meteors will thereby be diminished."

Now, if we refer to Fig. 10 we shall see that the observer does not reach the forward part of the earth (with reference to the apex of the earth's way) till midnight, and that the apex rises gradually till it is on his meridian at sunrise.

[1] *Monthly Notices*, 1864, vol. xxiv. p. 133.

Here, then, is the reason why the number increases from sunset to sunrise, based upon the theory of their cosmical origin, and really explainable in no other way.

Now for the yearly conditions as revealed by observation.

Dr. Julius Schmidt, the Director of the Observatory at Athens, observed, between the north latitudes of 49°·5 and 54°·2, during eight years from 1843 to 1850, on an average 470 meteors in every year. These were distributed among the several months as follows, taking an average of the entire series:—

Month.	Shooting-stars.	Total Shooting-stars.
July	49	
August	188	
September	41	... 400
October	37	
November	54	
December	31	
January	17	
February	5	
March	11	... 70
April	11	
May	12	
June	14	
	Total ...	470

Prof. A. Herschel was the first to point out that this yearly difference, as well as the daily difference in the hourly numbers, arrived at by Coulvier-Gravier, demonstrated the cosmical origin.

In 1864 he wrote as follows, assuming that the meteorites travelled faster than the earth:—[1]

"A season of frequency of aërolites, shooting-stars, and bolides, must be expected to succeed, in all latitudes, three months later than the summer season of the sun; but, on the other hand, a dearth of meteors, in the spring, one quarter of a year later than mid-winter. In general, and in all latitudes, the meteoric seasons, or seasons of meteoric frequency, must strictly follow the tropical seasons, and three months later in the year. Thus, in the earth's northern hemisphere, the Northern Pole remains directed to the sun from the equinox of March until that of September, and to the course of meteors from the solstice of June to the solstice of December. The greatest frequency of the meteorites will fall about the equinox of autumn, in September and October. This most nearly agrees with the European observations. The meteoric season of Arago may, therefore, be drawn as a consequence from his planetary hypothesis, if it be permitted to change the limits which he assigns to it by a small quantity—namely, from the Earth's apsides to its solstices in its orbit.

"The same fact, which appears strongly marked with regard to shooting-stars in the eight years' summary of Dr. Schmidt, is found repeated in a striking manner in the existing 'Northern Catalogues of Star-showers, Fire-balls, and Aëroliths.' The following references may be taken as examples:—

Appearances.	Number—July to December.	Number—January to June.
Star-showers from 1800 B.C. In M. Quetelet's catalogue ("Physique du Globe," 1861)	72	28
Aerolitic meteors, from the Christian era. In Mr. Greg's catalogue (British Assoc. Report, 1860) . .	216	186½
Large and small fire-balls. In same catalogue (*ibid.*) .	843	553."

It was now pointed out, by Newton[2] and Schiaparelli,[3] that, provided the actual facts of the daily and yearly variation were sufficiently assured, the true velocities of these bodies in space could not be just simply similar to the earth's velocity, nor their paths in space planetary orbits like that of the earth, and of about the same dimensions; but that as their motion was much faster their orbits would be variously distributed parabolas, and they would consequently be more akin to comets.

That the movement was really much faster was argued in

[1] *Monthly Notices*, vol. xxiv.
[2] *Silliman's Journal*, vol. xxxix., 1865; *Nat. Acad. Sci.*, vol. i.; *Annuaire de l'Observatoire de Bruxelles*, 1876, p. 201.
[3] *Les Mondes*, vol. xiii.

1865, from the duration of the flight of shooting-stars, by Prof. Newton.[1]

From Wartmann's observations of the duration of the flights of 368 shooting-stars at Geneva during one night by six observers, a mean was found of 0·49s. for each flight. The mean of 499 estimates made in August and November 1864 is 0·418s. The mean duration of the 867 flights is 0·45s.

Prof. Newton remarks:—"A mean duration of half a second, and a mean length of path between 39 and 65 kilometres, imply a mean velocity between 78 and 130 kilometres per second. The smallest of these (more than 48 miles) is twice and a half the velocity of the earth in its orbit about the sun. This cannot consist with the supposition that most of the meteoroids move in closed orbits about the sun."

Both the briefness, however, of this assumed duration, and even the least limit, accordingly, of the velocity so found, were presumed by Prof. Newton to be probably overrated.

The final step in this demonstration was taken by Schiaparelli, but before this Newton had distinctly shown that most of the meteors visible were not single in their movements round the sun, but that they belonged to systematic streams and that these streams were not rings.

With special reference to the November ring, Prof. Schiaparelli[2] came to the conclusion that the orbit, instead of being nearly circular, as Newton had at first supposed, was very elongated, like those of comets; and Prof. Adams[3] demonstrated shortly afterwards that, among several possible periods of the stream which Prof. Newton had already indicated, the true period was 33·25 years, the demonstration depending upon the increase of the longitude of the node by the action of the planets Jupiter, Saturn, and Uranus, the calculated increase amounting to 28′, while the actual increase was 29′, and he gave the following elements of the orbit of the swarm—

Period	33·25 years (assumed)
Mean distance	10·3402
Eccentricity	0·9047
Perihelion distance...	0·9855
Inclination	16° 46′
Longitude of node	57·28
Distance of perihelion from node...	6·51

Motion retrograde.

Aided by considerations suggested by observations of the conditions under which the meteors were observed—from a particular part of the sky, in a particular part of the earth's orbit, at a particular time and from a particular point of the earth's surface, we can understand at once that it was as practicable to determine the orbit of the swarm as it is to determine the orbit of a planet or of a comet.

The final step taken by Schiaparelli, to which we have referred, was a demonstration that the orbits of certain of these streams or swarms, to which reference has been made, were really identical with the elements of known comets.

Schiaparelli computed the elements of the orbit of the August meteors, supposing them to be moving along a cometary or parabolic orbit. For his calculations the data were the radiant in R.A. 44°, N. Decl. 56°, the time of the earth passing near the centre of the group in 1866, August 10·75. With the elements thus obtained he found those of the comet 1862 III., according to the latest determinations by Oppolzer,[4] to be nearly identical, as is seen in the following statement:—

	Elements of August Meteors.		Elements of Comet 1862 III.
	° ′		° ′
Long. of perihelion	343 38	...	344 41
Long. of node	138 16	...	137 27
Inclination	64 3	...	66 25
Perihelion distance	0·9643	...	0·9626
Motion	retrograde	...	retrograde
Perihelion passage	July 23·62	...	Aug. 22·9, 1860
Period			123·4 years

As remarked by Prof. Newton,[5] we come thus to the unexpected conclusion *that the comet of 1862 is nothing else than one of the August meteoroids, and probably the largest of them all.*

[1] *Silliman's Journal*, vol. xxxix. p. 203.
[2] *Bulletino Meteorologico dell' Osservatorio del Collegio Romano*, vol. v. 1866.
[3] *Monthly Notices*, vol. xxvi. p. 247, April 1867.
[4] *Astr. Nach.*, No. 1384.
[5] *Silliman's Journal*, vol. xliii., 1867.

When this relation of the comet of 1862 with the August meteors was discovered by Schiaparelli, no comet was known having similar relations with the November meteors. Oppolzer, however, shortly after,[1] published a corrected orbit of comet 1866 I., and the resemblance of its elements to those of the orbit of the November group was at once obvious, and attracted the attention of several astronomers.[2] The following table gives the details:—[3]

	Nov. Meteors.		Comet 1861 I.
Perihelion passage ...	Nov. 10·092, 1866	...	Jan. 11·160, 1866
Passage of descending node	,, 13·576		
	° ′		° ′
Long. of Perih. ...	56 25·9	...	60 28·0
,, ,, asc. node ...	231 28·2	...	231 26·1
Inclination	17 44·5	...	17 18·1
Perihelion distance ...	0·9873	...	0·9765
Eccentricity	0·9046	...	0·9054
Semi-major axis ...	10·340	...	10·324
Periodic time	33·250	...	33·176
Motion	retrograde	...	retrograde

Since this discovery of Schiaparelli's, one by one the various star showers have been shown to be due to meteorite swarms pursuing generally elliptic orbits round the sun, which orbits are identical with those of various known comets. Hence each "radiant point" is already, or will subsequently be, associated with a comet.

Distribution of Meteorites in the Solar System.

The *vide planétaire* is now ultimately abolished, and we find the solar system to be a meteoritic *plenum* in which sporadic meteorites and swarms of greater or less density are moving in orbits more or less elongated round the sun.

The demonstration that meteorites are extra-terrestrial bodies has been followed by researches which, as they have become more complete and searching, have gradually driven men of science to increase their estimates, till at last the numbers acknowledged to exist in what was formerly supposed to be empty space have become enormous.

First as to the sporadic meteorites.

Observations of sporadic falling stars have been used to determine the average number of meteorites which attempt to pierce the earth's atmosphere during each twenty-four hours. Dr. Schmidt, of Athens, from observations made during seventeen years, found that the mean hourly number of luminous meteors visible on a clear moonless night by one observer was fourteen, taking the time of observation from midnight to 1 a.m.

It has been further experimentally shown that a large group of observers who might include the whole horizon in their observations would see about six times as many as are visible to one eye. Prof. H. A. Newton and others have calculated that making all proper corrections the number which might be visible over the whole earth would be a little greater than 10,000 times as many as could be seen at one place. From this we gather that not less than 20,000,000 luminous meteors fall upon our planet daily, each of which in a dark clear night would present us with the well-known phenomenon of a shooting-star.

This number, however, by no means represents the total number of sporadic meteorites that enter our atmosphere, because many entirely invisible to the naked eye are often seen in telescopes. It has been suggested that the number of meteorites if these were included would be increased at least twenty-fold; this would give us 400,000,000 of meteorites falling in the earth's atmosphere daily.

If we consider only those meteorites visible to the *naked eye* as sporadic meteors or falling stars, and if we further assume that their absolute velocity in space is equal to that of comets moving in parabolic orbits, Prof. H. A. Newton has shown that the average number of meteorites in the space that the earth traverses is in each volume equal to the earth about 30,000. This gives us as a result in round numbers that the meteorites are distributed each 250 miles away from its neighbours.[4]

[1] *Astr. Nach.*, No. 1624.
[2] Peters, *Astr. Nach.*, No. 1624; Oppolzer, *ibid.*, No. 1626; Schiaparelli, *ibid.*
[3] *Bulletino Meteor.*, February 28, 1867.
[4] Article "Meteorites," Prof. Newton, "Encyclopædia Britannica," 9th edition, vol. xvi.; and "Abstract of a Memoir on Shooting-Stars," by Prof. Newton (*Silliman's Journal*, vol. xxxix., 1865).

Next as to systematic meteorites, those, that is, that are massed in swarms.

Much still remains to be done before their greater density is known. Prof. Newton has calculated that in the Biela swarm the meteorites are thirty miles apart.

J. NORMAN LOCKYER.

(*To be continued.*)

DR. JANSSEN ON THE SPECTRUM OF OXYGEN.

THE following is an abstract of the account given by M. Janssen, in Section A of the British Association, of his researches into the different forms of oxygen, in the direction of an inquiry into the molecular constitution of that element. These experiments have been made in the laboratory which has been organized under Dr. Janssen's supervision, and at the expense of the French Government, at Meudon. The hall in which the observations have been carried out is 100 metres in length. It contains every requisite for studying the optical properties of gases; principally instruments so constructed that a long column of gas may be examined under a high pressure. One of these is a set of steel tubes varying in length from 0·42 metres to 60 metres, terminated at each end by a glass plate, perpendicular to their axes, and constructed to resist a pressure of 200 atmospheres. The chief result of this work was the discovery of a new law of the selective absorption by oxygen of any beam of light, quite independently of its origin, whether from the sun or the electric light. It was proved that oxygen produces two kinds of absorption-phenomena on the spectrum of the light—first, the known rays; and, secondly, a system of dark bands which had not, up to this time, been noticed. M. Janssen has demonstrated that the intensity of the rays varies as the products of the length of the column into the density; while that of the bands varies as the products of the length of the column into the square of the density. The principal results obtained by M. Janssen are best displayed in the following table:—

1. Metres.	2. Atmospheres.	3. Atmospheres.	4. Atmospheres.
60	6	6	6
20	10 to 12	10·4	18
5	23	20·7	72
1·47	38	38·3	240
0·75	50 to 55	53·6	480
0·42	70 to 75	71·7	858

1. Length of the tube.
2. Pressure observed.
3. Pressure calculated by formula Ld^2 (product of length of column into the square of the density).
4. Pressure calculated by the formula Ld.

These numbers are fixed by the point at which the band in the yellow first appears, this phenomenon supplying the standard term of comparison. It is easy to see how nearly the observed results in the second column agree with the figures in the third, and how far they differ from those in the fourth. The law of the square has been discovered by an analytical method, which will be published in full in the Proceedings of the British Association. Dr. Janssen has proved the exactness of this law in its application to the oxygen contained in the atmosphere, in measuring the altitude of the sun necessary for the first appearance of the band. He verified the same law by experiments on oxygen in its liquid state, and found that a thickness of 4 to 5 millimetres was sufficient. The correctness of this law must be considered as valid from 0 to 700 atmospheres. For the flutings of the group B the law of variation according to the formula Ld has been verified from 0 to 100 atmospheres by direct observation of the tubes. It is curious to notice how by the systematic variation of length of column and density it is possible to obtain either lines without bands, bands without lines, or bands and lines together. Among the astronomical applications of this law it is noted that a nebula which might have a diameter of 2000 times the distance of the earth from the sun, containing oxygen at a density of one-millionth of an atmosphere, could be traversed by the light of a star without causing the appearance of oxygen-bands in the spectrum. M. Janssen stated that he is still pursuing these investigations, and others attendant thereon, relative to the molecular construction of oxygen and its presence in the atmosphere of the planets.

At the conclusion of Dr. Janssen's paper, Sir Wm. Thomson recapitulated the main facts to the audience, stating his opinion that the discovery of the law of the square of the density was a most brilliant achievement.

THE GROWTH OF ROOT-CROPS.[1]

THIS is a pamphlet of extremely closely written matter, which purports to be a lecture delivered on July 27, 1887, to agricultural students in Cirencester College. Viewing it as a lecture we should accord it qualified praise, because a lecture must be regarded as oral instruction, and ought to be sufficiently dilute and sufficiently *moist* to allow of the process of mental deglutition. The pamphlet is really a treatise upon the effect of fertilizers on the growth of roots and their composition, and it would be presumption on our part to do more than bow respectful acquiescence to each statement made by so learned and so experienced a specialist.

Dr. Gilbert has studied turnips ever since 1843, and probably long before then, and his knowledge of their habits, their requirements, and their uses, is unequalled by that of anyone else in this country. Anyone who will read through the pamphlet now before us will find his ideas with regard to these esculents enlarged and dignified. Dr. Gilbert chiefly treats his subject from a chemical point of view—the fertilizers best suited for producing a crop, and the composition of the crop after it is grown. The extraordinary dependence of the turnip upon artificial help is shown by many tables, and the erroneous idea that the turnip acts as a *renovator* or restorer of fertility is exposed and disproved. If any crop is capable of completely exhausting a soil of all its available fertility, it is a turnip crop manured with superphosphate of lime. So far from being a renovator it is a waster. Still, circumstances control cases, and the special circumstances which accompany turnip cultivation are of an ameliorating sort. True, if your turnip is sold off the farm it may be looked upon by the landowner as a burglar making off with his goods and chattels, but consumed "on the premises" it yields up its wealth and becomes beneficent. Like John Barleycorn, it springs up again after ever such rough usage, and its spirit lives in succeeding corn crops.

The superiority of swedes over turnips is shown by the much smaller proportion of leaf existing in them in comparison with white turnips; and also in the larger proportion of dry matter in the root. White turnips, especially when dressed with nitrogenous matter, gave 600 parts in weight of leaf to 1000 of root. Swedes gave under similar circumstances 228 parts of leaf to 1000 of root. White turnips were found to contain from 7·66 to 8·54 per cent. of dry matter, while swedes contained from 10·83 to 12·04 per cent. of dry matter. In both swedes and turnips the effect of superphosphate of lime in increasing the crop is remarkable when there is a sufficient stock of nitrogen in the soil. A single crop will, however, deplenish the excess of nitrogen, and fresh applications of superphosphate will not act with the same energy. Take, for example, the series of root-crops grown in rotation with other crops, but recurring at intervals of four years in 1848, 1852, 1856, &c. The portion unmanured yielded 9 tons per acre the first year, but the fifth, ninth, thirteenth, and seventeenth, it only yielded from helf a ton to one ton per per acre. Similarly, superphosphate gave a crop of 14½ tons in 1848, and of 11 tons in 1852; but in 1856, 1860, and 1864, the yields produced by the same top dressing varied from 1½ to 6¾ tons per acre. In no crop more than in the turnip crop is a full supply of nitrogenous and mineral plant foods more essential, and hence the importance of farm-yard manure for its thorough development.

But the most interesting portion of the lecture is the second part, in which the effect of fertilizers upon the proportion of sugar and albuminoids in root-crops is dealt with. The effect of nitrogenous dressings in increasing the power of the plant to take carbon from the air, and especially to elaborate it into sugar, is much enforced. It is, however, evident that the effect of the nitrogenous manure, especially in the case of mangel-wurzel, consists in increasing the crop, and the crop being increased the amount of sugar and dry matter generally, will naturally increase also. So far indeed as percentage goes, it is higher where no nitrogenous manure is used than in any other cases. In fact, wherever nitrogenous manures are employed, the percentage of sugar is

[1] "The Growth of Root-Crops," by J. H. Gilbert, M.A., LL.D., F.R.S., Sibthorpian Professor in the University of Oxford.

mmediately reduced. The great increase in the actual weight of the crop treated with nitrogenous manures, however, completely overrides percentages, and hence the table showing the effect of nitrogenous manures records a great increase of sugar, corresponding with the application of nitrogenous fertilizers. Dr. Gilbert says: "I cannot discuss the physiological explanations of the fact that nitrogenous manures have such a marked effect on the production of the non-nitrogenous substance—sugar."

It would also be an interesting physiological question why the percentage of sugar is highest when no nitrogenous manure is applied, and also why nitrogen, even in the form of farm-yard manure, appears to at once lower the proportion of sugar in mangel. Also, why further additions of nitrogen still further lower the percentage of sugar. The percentages stand as follows:—

Sugar, per cent. (in mangel-wurzel).

No manure	11·4 per cent.
Superphosphate...	10·4 ,,
Farm-yard manure	8·6 ,,
Farm-yard manure and sodium nitrate	7·1 ,,

The actual quantities of sugar per acre stand as follows, in pounds:—

No manure	950 pounds per acre.
Superphosphate	1028 ,, ,,
Farm-yard manure	2513 ,, ,,
Farm-yard manure and sodium nitrate...	3109 ,, ,,

Judged by *percentages* we have a descending series, but judged by actual quantities an ascending series of figures. It is somewhat difficult in the face of the diminishing percentages of sugar caused by the application of nitrogenous manures, to see how the functional powers of the plant to make sugar have been heightened or intensified. Still, Dr. Gilbert says: "A direct connection between the supply of nitrogen to the plant and the formation of non-nitrogenous substances is obvious." Might it not be as truly said, "A direct connection between the weight of the crop and the weight of non-nitrogenous substances contained in the crop is obvious"?

We have received a copy of memoranda of the origin, plan, and results of the Rothamsted field and other experiments, which gives an excellent idea as to the work carried on by Sir John Lawes on his Hertfordshire property. Sir John began to experiment on growing crops in 1837, but fixes the actual commencement of the Rothamsted Station in 1843, when he associated Dr. Gilbert with himself in carrying out a magnificent series of agricultural experiments. A large staff of chemists and assistants are employed entirely at Sir John's own cost, and he has provided for the continuance of the work after his death by setting apart £100,000 for the purpose as well as sufficient land for carrying out his intentions. It is pleasant to find Sir John Lawes and his indefatigable coadjutor Dr. J. H. Gilbert still young in mind and constitution, and able to throw all their old ardour into their work.

FLETCHER'S COMPRESSED OXYGEN FURNACE.

THE use of oxygen with coal-gas in a laboratory furnace has up to the present been attended with serious difficulties, owing to the intensely local nature of the heat obtained, and the consequent perforation and destruction of crucibles and other vessels.

In this furnace, diffusion of the heat is secured by using a fine jet of Brin's compressed oxygen directed centrally into one end of a tube a quarter of an inch in bore, open at both ends, the oxygen jet acting as an injector, and drawing with it from four to eight times its bulk of air, the proportion depending on the size of the oxygen jet. This tube, containing the mixture of oxygen and air, is used as the central part of an ordinary blowpipe of heavy cast-iron, which is placed close up against the burner-opening of one of Fletcher's ordinary injector furnaces, lined with a specially refractory material.

The power of the furnace depends entirely on the quantity of oxygen and gas supplied, and can be adjusted to any power from a dull red, which can be maintained for many hours steadily, without attention, to a heat which will "drop" the most refractory crucible in less than five minutes from the time the gas is lighted.

When working at moderate temperatures, the furnace is sufficiently quiet to admit of its use on a lecture-table, but at its highest power the noise is considerable.

There is no difficulty in adapting the burner to other forms of furnace, provided it is found possible to produce satisfactory casings to withstand the heat; those made for the crucible furnace stand, as a rule, exceedingly well, but with alterations in form great difficulties are introduced, more especially with muffles, which, as at present made, will not stand any sudden

heat, nor will they hold their shape at any temperature approaching whiteness. The burner alone will be useful in heating many substances in the open, but, owing to the broad and diffused flame, it is of little practical value for blow-pipe work.

The special advantages of the apparatus are that it is entirely self-acting, requires no attendance, and that it greatly increases the range of temperatures which can be obtained by any simple apparatus. The largest size at present made takes crucibles not exceeding 3 inches high.

FOREST CONSERVANCY IN CEYLON.

COLONEL CLARKE, the Acting-Conservator of Forests in Ceylon, in his Report for last year says that since attention was called in 1873 to the gradual destruction of forests in Ceylon efforts have been made to check the evil. At first the expense was the great obstacle. The Government did not see its way to expend the large sums that would be necessary before the forests could be regarded as self-supporting. However, in 1885, "The Forest Ordinance" was passed, under which certain areas of forest lands were acquired by the State and made State forests, the owners of those areas or persons having any interest in them being compensated for the loss of their rights. These tracts were to be clearly marked out, and, where necessary, replanted and improved. It is yet too soon to say what the effects will be of this systematic treatment, but the Government hopes that a constant supply of good timber will be at hand, and that the climate of the island will be benefited by increased care of the forests. Forests, Colonel Clarke says, make the climate more equable, increase the relative humidity of the air, and perhaps increase the rainfall. Furthermore, the water-supply is regulated by forests, the springs being more regular and sustained, and the rivers more continuous in their flow. Adjacent fields are protected by them and the speed of the wind is reduced. In tropical countries especially, where, during the wet season, the rain falls in torrents, forests are useful in preventing the soil from being washed away into the rivers and bays. Besides, it is confidently expected that a substantial revenue will be derived from the sale of timber, fuel, &c. India, which, relatively speaking, has not more valuable forests than Ceylon, yielded in the year 1883-84 a gross revenue of £1,052,190, representing a clear profit of £403,815. In the past the native forest-keepers connived with gangs of natives who plundered the forests and deprived the island of the revenue that would otherwise have accrued. The evil effect of the destruction of forests that was so very common until quite recently in every quarter of the globe, is apparent everywhere. Some striking instances were given in 1885 before the Select Committee of the House of Commons on Forestry. For example, what was fifty years ago the great rice-producing district of the west of India, Ratnagiri, has suffered terribly from the denudation of the Western Ghats of the dense forests which extended all over that range of mountains. Again, the native State of Jinjira was all but ruined by the indiscriminate felling of the forests which covered the whole State, which is from fifteen to a hundred miles in breadth, and about forty in length. Similarly, in Ceylon itself, the chena cultivator in the Southern and North-Western Provinces and in the Province of Uva is threatened with ruin.

The recommendations made by Colonel Clarke in 1887, and approved of by Government were the following:—The Government Agent and the Conservator of Forests were annually, subject to the approval of the Government, to agree on what works were to be accomplished in the way of demarcation, conservation, &c., and these were to be carried out by the Provincial Forester under the authority and protection of the Government Agent. In departmental questions, such as those relating to pay, promotion, discipline, and other matters, the Conservator of Forests was to be supreme. The present mode of working is illustrated by the plan of operations for this year, drawn up by Colonel Clarke, and sanctioned by the Government in March last. The plan is drawn up under four heads: (1) demarcation; (2) timber and firewood supply; (3) re-afforestation; (4) extra establishments. With regard to demarcation it was seen that this was urgently needed in the neighbourhood of the large towns, and Government was, therefore, recommended to allow the whole available staff to be placed at this work. The forests in the northern, eastern, and north-central provinces were to be allowed to take care of themselves for a time, as the population was very sparse in those regions. Thus it was proposed to begin at once with the Mitirigala and Kananpella forests, which lie in the vicinity of Colombo and on the banks of the Kelani. The present system, by which contractors cut timber for the Public Works Department, is to be changed, for no sufficient check can be exercised over the contractors and their workmen, and it is intended to establish depots in various centres where it is considered that there will be sufficient demand for timber and firewood. When this is done, not only will the heavier timber be utilized as at present, but also the lighter portions which are now left to rot in the forests. Two great depots are to be established, one on the east coast and one at Colombo. To the latter will be sent all the timber that is intended for export, such as ebony, satinwood, &c., and to the other depot those timbers which are in demand in India, but which would not bear the cost of transit to Colombo. According to the Report ten depots in all will be established this year. An effort will be made to give the forests of Ceylon a trial for railway sleepers. Colonel Clarke says that the local demand should be met, as two trees which are very plentiful in the island are, in his opinion, suitable for that purpose, Palai (*Mimusops Indica*) and Kumbuk (*Terminalia glabra*). Re-afforestation, in Colonel Clarke's opinion, is not a pressing question; demarcation should first be completed. Many of the Ceylon forests, he thinks, are overworked, and require a long period of rest. To carry out the works now absolutely necessary for the protection of the forests, the staff is to be increased by adding forest-rangers and river-guards.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The list of lectures in Physics this term includes Prof. Stokes's on Physical Optics, Prof. Thomson's on the Properties of Matter and on Mathematics for Students of Physics, and Mr. Wilberforce's on Dynamo-electric Machines. Among the numerous chemical lectures we do not note any very novel feature. Prof. Newton will lecture on the Evolution of the Animal Kingdom, and Mr. Gadow on the Morphology of the Ichthyopsida, recent and extinct. In Botany, the Readership has not yet been filled up; Mr. Gardiner is giving a general elementary course, Mr. Potter is lecturing on the Geographical Distribution of Plants, and Mr. Vaizey on the Classification of Plants. In Geology, Mr. Marr lectures on the Principles and on Advanced Stratigraphy, Mr. Harker on Petrology, Mr. Roberts on Advanced Palæontology, and Mr. Seward on Palæobotany. The physiological and anatomical courses are much as usual. There are three (graduated) sets of demonstration classes in Mechanism, and lectures by Prof. Stuart and Mr. Lyon. In Mathematics, Prof. Cayley is lecturing on Elliptic Functions, Prof. Darwin on Orbits and Perturbations of Planets, Mr. Pendlebury on the Theory of Numbers, Mr. Hobson on Fourier's Series and on Conduction of Heat, Mr. Larmor on Electrostatics, Mr. Forsyth on Theory of Functions, Dr. Besant on Analysis, Dr. Glaisher on Elliptic Functions, and Mr. Herman on Hydrodynamics.

At Sidney Sussex College, an examination for Open Scholarships in Natural Science will be held on January 1 next; two are offered, one of £70 and one of £40; subjects—Chemistry, Physics, Biology, and Geology. The Tutor will give further particulars on application.

King's College offers one Exhibition for Natural Science; examination to begin about December 10.

Emmanuel, Jesus, and Christ's Colleges will hold joint examinations for Open Scholarships on December 11 and following days. All candidates must show a competent knowledge of Chemistry. Candidates may also be examined in Physics, in Elementary Biology, and in Geology. The Tutors will give full particulars.

SCIENTIFIC SERIALS.

Bulletin de la Société de Naturalistes de Moscou, 1888, No. 2.—On the development of Amphipods, by Dr. Sophie Pereyaslavtseva.—List of plants of Tambof, by Litvinoff.—On the great comet of 1887, by Th. Bredichin (in French).—Short notes on some Russian species of *Blaps*, by E. Ballion (in German).—On the Mollusks of Caucasia, by O. Retowski. Twenty-nine species from Novorossiisk, and ten from Abhasia are described (in German).—The *Chlorophyceæ* of the neighbourhood of Kharkoff, by D. B. Ryabinin. Until now, this subdivision of Algæ has been rather neglected in Russia, and only 100 species have been described in the neighbourhood of Moscow. M. Ryabinin's list comprises 233 species, belonging to 74 different genera (with notes in French).—Materials for the flora of Moscow, by Prof. Gorojankin (in Russian). The capital work of the late Prof. Kaufmann, "The Flora of Moscow," which was published in 1866, has been revised by M. Petunnikoff, who compared it with the rich materials of the Moscow Botanical Garden, and published a supplementary list. Students of the Moscow University having been directed during the last three years to collect new materials during special excursions, Prof. Gorojankin has availed himself of all their collections, as well as of a dozen other collections, and now publishes a new supplementary list, which contains 102 new species of Phanerogams and two species of Cryptogams.—The spiders and other insects of Sarepta, by A. Becker (in German).—The Dariinsk mineral water in the Government of Moscow, by A. Sabanéeff (in Russian). The spring is rich in iron, and is like that of Lipetsk.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, October 3.—Dr. D. Sharp, President, in the chair.—Mr. F. P. Pascoe exhibited a number of new species of *Longicornia*, from Sumatra, Madagascar, and South Africa.—Dr. P. B. Mason exhibited, for Mr. Harris, a specimen of *Chœrocampa Nerii*, recently captured at Burton-on-Trent.—Mr. S. Stevens exhibited a specimen of *Vanessa Antiopa*, which he caught in the Isle of Wight in August last.—Mr. E. B. Poulton exhibited a living larva of *Smerinthus ocellatus* in the last stage, fourteen larvæ of *Boarmia roboraria*, and some cocoons of *Rumia cratægata*. The object of the exhibition was to show the influence of special food-plants and surroundings on the colours of the larvæ and cocoons.—Mr. M. Jacoby exhibited a varied series of *Titubœa sanguinipennis*, Lac., from Central America. He stated that many of the varieties exhibited had been described in error as distinct species.—Mr. Billups exhibited specimens of *Bracon brevicornis*, Wesm., bred from larvæ of *Ephestia Kühniella*. He remarked that this rare species had only been recorded as bred on two or three occasions, viz. by the Rev. T. A. Marshall, Mr. W. F. Kirby, Herr Brischke, and Mr. Sydney Webb.—Mr. W. Warren exhibited specimens of *Antithesia ustulana* and *A. fuligana;* also bred series of the following species: *Eupœcilia Degreyana*, *Stigmonota pallifrontana*, *Cacœcia decretana*, and *Gelechia peliella*.—Lord Walsingham, F.R.S., exhibited specimens of several species of the genus *Cryptophasa* of the Tineina, some of the most remarkable being males and females of *Zitua balteata*, Walker, bred by Mr. Sidney Olliff from pupæ found in January last, at Newcastle, New South Wales, in burrows in branches of a species of Acacia.—Mr. F. D. Godman, F.R.S., exhibited a larva of a *Cicada*, from Mexico, having a fungoid growth on the head.—Captain Elwes exhibited a large number of butterflies, representing about 180 species, recently collected by himself and Mr. Godman in California and Yellowstone Park. The collection included many species of great interest, amongst others a *Cœnonympha* described by Edwards as an *Erebia*, a very rare species of *Thecla*, and a remarkable series of species of the genus *Colias*.—Mr. H. Goss exhibited, for Mr. W. J. Cross, an extraordinary variety of *Agrotis segetum*, caught by the latter near Ely in July last.—Mr. W. L. Distant read a paper entitled "An enumeration of the *Rhynchota* received from Baron von Müller, F.R.S., and collected by Mr. Sayer in New Guinea during Mr. Cuthbertson's expedition."—Mr. Poulton read a paper entitled "Notes in 1887 upon Lepidopterous larvæ, including a complete account of the life-history of *Sphinx convolvuli* and *Aglia tau*"; and Mr. White exhibited specimens of preserved larvæ of *S. convolvuli*, *A. tau*, and other species referred to in Mr. Poulton's paper. Mr. Jenner Weir, Mr. Kirby, Mr. White, and Dr. Sharp took part in the discussion which ensued.

PARIS.

Academy of Sciences, October 8.—M. Des Cloizeaux in the chair.—Order of appearance of the first vessels in the leaves of *Humulus Lupulus* and *japonicus*, by M. A. Trécul. These researches show that, as already announced by the author so far back as 1853, the stipuli may sprout long before any of the leaf-lobes make their appearance. The verification of the phenomenon is easy either in the *Humulus* here studied or in the *Cannabis sativa* previously described.—On the molecular weight and on the valency of perseite, by M. Maquenne. In a recent communication (*Comptes rendus*, cvi. p. 1235) the author showed that perseite possesses the function of a polyvalent alcohol, and that its ethers present the same centesimal composition as those of mannite and dulcite. It was also shown that the analysis of perseite yields the same results as mannite, and that these bodies at equal weight equally lower the freezing-point of their solvents. Hence perseite might be supposed isomerous with the mannites, $C_6H_{14}O_6$. But further researches, and the study of some new derivatives of perseite, clearly show the inaccuracy of the formula of this substance as determined in the former note, and as previously accepted by MM. Müntz and Marcano. It is now shown to be the immediate superior homologue to ordinary mannite with corrected formula $C_7H_{16}O_7$. It is at once the first heptavalent alcohol and the first sugar in C_7 that has yet been determined.—On the orbit of Winnecke's periodical comet, and on a new determination of the mass of Jupiter, by M. E. de Haertl. The results are given of the author's protracted observations, undertaken for the purpose of ascertaining whether any change due to a resisting medium has taken place in the revolutions of this short-period comet, whose return was carefully recorded in 1858, 1869, 1875, and 1886. A fresh calculation is made of Jupiter's mass, based on its disturbing effect on the comet's orbit. The value of the mass that best satisfies all the observations is $m = 1 : 1047{\cdot}1752 \pm 0{\cdot}0136$.—Reflected image of the sun on the marine horizon, by M. Riccò. The observations here recorded have been taken since 1886, on the east terrace of the Observatory of Palermo, 2 kilometres from the shore and 72 metres above sea-level. They were interrupted this year by the foggy horizons, probably caused by the eruptions of Vulcano, which began on August 2, and have continued at intervals down to the present time. The observations will be renewed next spring, with the return of the sun to the marine horizon. Under clear skies and in calm weather the elliptical form of the image of the sun is very evident, so that it seems strange the ancient astronomers did not perceive in this phenomenon an indication of the rotundity of the globe.—A study of the heats of combustion of some acids connected with the series of the oxalic and lactic acids, by M. Louguinine. The results of the researches communicated in this memoir have reference to the malonic, succinic, pyrotartaric, suberic, sebacylic, and oxy-isobutyric acids. The first five present homologous relations between themselves and with oxalic acid; the last is similarly connected with lactic acid.—On the freezing-points of the solutions of the organic compounds of aluminium, by MM. E. Louïse and L. Roux. The determination of the vapour densities of these substances has led the authors to give them the general formula Al_2X_6. Their further investigations, here described, have been carried out with a view to determining the value of the molecular weights of the organic compounds of aluminium by Raoult's method based on the lowering of the freezing-points of the solutions. Their new determinations confirm their previous conclusions on the vapour densities, and show that these substances can in no case be represented by the simple formula AlX_3.—M. E. Picard contributes a paper on Laplace's transformation and linear equations with partial derivatives; and the Perpetual Secretary gives the analysis of a note presented by M. G. Govi on a new method for constructing and calculating the place, position, and size of images given by complex optical systems.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Elementary Commercial Geography: H. R. Mill (Cambridge University Press).—Star Atlas: Dr. H. J. Klein, translated by E. McClure (S.P.C.K.).—Reports on the Mining Industries of New Zealand (Wellington, N.Z.).—The Illustrated Optical Manual, 4th edition: Sir T. Longmore (Longmans).—British Birds, August, September, and October: H. Saunders (Gurney and Jackson).—British Dogs, No. 24: H. Dalziel (U. Gill).—The Speaking Parrots, Part 6: Dr. K. Russ (U. Gill).—Elementary Statics: Rev. J. B. Lock (Macmillan).—Chemical Notes and Equations, 3rd edition: R. M. Murray (Maclachlan and Stewart, Edinburgh).—Catalogue of the Fishes in the Collection of the Australian Museum, Part 1; Recent Palœichthyan Fishes: J. D. Ogilby (Sydney).—Three Formations of the Middle Atlantic Slope: J. M. McGree.—Zeitschrift für Wissenschaftliche Zoologie, xlvii. Band, 2 Heft (Williams and Norgate).—Bulletin de l'Académie Royale des Sciences de Belgique, No. 8 (Bruxelles).

CONTENTS.

THURSDAY, OCTOBER 25, 1888.

EMPIRICISM VERSUS SCIENCE.

THERE is among the general public a perennial tendency to exalt and honour the man of affairs—the man whose business it is to pose as figurehead and carry through great schemes in the face of the community—at the expense of the quiet student or the scientific pioneer. And every now and then this permanent tendency is played upon by someone who ought to know better, and excited into more conspicuous vitality; sometimes taking the form of a demonstration in favour of "practice" as opposed to "theory," sometimes the form of a flow of ribaldry against scientific methods and results. Such a periodical outburst seems to have broken loose just now, and the technical press is full of scoffs at men of science, and glorification of the principle of rule-of-thumb.

It is easy for students of science to smile at the absurdities thus propounded and to take no further notice. It is only statements which have a germ of truth about them that are able really to bite and sting. And if a feeling of momentary irritation is excited by reading some piece of extra absurdity set forth for the unedification and misleading of the public, the best antidote is a return to one's own work, and silence.

It is possible, however, sometimes to carry complaisance too far. "If you make yourself a sheep," was one of Franklin's mottoes, "the wolves will eat you"; and there is sound worldly wisdom in the maxim, though it may be difficult always to reconcile it with some other precepts of a higher authority.

The only really irritating thing about these attacks is that they do not call things by their right names: if they did, the absurdity would be too glaring for anyone of importance to be taken in. So they sing the praises of empiricism and decry science under the totally false and misleading names of "practice" and "theory" respectively. Now plainly there is no real antithesis possible between theory and practice unless one is right and the other wrong or incomplete. If both are right, they must agree. If one is conspicuously right and the other conspicuously wrong, it is a very cheap and simple matter to distribute praise and blame.

Whenever there is discordance between theory and practice—a theory which says how a thing ought to be done, and the practice by which its doing has hitherto been attempted—manifestly there is something wrong with one or other of them. The blame should be applied to the error, and the error may lie equally well on either side. The practice in early steam-engines was to cool the cylinder at every stroke in order to condense the steam. It certainly did condense the steam, and was therefore successful. The self-styled "practical man" of that day would most likely have derided any small-scale laboratory experiments as futile and ridiculous, and not corresponding to the conditions of actual work. Nevertheless, that eminent theorist, James Watt, by studying the behaviour of saturated steam under various circumstances in a scientific manner, and by discovering that the pressure in any connected system of vessels containing vapour would rapidly become equal to the vapour-tension corresponding to the coldest, did succeed in introducing a noteworthy improvement into a time-honoured practice. Again, the question of the specific heat of saturated steam, whether it be zero, or positive, or negative, is a highly scientific question, first solved on the side of theory by Clausius, an eminent example of the purely scientific worker; but the fact that it is negative has an immediate practical bearing on the important subject of steam-jacketing, and fully explains the advantage of that process.

But it may be said the advantage of the steam-jacket was discovered by experience. Very likely. It is a conspicuous and satisfactory fact that progress can be made in two distinct ways. Sometimes the improvement is discovered by what may be termed blindfold experience: a certain operation turns out to be uniformly successful, and, without any further knowledge, that is sufficient justification of its performance. The observed fact that inhalations of chloroform produced temporary anæsthesia was sufficient justification of its use in surgery without any theory as to why it so acted. The motion of the planets in ellipses, according to certain laws, might have been deduced from the theory of gravitation; but historically those motions were deduced by a laborious comparison of observations. Sometimes observation is ahead of theory; sometimes theory is ahead of observation. It is mere nonsense to decry either on that account.

It is also absurd to deny that our knowledge of a fact, and our confidence in its use, and of all the conditions under which it may be used or may not be used, are enormously enhanced when one knows not only the bare fact by observation empirically, but when also one thoroughly understands the reasons and the laws connected with it. It would be justifiable to employ a successful drug even if one knew nothing of its mode of action, and could give no reason for its effects; but it is far more satisfactory to understand it exactly, and to have a complete theory of its physiological action. One can then decide beforehand, without empiricism, or a possibly fatal experiment, under what circumstances and to what constitutions it would be noxious.

The fact that lightning-conductors are often successful is ample justification for their use, but it will be far more satisfactory when, by help of laboratory experiments and theory, one understands all the laws of great electrical discharges, and can provide with security against their vagaries.

These things are truisms, but it would seem to be sometimes necessary to utter truisms.

Sometimes one hears a judgment such as this: "Yes, he is a very good man in some ways, but he is too much of a theorist." And then there is a sapient shaking of heads, as if the term "theorist" were an intelligible term of abuse. You suppose it means that the wretched man knows too much about the mode of working of things; too much about the strength of materials, too much about graphical statics, if he is engaged in building a bridge; but if you ask the meaning of the fatal term, you find it explained in some such way as that "he does not attend to details," or "he does not look after his workmen," or "he accepts rotten materials." Then why not apply some term which shall legitimately mean these things, such as careless, or lazy, or ignorant, or unbusiness-like? Probably the word "theorist" as a term of abuse is meant to euphemistically imply all these things. If so, it is a foolish euphemism.

There are certain notable theorists who are so eminent that no one is willing to stultify himself by abusing them; and inasmuch as the superabundant energy of some of these men often leads them occasionally out of their main pursuits into alien fields of activity, wherein nevertheless they frequently shine as the equals or superiors of smaller men whose life-work lies in the same fields, it is becoming customary to ingeniously attempt to exclude them from the class it is wished to denounce, and to include them in the circle wherein they are comparatively amateurs or dabblers.

At the recent meeting of the British Association the old joke was repeated about claiming Sir William Thomson as an electrical engineer instead of a physicist and mathematician. This is all very well as a joke, but the British public is too apt to take these things in sober earnest. The range of activity of a pre-eminently great man is frequently not a narrow one, and he is extremely likely to shine in whatever he takes up, even if it be only as a pastime, or as relief from more serious work. Sir Isaac Newton made an excellent Master of the Mint. Perhaps therefore, in his day, City men claimed him as essentially one of themselves. Sir William Thomson has amused himself with navigation, as well as with electrical engineering.

This outcry against theory is becoming absurd. It used to be confined to the conclusions of mathematics. It is indeed still rampant there, but it is being extended also to conclusions deduced in the laboratory. Everything done in the laboratory or the study is looked at with suspicion. The right place to study the laws of steam-engines is on a locomotive. The right place to study marine engineering is in the hold of a steamship. The only place to study lightning is in a thunderstorm.

Give out these plausible fallacies with a certain unction to a British audience, and you will evoke "loud applause." It is so easy to evoke loud applause by talking pernicious but plausible nonsense. Your British audience hates to think, and likes to have its stupidity tickled by some after-dinner sentiment, which makes it feel that, after all, no one really knows anything about anything; that whoever professes to understand a subject theoretically is *ipso facto* a quack; and that the only difference between itself and everybody else is that some people cloak their ignorance under a show of learning and mathematical formulæ. These humbugging theorists may therefore be cheaply derided. "There is a lot of arrant humbug stowed away now and then under a mathematical cloak," said a technical paper the other day.

And what of the "practical" man? Any man who talks sense and goes to the bottom of things, so as to really understand and to be able to explain what he means and how things are, is essentially a practical man. One class has no right to monopolize this adjective. A mathematician may make statements according completely with facts and phenomena, and leading to the most complete understanding of every-day truths. An empiric may utter the most glaring absurdities, utterly out of harmony with anything in heaven or earth, or under the earth. Is Prof. Stokes therefore to be styled unpractical, and Prof. (shall we say) Pepper practical?

Push the matter to an extreme, and you can enunciate sentences like these. If you want to know about steam-engines and compound locomotives, you must go, not to theorists like Rankine, or Unwin, or Cotterill, or even to Mr. Webb. The driver of the Scotch express is the man really able to give you trustworthy and practical information.

If you want to know the principles underlying the construction of ships, and why some ships go quicker than others, do not think of applying to the writings of the late William Froude with his nonsensical paraffin toys, but consult the captain of the *Umbria* or the *City of Rome*.

We have set down these sentences as a *reductio ad absurdum* of some of the claims set forth in favour of empiricism as against science, under the specious and plausible heading of practice against theory: but really they are not a whit more absurd than much that is seriously argued; and were they propounded under favourable auspices to an average British audience, they would very likely be swallowed without nausea. The experiment is almost worth trying, only it would be difficult for anyone himself faithless to avoid some suspicion of irony, which would be fatal to success.

Space may be afforded for a few more very brief extracts from some of the engineering and technical journals during the past month. The first is so choice as to need no comment:—"The world owes next to nothing to the man of pure science. . . . . The engineer, and the engineer alone, is the great civilizer. The man of science follows in his train." This doctrine is explained and illustrated by insistence on the futility of Faraday's work in connection with magneto-electricity, until taken up and realized by the practical man.

In the same paper, a week later, occurs the following:—"No one knows anything with certainty about lightning outside of the common knowledge possessed by most fairly educated people." And again, "We fail to see that what is true in the laboratory must be true out of doors."

This is interesting as an almost exact reproduction of one of the historic objections made to Galileo's unwelcome discovery of Jupiter's satellites. It was then similarly maintained that, though the telescope was all very well for terrestrial objects, it was quite misleading when applied to the heavens.

An instance of a converse proposition is told in a recent popular work on astronomy (is it Sir R. Ball's?), about a farmer and amateur astronomer, who came to the writer with a revolutionary system of astronomy, based upon a number of observations which he had taken with a sextant of the altitude of the heavenly bodies. The gentleman had thus found that the generally received opinion about the distances of the fixed stars was extremely erroneous. But on inquiry it turned out that his altitudes were all calculated on the common-sense and well-known fact that sixty-nine miles make a degree. Finding it impossible to get the gentleman to put his mind into an attitude for receiving any instruction on the theoretical subject of the measurement of angles, the representative of the orthodox clique who impose their statements on the world as something more trustworthy than common information prevailed on the gentleman to apply his sextant to determine the altitude of his own barn. This *reductio ad absurdum* was avoided, however, and the overthrow of orthodox astronomy successfully maintained, by the hoped-for convert "failing to see that an astronomical instrument had any application whatever to terrestrial objects."

A paragraph recently inserted in an electro-technical journal, with editorial sanction, styles mathematicians "the accountants of science," and goes on in a tone less comic than bitter:—"When some young shaver shoots off his school learning" (*i.e.* uses some mathematical operation or notation), "I feel inclined to reply to him in Italian, as both are as generally and completely understood in the Society of ——." Now if the subject under discussion were, say, passages in Tasso or Dante, an Italian quotation would be very natural, and persons ignorant of the language would hardly be invited, or indeed anxious, to express an opinion. Is it not equally clear that when the subject-matter is numerical magnitude and quantity, the appropriate language may sometimes have to be used?

It has always been customary, as we have before remarked, for the empiric to feel some hostility to the mathematician, especially to the mathematician who endeavours to apply his powerful and beautiful machinery to the elucidation of the facts of Nature. But only recently has it become the fashion to extend the same attitude of mistrust and dislike to the experimental worker in a laboratory. Both these hostilities probably have their root in an instinct of self-protection. Without them the empiric would be constantly suffering wounds in his self-esteem, and might lose confidence in his faith as to the universal prevalence of ignorance and the advantages of rule-of-thumb. For a man of the world professing a certain science to have to recognize a certain number of minds as immeasurably superior to his own, and their conclusions in that very science as being almost certainly correct, although flatly opposed to his own instinct and traditions: this is in many cases intolerable. He cannot away with these great theorists, neither can he in his heart contemn them; but he can do his best to deceive himself and others by extending to them euphemistic terms of abuse, and by pretending that he could do all that they do if only he thought it worth while. He may even go further, and flinging abroad a universal accusation of ignorance will easily delude a gullible public into the belief that knowledge is after all only a matter of opinion, and that what one man says is quite as good as what is said by another.

And in this procedure he is fairly secure against any retaliation from the great men. They are deeply and painfully conscious of ignorance in one sense: their knowledge sits lightly upon them; and when broadside and grotesque accusations of ignorance are hurled at them with the intention of putting them on a level with the uninstructed and, in quite another sense, "ignorant" populace, they resent it not; scarcely recognizing, indeed, the absurdity of the position.

The hostility of the "practical man" for the systematic and recondite methods of science was at one time mainly borne by mathematicians, because they it was mainly who spoke a language and thought thoughts too high for common apprehension. Since then experiment has become more exact, more illuminated by theory, more scientific and less empirical; hence it is that the hostility is now being extended to the experimentalist in his laboratory as well.

But really, it may be rather offensively suggested, what other attitude can be taken up? If a man is to be capable of getting schemes through Parliament, of impressing a jury, and generally of playing to the gallery and becoming a power in the State, he cannot, unless very exceptionally endowed, have the aptitudes and powers proper to a man of high science. And yet it will never do to allow even to himself that the scientific man is in his own line immeasurably above him. Such a reverent and submissive attitude would ruin his chance with the gallery at once. Swagger and a confident front are more than the tricks of the trade, they are the essentials to success.

We are glad to recognize, however, that the recent outburst against the methods and conclusions of pure science is the work of the camp-followers rather than of the leaders on the commercial side. There have been and are several conspicuous examples not only of the scientific man taking a high position on the commercial side, but also of the commercial man taking a high position in the ranks of pure science. This interchange of individuals, and the further *rapprochement* which the great extension of science into industrial life of various kinds has caused, and must in the future still further cause, are making it now clearly recognized how intimately pure science and the commercial applications of science are connected together, how great is their mutual dependence on each other, and how essential to the well-being of each is a close and friendly co-operation with the other.

These facts, and the friendly attitude of the leaders on both sides, render the attempt made in the rank and file to sow discord between the two great classes the more absurd, and must make it in the long run entirely futile.

THE MESOZOIC MAMMALIA.

The Structure and Classification of the Mesozoic Mammalia. By H. F. Osborn. *Journ. Ac. Nat. Sci. Philadelphia*, Vol. IX. No. 2. (Philadelphia: Published by the Academy, 1888.)

IN the elaborate memoir before us, comprising eighty quarto pages of text, illustrated by thirty woodcuts and two plates, Prof. Osborn, of Princeton College, New Jersey, gives us the result of his researches into the structure of the Mesozoic and allied Tertiary Mammals, based upon observations carried on both in America and Europe. As a rule, these Mammals are of small size, and are mainly known to us by more or less imperfect jaws and teeth; by far the greater number of specimens consisting of the lower jaw or mandible. Now, it is well known that even in groups of the smaller Mammals which are well represented at the present day, such as the Shrews among the Insectivora, or the Bats, it is almost, if not quite, impossible to recognize many of the genera, to say nothing of the species, when we have to deal only with a series of fossil or sub-fossil lower jaws from the cavern or later Tertiary deposits. And if this be so in groups with which we are well acquainted, the difficulty is of course increased many times over when we have to deal with forms having no close analogues among the existing fauna. The puzzle is further increased by the difficulty of referring such portions of upper jaws as are more rarely found to the species indicated by mandibles;

and this induces a great danger of founding species or higher groups upon the evidence of upper jaws, which cannot be decisively shown to be distinct from those founded upon the evidence of the mandibles. Prof. Osborn, as will be noticed below, has not altogether steered clear of this danger; and we consider it would be advisable in delicate researches of this nature to lay down a rule that family or higher groups should only be formed upon the evidence of homologous parts, even if genera and species have been named upon the evidence of dissimilar parts of the skeleton.

Before, however, proceeding to any detailed criticism, it will be advisable to take a brief survey of the memoir before us, and to note the scheme of classification which is proposed. The memoir begins with a survey of previous work on the subject, especial attention being directed to the labours of Sir Richard Owen in Europe, and to those of Profs. Cope and Marsh in America. On the second page (187) a table is given of all the described genera of Mesozoic Mammals, which include forms from Europe, America, and South Africa; together with certain allied Tertiary genera from North America and France, and *Thylacoleo* of the Pleistocene of Australia. We may add that since this memoir was sent to press, forms allied to those of the North American Eocene have been described by Señor Ameghino in the Tertiaries of the Argentine Republic. The next section is devoted to a detailed description of the British forms, in which certain generic terms, proposed by the author in a preliminary communication, are fully described and illustrated. We may here mention that the author tells us that the process of passing his memoir through the press occupied an unusually long period, during which certain other memoirs appeared on the subject; and that he thus saw occasion to modify in some respects several statements made in the earlier part of the work, footnotes being usually appended to this effect.

After the descriptive portion we come to what is really the most important section of the whole memoir—namely, that headed the classification and zoological relationships of the Mesozoic Mammalia. It is here observed that these forms may be divided into two large groups. "In the first group, *A*, one of the incisors is greatly developed at the expense of the others, and of the canine, which usually disappears; behind these teeth is a diastema of varying width, followed by premolars which are subject to great variation in form and number, while the molars bear tubercles. In the second group, *B*, the incisors are small and numerous, the canine is always present and well developed; the teeth usually form a continuous series, and the molars bear cusps instead of tubercles." These two groups are compared to the Diprotodontia and Polyprotodontia, among existing Marsupials, and the following scheme of classification is proposed:—

A. First Group.

I. Sub-order **Multituberculata.**

1. Family PLAGIAULACIDÆ.—*Microlestes, Plagiaulax, Ctenacodon, Ptilodus, Neoplagiaulax, Meniscoessus*, and perhaps *Thylacoleo.*
2. Family BOLODONTIDÆ. — *Bolodon, Allodon*, and perhaps *Chirox.*
3. Family TRITYLODONTIDÆ.—*Tritylodon, Triglyphus,*
4. Family POLYMASTODONTIDÆ.—*Polymastodon.*

Incertæ sedis—*Chirox.*

B. Second Group.

I. Order **Protodonta.**

Family DROMATHERIIDÆ.—*Dromatherium, Microconodon.*

II. Sub-order **Prodidelphia.**

1. Family TRICONODONTIDÆ.—*Amphilestes, Amphitylus, Triconodon, Priacodon, Phascolotherium, Tinodon, Spalacotherium, Menacodon.*
2. Family AMPHITHERIIDÆ.—*Amphitherium, Dicrocynodon* (*Diplocynodon*), *Docodon, Enneodon, Peramus.*
3. Family PERALESTIDÆ. — *Peralestes, Peraspalax, Paurodon.*
4. Family KURTODONTIDÆ.—*Kurtodon.*

III. Sub-order **Insectivora Primitiva.**

1. Family AMBLOTHERIIDÆ.—*Amblotherium, Achyrodon.*
2. Family STYLACODONTIDÆ.—*Stylacodon, Phascolestes, Dryolestes, Asthenodon.*

Incertæ sedis—*Laodon.*

The Multituberculata, excluding *Thylacoleo*, extend in time in Europe and North America from the Upper Trias to the Lower Eocene, but the recently discovered South American forms may be of later age. In discussing the relationship of this group of families on p. 212, the author states that, admitting their Marsupial relationship, it is clear that the genera "are closely related to each other, and widely separated from the Diprotodontia by their dental structure, which is very dissimilar, and indicates that they probably branched off from the stem of the recent Marsupials at a remote period, probably the Triassic." They are accordingly regarded on the following page as a sub-order of Marsupials, characterized by the tuberculated characters of their molars. If, however, as suggested on p. 214, *Thylacoleo*, which is evidently only an aberrant and specialized Phalanger, has any sort of relationship to the *Plagiaulacidæ*, then it will be evident that this group cannot be even subordinately separated from the Diprotodonts Further observations upon the relationships of this group. are given upon pp. 251 and 254, the latter section having evidently been written subsequently to the earlier sections. On the former page evidence is adduced to show that in some of these forms the first upper incisor has been lost, and the second becomes hypertrophied, whereas in existing Marsupials it is the first which always persists and becomes enlarged. There is no evidence as to the serial homology of the lower incisor. On p. 254 and the following pages, the suggestion of Prof. Cope, based on the resemblance of the molars of the Multituberculata to the aborted teeth of *Ornithorhynchus*, that these forms may be Monotremes, is discussed at some length, but without any definite conclusion being reached. We presume, however, that in writing this part of the memoir the author had come to the conclusion that the relationship of these forms to *Thylacoleo* is altogether a myth. It is, however, at first sight not very easy to believe that the general similarity in the structure of the cutting fourth premolar in the Multituberculata and the modern Diprotodontia is not indicative of a real affinity between the two; and as to the argument that the peculiar structure of the two molars is of itself sufficient to indicate the subordinal distinction of the Multituberculata, we think that a sub-order which contains such different types of molar dentition as are shown by *Macropus, Pseudochirus, Phas-*

colarctus, and *Phascolomys*, could surely also find room for the Multituberculate type. The evidence of the homology of the incisors is, however, a weighty one in the author's favour.

Prof. Osborn places the Triassic *Microlestes* with the *Plagiaulacidæ* rather than the *Bolodontidæ*, but we think the existence of a cutting fourth lower premolar ought to be proved before this view can be definitely admitted. There may also be considerable hesitation in accepting the view expressed on p. 217, that there are five premolars in the upper jaw assigned by Prof. Marsh to *Ctenacodon;* but beyond these and other small points the author's classification of this group appears to commend itself.

We cannot say the same in regard to the classification of the second group, which, as we have seen, it is proposed to split up into one distinct order, into one sub-order provisionally referred to the Marsupialia, and a second assigned with more hesitation to the Insectivora. In this group the author has, we venture to think, found differences which, if they exist at all, are by no means of the importance he attributes to them; while at least one case occurs to us, where, to say the least, there is a considerable presumption that specimens assigned to the two sub-orders may really be referable to a single genus. Sufficient account does not, indeed, appear to have been taken of the variation in the dentition of different recent genera of Marsupials which are usually included in a single family; as, for example, *Thylacinus*, *Dasyurus*, and *Myrmecobius* among the Polyprotodonts, and *Phalanger*, *Pseudochirus*, and *Phascolarctus* in the Diprotodonts. In the case of obscure fossil forms like the present, it appears to us that there ought to be the greatest hesitation in making groups of higher value than family rank; and that even in the case of families their limits ought to be much more loosely drawn than among existing forms, where we have full evidence before us. It is, indeed, far more advantageous to keep all such obscure forms more or less closely associated until absolutely decisive evidence is forthcoming as to their right to wide separation. In the present instance, however, the author has, to put it in the mildest form, by no means adduced any such decisive evidence; while, as already mentioned, there is a strong presumption that in certain particular cases he has widely separated closely allied, if not absolutely identical, forms.

The first so-called order—the Protodonta—is formed for the reception of the American Triassic *Dromatherium*[1] and *Microconodon;* if, indeed, the latter be really entitled to generic distinction. The grounds for the ordinal distinction of these forms are that the roots of the cheek-teeth are not fully divided; but stronger evidence than this is required before these obscure forms can be definitely regarded as entitled to constitute more than a family. And even if they belong to an order distinct from the Marsupials, there is no evidence to show that they are not Monotremes, or perhaps rather Prototheria.

The sub-order Prodidelphia is defined as including primitive Marsupials, generally characterized by the presence of four premolars and numerous molars, the latter having distinctly divided roots. It is, however, added (on p. 259) that "no definite sub-ordinal character can be assigned; but in view of the retention of several features, and of their ancestral position, these Mammals may be distinguished from the recent Marsupials as the sub-order Prodidelphia." In our own judgment, the formation of a large group which confessedly cannot be distinguished from one already established is unjustifiable, and not conducive to any advantage. The first family of this group is the *Triconodontidæ*, in which, as shown above, our author includes a large number of genera. The genus *Triconodon*, together with the allied or identical American *Priacodon*, has, however, such a totally different *facies* from all the other forms, that we are inclined to follow Prof. Marsh in regarding it as alone constituting the family. We are, morever, rather at a loss to find the value of the characters which Prof. Osborn regards as distinctive of the enlarged family; for, whereas he states in the definition of the family (on p. 227) that the "condyle is low," on the opposite page the genus *Amphitylus* is described as having the "condyle lofty." Some very interesting observations are recorded (p. 198) as to the changing and development of the teeth in *Triconodon*, in which it is concluded, as had been previously indicated by Mr. O. Thomas, that the replacement was limited, as in modern Marsupials, to a single premolar; while it is further shown that in many instances it appears probable that the last true molar was never developed. In classing *Phascolotherium*, of the Stonesfield Slate, in the *Triconodontidæ*, the author appears to have been greatly influenced by regarding *Triconodon* as having the condyle placed low down on the mandible. We have, however, considerable doubts whether this is a character of much importance, as it varies so much in the allied *Phascolotherium* and *Amphitylus*. In considering that the whole of the seven cheek-teeth of *Phascolotherium* are true molars, the author departs very widely from the view taken by Sir R. Owen, and a great deal more evidence is required before it can be considered proved that at least the first two of these teeth are not premolars.

In making such mention as space permits of some of the other genera, we must take those included under the Prodidelphia and the so-called Insectivora Primitiva together. In this connection it appears that a great deal depends on the interpretation of the dental characters of the original genus *Amphitherium*, to which Prof. Osborn refers the fragment of a mandible figured on p. 192. It is stated, with great fairness, that when the author examined this specimen he regarded it as totally distinct from *Amphitherium*, but that comparisons of his drawings with figures led him to change his opinion. On p. 192 it is observed that "when these mutilated crowns [of the type] are compared with the perfect crowns of the newly-acquired jaw, there can be *no doubt* that they belong to the same pattern. *If this be the case*, the latter specimen is of great interest, as it enables us for the first time to fully characterize the molar dentition of *Amphitherium*." We have purposely italicized portions of the above sentences, since they show a somewhat curious instance of the author's method. Thus, in the first sentence the teeth of the new jaw are definitely stated to be of the type of those of *Amphitherium*, while in the second a provisional element is introduced; and yet subsequently this jaw is again definitely taken as affording the true structure of the *Amphitherium* molar. Far be it from us

[1] Prof. Osborn proposes to alter the spelling of this name to *Dromotherium*.

to say that this jaw does not belong to *Amphitherium*—it very probably does; but it certainly does not afford decisive evidence on which to base an extensive superstructure, and to make *Amphytherium* the type of one family, while *Amphitylus* and *Amphilestes* (regarded by Owen as closely related to the former) are referred to the *Triconodontidæ*. Then, again, exception may be taken to the interpretation of the molar structure in the jaw in question. Prof. Osborn regards the teeth as consisting of two cusps and a talon in line, approximating to the fashion of *Amphilestes;* but to us they appear to resemble those of the Upper Jurassic genus *Amblotherium*, in which the molars consist of a trilobed blade and a posterior talon. Now, *Amblotherium* is made the representative of a family which is taken as the type of the Insectivora Primitiva. Apart from the question of what *Amphitherium* really is, the molar teeth of *Amblotherium*, as already said, differ considerably from those of *Amphilestes* (Prodidelphia); but, since precisely analogous differences occur in a single family of existing Marsupials, these differences do not appear to afford grounds for even family, let alone ordinal, distinction. No definite characters are, indeed, given by which the Insectivora Primitiva (p. 235) are to be distinguished from the Prodidelphia; and if we compare the figure of the mandible of *Amphilestes*, given on p. 228, with that of *Amblotherium*, represented in Plate ix., Fig. 11, the resemblance in the contour of the posterior portion of the jaw is so close that scarcely even generic distinction could be drawn from this part. The conclusions drawn from this portion of the jaw in the different forms are indeed very remarkable. Thus we have already noticed how the low condyle is given as a character of the *Tritylodontidæ*, and yet the feature is totally wanting in the first genus, *Amphilestes*, which agrees exactly with *Amblotherium* in its lofty condyle. The alleged broad and narrow coronoids of the two forms may be in great part due to the effects of pressure. The absence of inflection in the angle of *Amblotherium* is shared by some of the forms included in the *Triconodontidæ*. Then, again, we are totally unable (after repeated examinations of the types) to see how the lower jaw, on which *Peraspalax* was founded, can be even generically distinguished from *Amblotherium*, the dental formula being, with the exception of an additional lower molar, identical; and yet the one genus is referred to the Prodidelphia, and the other to the Insectivora Primitiva. As another instance, the general similarity in the structure of the lower molars of *Spalacotherium* to those of *Chrysochloris* coupled with an analogous similarity existing between the upper ones of *Peralestes* and those of the same existing genus, suggests at all events a very considerable presumption that the two fossil genera may be identical. We find, however, *Spalacotherium* placed in the *Triconodontidæ*, while *Peralestes* is made the type of another family of the Prodidelphia, which includes the above-mentioned *Peraspalax*. Now, even if the above obvious resemblance is ignored, we totally fail to see any reason for including *Spalacotherium* in the *Triconodontidæ*, and agree with Prof. Marsh in regarding it as the type of a distinct family. If, moreover, any of these forms are to be referred to the Insectivora, we should have thought that *Spalacotherium*, with its *Chrysochloris*-like molars, and the reduction of its lower incisors to the Eutherian three, was the very one which had a claim to such a position. In regard to the new genus *Kurtodon*—the type of the *Kurtodontidæ*—we can only say that there appears to us to be no evidence that the upper jaws on which it is founded may not belong to one of the genera named on the evidence of the mandible.

Other points might be noticed if space permitted; but we have indicated enough to show that a great deal more must be absolutely proved before many of the genera admitted by Prof. Osborn can be even allowed to stand as definitely distinct; while, as to the proposed division of the Polyprotodont forms into Insectivora and Marsupialia, we have shown that in its present form it breaks down hopelessly at every point, although we are far from saying that all the known forms are certainly Marsupial. It appears, however, desirable, till we attain much fuller knowledge of their organization, to leave a large proportion of them in a single ill-defined family.

In criticizing this memoir we have not hesitated, in what appear to us to be the true interests of science, to speak freely. We should, however, be unjust if we failed to recognize the amount of labour of a very trying kind which the author has bestowed on the subject; and we especially commend the value of his observations on the Multituberculata. It is also a decided advantage to have all the American and European forms compared together by one who has had the good fortune to study so many of the types from both areas. Finally, no one can fail to be struck with the excellent illustrations with which the monograph is adorned, a large number of which, we believe, are from the author's own drawings.

EARTH SCULPTURE.

Les Formes du Terrain. Par Lieut.-Colonel G. De la Noë, avec la collaboration de Emm. de Margerie. 2 Vols. (Text, pp. 205; Plates xlix.). (Paris, 1888.)

THE origin of the features of the earth's surface must always prove an attractive subject no less to the geographer than to the geologist. The one describes and the other expounds; and the work before us is an admirable example of what may be done by the joint labours of geologist and geographer in illustrating and explaining the form of the ground.

In turning over the pages of the work, and in contemplating the many instructive diagrams and pictorial illustrations, one is prepared for a more exhaustive treatment of the origin of scenery than is really to be found in these volumes. So far as the geologist is concerned, the work is mainly a treatise on sub-aërial denudation, and with special reference to France. It is almost entirely occupied with the method of denudation by rain and rivers, and with an account of the features which they originate. We are told how different rocks are disintegrated by surface agencies, and how the broken material is afterwards transported by streams. Attention is especially called to the action of running water on rocks of varying character and inclination, to the influence of vegetation in preserving slopes at certain inclinations, and to the effect of rain in diminishing the angle of slopes. The influence of climate is dwelt upon, and it is shown how the permeable strata are characterized by dry valleys and few water-courses, while the impermeable beds support abundant streams.

The relations of disturbed strata, of anticlinals and synclinals to valley and hill, are duly noted; and it is pointed out how the flow of rivers is determined by the lie of the land when it is upraised from beneath the sea-level, and that in few cases are their courses directed by faults or fractures. The authors explain the recession of escarpments by the undermining or undercutting of softer beds and the production of landslips; and they note the influence of lateral streams in eroding these softer strata at the foot of the hills, a subject illustrated by reference to the Wealden area and other districts.

Little is said about marine denudation, for the action of the sea is essentially limited to the destruction of cliffs along its margin, and to the formation of marine platforms. Concerning great "plateaux of abrasion," or so-called "plains of marine denudation," the authors express their opinion that it would be wrong to attribute their formation exclusively to the sea, for they consider that the prolonged action of sub-aërial forces is to reduce the land to a level. Nor do the authors attribute great excavating power to glaciers. In their opinion these icy agents occupied and modified old valleys, and have not always effaced the pre-Glacial alluvial deposits; and they see little evidence of post-Glacial erosion. In these respects their observations are based on local and limited evidence; for in this country, although the main features were marked out in pre-Glacial times, there is abundant evidence of denudation by glacial action, and subsequently in times when the ice had done its work.

The authors have clearly pointed out that the topographical features are as a rule in direct relation with the geological structure; indeed, the form of the ground is one of the most important guides to the field-geologist in his delineation of the superficial distribution of the rock-masses. Nevertheless, in the explanation of the origin of our scenery, there are many points concerning the original extent of each formation, and the changes in texture which the rocks have undergone, that are but briefly, if at all, noticed in this work. In this respect, however, each country must be studied in detail before the complex history of its physical features can be deciphered.

The present work, as before stated, deals mainly with the mode in which rain and rivers sculpture the surface of the earth. It is an instructive summary of what is known on this subject, supported by original observations and by references to the principal authorities, and illustrated in a far more sumptuous manner than has ever been attempted in this country. H. B. W.

OUR BOOK SHELF.

Eclectic Physical Geography. By Russell Hinman. (New York: Van Antwerp, Bragg, and Co., 1888.)

To quote the author's preface, "The aim of this book is to indicate briefly what we know or surmise concerning the proximate causes of the more common and familiar phenomena observed at the earth's surface." The book commences with an introduction to the general laws of Nature, in which short outlines of the properties of matter and the various forms of energy are given. The earth is then treated as a planet; its relation to the sun and stars, and the nature and results of its movements, being described. Next come chapters on the atmosphere, the sea, the land, meteorology; and finally, the various forms of life. The causes of the movements of the atmosphere, sea, and land, and their respective effects, are all clearly stated. Brief outlines are given of the gradual disintegration of terrestrial rocks, and the subsequent transportation and accumulation of the products. Fossils and their teachings also receive attention. In short, nothing of importance has been omitted.

The general plan of the book bears a considerable resemblance to that suggested by the syllabus of the Science and Art Department's course of elementary physiography, and with a teacher to extend the preliminary chapter on the forms of energy, would form an admirable text-book for that subject. The order in which the subjects are taken is practically the same, and is obviously the most natural and rational.

The chapters on the forms of life and their distribution will prove of special interest to young students or general readers. There is a good outline of the development theory, and of what we know of man from prehistoric times.

The book throughout is illustrated by a great number of drawings, maps, and charts, which not only beautify but illustrate the text in a most admirable manner. The charts are drawn on three different systems of projection, each system being applied where it is most suitable; and, what is very important, the different systems are fully explained. A book like this cannot fail to impress the reader with a due sense of the importance of diagrammatic representation in facilitating description. The various sectional drawings are especially valuable in this respect.

The book thoroughly deserves the highest praise, and as an introduction to the study of science must certainly rank among the best.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Prophetic Germs.

PROF. RAY LANKESTER has mistaken me. When I said in my last letter of October 8 that "all organs whatever do actually pass through rudimentary stages in which actual use is impossible," I referred specially to the embryological development of the individual. This is a fact which cannot be denied. But on the Darwinian hypothesis this fact applies equally to the birth of species—which are nothing but the passing results of individual variation. If true now of all individuals, it must, on that hypothesis, have been true of them for all time.

Inheritance is no explanation of this fact. It is merely one part of the fact separately stated. Neither is "correlation of growth" any explanation of it. This, again, is a mere phrase stating in another form the very fact which it pretends to explain. All organic growths are "correlated." But with what? First, with each other; and, secondly, with some combined use, which invariably lies in the future when such growths begin. "Correlation of growth" is the law under which "prophetic germs" begin to be developed; and this prophetic character becomes all the more marked in proportion as we carry back existing forms of life to the forms which were primæval. It is a favourite idea among the disciples of Darwin that the embryological development of individuals represents in epitome the whole history of organic life. I do not see why they should object to it when it leads us to the conclusion that the whole organic world must have begun in germs which were prophetic—that all organs must have come into being before they could be used.

ARGYLL.

Definition of the Theory of Natural Selection.

IN his Presidential address before Section D of the British Association, Mr. Dyer is reported to have said, while alluding to myself:—"He has startled us with the paradox that Mr. Darwin did not, after all, put forth, as I conceive it was his own impression that he did, a theory of the origin of species, but only of adaptations. And inasmuch as Mr. Romanes is of opinion that specific differences are not adaptive, while those of genera are, it follows that Mr. Darwin only really accounted for the origin of the latter, while for an explanation of the former we must look to Mr. Romanes himself" (NATURE, September 13, p. 476).

It is here stated: (1) that in my opinion specific differences are not adaptive; (2) that I regard Mr. Darwin's theory as explaining the origin of genera, but not the origin of species; and (3) that, consequently, biologists are virtually invited by me to accept the theory of physiological selection as a substitute for Mr. Darwin's theory of natural selection, in so far, at all events, as the origin of species is concerned.

In direct contradiction to all these statements I will now quote passages from the paper with reference to which they are made. It would be easy for me to add further quotations to the same effect under each of the three heads, but the following will be sufficient to serve the double purpose which I have in view—namely, first to correct misrepresentations, and next to furnish a basis for further remarks upon the subject. The italics have reference only to the former purpose.

(1) and (2).—"It [the theory of natural selection] is not, strictly speaking, a theory of the origin of *species*: it is a theory of the origin—or, rather, of the cumulative development—of *adaptations*, whether these be morphological, physiological, or psychological, and whether they occur in species only, or likewise in genera, families, orders, and classes.

"These two things are very far from being the same; for, on the one hand, in an enormously preponderating number of instances, adaptive structures are common to numerous species; while, on the other hand, the features which serve to distinguish species from species are, as we have just seen, by no means *invariably*—or even *generally*—of any adaptive character. Of course, if this were not so, or if species *always* and *only* differed from one another in respect of features presenting some utility, then any theory of the origin of such adaptive features would also become a theory of the origin of the species which presented them. As the case actually stands, however, not only are specific distinctions *very often* of no utilitarian meaning; but, as already pointed out, the most constant of all such distinctions is that of sterility, and this the theory of natural selection is confessedly unable to explain. . . . *In so far as natural selection has had anything to do with the genesis of species, its operation has been, so to speak, incidental: it has only helped in the work of originating species in so far as some among the adaptive variations which it has preserved happen to have constituted differences of only specific value.* But there is an innumerable multitude of other such differences with which natural selection can have had nothing to do—particularly the most general of all such differences, or that of mutual sterility; while, on the other hand, by far the larger number of adaptations which it has preserved are now the common property of numberless species. *But let me not be misunderstood. In saying that the theory of natural selection is not, properly speaking, a theory of the origin of species, I do not mean to say that the theory has no part at all in explaining such origin. Any such statement would be in the last degree absurd.* What I mean to say is that the theory is one which explains the origin or the conservation of adaptations, whether structural or instinctive, and *whether these occur in species,* genera, families, orders, or classes. *In so far, therefore, as useful structures are likewise species-distinguishing structures,* so far is the theory of their origin *also* a theory of the origin of the species which present them."

(3) "Let it, therefore, be clearly understood that it is the office of natural selection to evolve adaptations—not therefore or necessarily to evolve species. *Let it also be clearly understood that in thus seeking to place the theory of natural selection on its true logical footing, I am in no wise detracting from the importance of that theory.* On the contrary, I am but seeking to release it from the difficulties with which it has been hitherto illegitimately surrounded. . . . I cannot feel that I am turning traitor to the cause of Darwinism. On the contrary, I hope thus to remove certain difficulties in the way of Darwinian teaching; and I well know that Mr. Darwin himself would have been the first to welcome my attempt at suggesting *another factor in the formation of species, which, although quite independent of natural selection, is in no way opposed to natural selection, and may therefore be regarded as a factor supplementary to natural selection.* . . . And here, as elsewhere, I believe that the co-operation enables the two principles to effect very much more in the way of species-making than either of them could effect if working separately. On the one hand, without the assistance of physiological selection, natural selection would, I believe, be all but overcome by the adverse influences of free intercrossing—influences all the more potent under the very conditions which are required for the multiplication of species by divergence of character. On the other hand, *without natural selection, physiological selection would be powerless to create any differences of specific type, other than those of mutual sterility and trivial details of structure, form, and colour*—differences wholly without meaning from a utilitarian point of view. But in their *combination* these two principles appear to me able to accomplish what neither can accomplish alone—namely, a full and satisfactory explanation of the origin of species."

These quotations appear to me sufficient to prove the inaccuracy of Mr. Dyer's remarks. But I should not have taken the trouble to notice misinterpretations of so absurd a kind, were it not that I have something more to say on the subject of which they treat. For Mr. Dyer, in his address, alludes to a recent criticism by Mr. Huxley, which also deals with my "paradox," but does so in a very different manner. That is to say, the passages which Mr. Huxley devotes to this subject exhibit a much more careful consideration of the points in it to which he alludes, as well as a manifest desire to state the issue fairly. I will therefore pass on to consider the criticism as it was originally presented by Mr. Huxley, leaving behind the teratological reproduction of it by Mr. Dyer as effectually disposed of by mere quotations from my paper itself.

The substance of Mr. Huxley's criticism, in so far as it apparently applies to me, is conveyed in the following words:—"'Favourable variations' are those which are better adapted to surrounding conditions. It follows, therefore, that every variety which is selected into a species is so favoured and preserved in consequence of being, in some one or more respects, better adapted to its surroundings than its rivals. In other words, every species which exists, exists in virtue of adaptation, and whatever accounts for that adaptation accounts for the existence of the species. To say that Darwin has put forward a theory of the adaptation of species, but not of their origin, is therefore to misunderstand the first principles of the theory. For, as has been pointed out, it is a necessary consequence of the theory of selection that every species must have some one or more structural or functional peculiarities, in virtue of the advantage conferred by which, it has fought through the crowd of its competitors, and achieved a certain duration. In this sense, it is true that every species has been 'originated' by selection" (*Proc. Roy. Soc.*, vol. xliv. No. 269, p. xviii.).

Now, in the first place, I have nowhere said that "Darwin has put forward a theory of the adaptation of species, but not of their origin." I said, and continue to say, that he has put forward a theory of *adaptations in general*, and that where such adaptations appertain to species only (*i.e.* are peculiar to particular species), the theory becomes "*also* a theory of the origin of the species which present them." The only possible misunderstanding, therefore, which can here be alleged against me is, that I fail to perceive it as a "necessary consequence of the theory of selection that *every* species *must* have some one or more structural or functional *peculiarities*" of an adaptive or utilitarian kind. Now, if this is a misunderstanding, I must confess to not having had it removed by Mr. Huxley's exposition.

The whole criticism is tersely conveyed in the form of two sequent propositions—namely, "Every species which exists, exists in virtue of adaptation; and whatever accounts for that adaptation accounts for the existence of the species." My answer is likewise two-fold. First, I do not accept the premiss; and next, even if I did, I can show that the resulting conclusion would not overturn my definition. Let us consider these two points separately, beginning with the latter, as the one which may be most briefly disposed of.

I. Provisionally conceding that "every species which exists, exists in virtue of adaptation," I maintain that my definition of the theory of natural selection still holds good. For even on the basis of this concession, or on the ground of this assumption, the theory of natural selection is not shown to be *primarily* a theory of the origin of species. It follows, indeed, from the assumption—is, in fact, part and parcel of the assumption—that all species have been originated by natural selection; but why? *Only because natural selection has originated those particular adaptive features in virtue of which species exist as species.* It is only in virtue of having created these features that natural selection has created the species presenting them—just as it has created genera, families, orders, &c., in virtue of *other* adaptive features extending through progressively wider areas of taxonomic division. Everywhere and equally this principle has been primarily engaged in the evolution of adaptations, and if one result of its work has been that of enabling the systematist to trace lines of genetic descent under his divisions of species, genera, and the rest, such a result is but secondary or incidental. A wing, for example, is an adaptive structure which is formed on at least four completely different plans in different classes of the animal kingdom; and it is the function of natural selection as a theory to explain all this variety of adaptive structure, with its infinite number of subordinate variations through the different forms in each class, whether "species" or otherwise. Now, I say that such a theory is first of all a theory of the evolution of adaptations, even though it be conceded that all species exist in virtue of differing from one another in respect of adaptations, and hence that the theory becomes *also* a theory of the evolution of species, as it is *also* a theory of the evolution of genera, families, &c. Take a parallel instance. If a man were to define the nebular theory as a theory of the origin of Saturn's rings, an astronomer would tell him that his definition is much too limited. The theory is, indeed, a theory of the origin of Saturn's rings; but it is so because it is a theory of the origin of the entire solar system, of which Saturn's rings constitute a part. Similarly, the theory of natural selection is a theory of the entire system of organic Nature in respect of adaptations, whether these are distinctive of particular species only, or likewise common to any number of species. In short, it is "*primarily*" a theory of adaptations *wherever these occur*, and only becomes "*also*" or "*incidentally*" a theory of species in cases where adaptations happen to be restricted in their occurrence to organic types of a certain order of taxonomic division.

This, I think, is enough to justify my definition in a formal or logical sense. But as Mr. Huxley's criticism involves certain questions of a material or biological kind, I should like to take this opportunity of considering what he has said upon them. Therefore I will now pass on to the second head of my answer.

II. Hitherto, for the sake of argument, I have conceded that, in the words of my critic, "it is a necessary consequence of the theory of selection that every species must have some one or more structural or functional peculiarities" of an adaptive kind. But now I will endeavour to show that this statement does not "follow as a necessary consequence" from "the theory of selection."

Be it observed, the question which I am about to consider is not whether "every species which exists, exists in virtue of adaptation" common to its genus, family, order, class, or sub-kingdom. The question is whether every species which exists, exists in virtue of some advantageous "*peculiarity*" or adaptive advantage *not shared by its nearest allies*. In other words, we are not disputing whether it is a necessary consequence of Mr. Darwin's theory that all "species" must present "adaptations." This, of course, I fully admit. But what we are disputing is, whether it is a necessary consequence of Mr. Darwin's theory that every species must present at least one adaptive character (or combination of adaptive characters) *peculiar to itself alone*. Now, such being the question, let us consider Mr. Huxley's treatment of it.

Most obviously "it follows" from the theory of selection that "every variety which is selected into a species is favoured and preserved in consequence of being, in some one or more respects, better adapted to its surroundings than its rivals." This, in fact, is no more than a re-statement of the theory itself. But it does not follow that "every species which exists, exists in virtue of adaptation" peculiar to that species; *i.e.* that every species which exists, exists *in virtue of having been "selected."* This may or may not be true as a matter of fact: as a matter of logic, the inference is not deducible from the selection theory. Every variety which is *selected into* a species must, indeed, present some such peculiar advantage; but this is by no means equivalent to saying, "in other words," that every variety which *becomes* a species must do so. For the latter statement imports a completely new assumption—namely, that every variety which becomes a species must do so because it has been selected into a species. In short, what we are here told is, that if we believe the selection principle to have given origin to *some* species, we must further believe, "*as a necessary consequence*," that it has given origin to *all* species.

Not to perceive a consequence so necessary is said to betray a fundamental misunderstanding of the first principles of Mr. Darwin's theory. Perhaps, therefore, it is worth while to consider the matter from another and less formal point of view.

It surely is no essential part of Mr. Darwin's theory to deny that isolation (in all its kinds) may lead to the survival of new varieties, and so, in some cases, to the origin of new species, which need not necessarily present any change in the adaptive characters respectively inherited from their parent stocks. Under isolation, and the consequent absence of what Prof. Weismann has called panmixia, there is much reason to believe that new "structural or functional peculiarities" may arise (whether by direct action of changed conditions, by independent variation in the absence of panmixia, or by both these principles combined) which are without any adaptive significance; and I cannot see why it should be held to constitute any essential part of Mr. Darwin's theory to deny that such is the case. No one, I suppose, will venture to express a doubt that there are named species, both of plants and animals, which have been formed under isolation, and which experiments—such as those recently made with our severally-isolated forms of British trout—would prove to be but "local varieties," capable of being changed one into another by mere change of habitat, without any question of "selection" being so much as possible. Here it is the direct action of changed conditions which induces modifications of type sufficiently pronounced to take rank as distinct species in the eyes of a systematist; and the only difference between such a case and one where the modifications are due to independent variation is that in the former case their non-adaptive character admits of being proved by experiment. According to the general theory of evolution, there is no distinction to be drawn between a local variety and a new species, save as regards the extent to which modification may have proceeded. If, therefore, as in the case of the trout, mere change of habitat from one district of Great Britain to another (apart from any "selection") is able to induce modifications sufficient in amount to have been ranked as species by expert ichthyologists, much more may this frequently be the case under geographical isolation in larger areas, with exposure to different climates, and subject to the superadded influence of independent variation.

I have good reason to be well aware that great differences of opinion are entertained by different naturalists touching the degree of importance which should be assigned to isolation as a factor of organic evolution; and in one of the very last issues of NATURE, Mr. Wallace presents with great lucidity the view that isolation alone can never originate a new species by independent variation without the unavoidable intervention of natural selection, seeing that "at each step of the divergence" there must be "necessarily selection of the fit" from the less fit (September 20, p. 491). I will not wait to show that, if in an isolated section of a species no new peculiarities should be required to render its constituent individuals more "fit," selection need not necessarily effect any change with regard to adaptive characters; nor need I remark that even when selection is enabled to effect such a change under such circumstances, it does so *because it is assisted by isolation*, thus becoming, not *the* cause, but *a con-cause* of "the origin of species." A great deal could be said on both these points; but, for the sake of brevity, I will take my stand on the bare fact that, according to the general theory of evolution, a local variety is what Mr. Darwin calls "an incipient species"; and, on the ground of this fact, I ask where the line is to be drawn between varieties and species in respect of adaptive characters? If no answer can be given, we must take it from Mr. Huxley, as "a necessary consequence of the theory of selection," that every *variety* "which exists, exists in virtue of adaptation." Thus, to take but two illustrations from among several that might be drawn from the trout just alluded to, when two lots of "Lochlevens" were placed in two separate ponds within a very short distance of each other, and exposed, as far as could be ascertained, to parallel conditions of life, remarkable—but in no conceivable respect adaptive—differences in coloration were developed between the trout which respectively inhabited the two ponds ("British and Irish Salmonidæ," pp. 226–27, 1887). Will anyone undertake to affirm, after looking at the coloured plates, that these changes must necessarily have been due to selection? Again, in a recent communication to the *Field* (July 7), Mr. Day gives an engraving of a remarkable variation which is taking place in the gill-covers of trout which have been transported to New Zealand, and there "turned down" under nature. Premising only that, although this is a change of structure, there is no more adaptive meaning to be found in it than in those changes of colour above mentioned,[1] I will quote Mr. Day's remarks upon the subject: "It will be interesting to watch the changes occurring among these trout in their new home, and to observe whether these serrations are continued or merely temporary; for if they should become developed with time there would be still more reason for constituting them a new species than now exists among the various European races; while, should trout with serrated preopercles and interopercles be admitted as constituting a new species, we could now trace the process of development from its commencement, and show how such has been occasioned by transplanting our European trout to the warmer waters of the Antipodes."

Should it be objected that, as a matter of fact, the state of matters anticipated by Mr. Day has not yet arrived, my answer would be obvious—namely, *supposing that such a state of matters had arrived*, could the fact be reasonably held to annihilate the whole theory of natural selection? Yet this is what such a fact would *necessarily* do, if we hold it to be "a *necessary consequence of the theory*" that every species which exists, exists in virtue of having been "*selected*." If we have not here a *reductio ad absurdum*, I do not know how one can ever hope to apply that method.

Of course I am not disputing that in general there is a very great distinction between local varieties and good species in respect of peculiar adaptive characters. In other words, I have no doubt at all that probably the great majority of of species have been originated by natural selection, either as the sole cause or in association with other causes. But the allegation which I am resisting is, that it follows as a necessary consequence *from the theory of selection itself* that *every* species *must* owe its origin to selection. And I have endeavoured to show that this allegation admits of being reduced to an absurdity. When Mr. Wallace, in the letter above referred to, expresses dissent from Mr. Gulick's view that species are frequently originated by the influence of isolation alone, he adds: "If this is a *fact*, it is a most important and fundamental fact, equal in its far-reaching significance to natural selection itself; I accordingly read the paper with continual expectation of finding some evidence of this momentous principle, but in vain." Now, supposing that Mr. Wallace had found the evidence which would have fully satisfied him, would he therefore have been logically required to abandon his own great generalization? Would he have been required to acknowledge, not only, as he says, a principle "equal in its far-reaching significance to natural selection itself," but a principle which altogether superseded that of natural selection? I say it is absurd to suppose that such would have been the case, and yet it must *necessarily* have been the case if it be "a necessary consequence" of his theory that *all* (if *any*) species are originated by selection.

It will be remembered that I am not arguing the biological question whether, or how far, species exist which do not owe their existence to selection; I am arguing only the logical question whether it is "a necessary consequence of the theory of selection" that they cannot. And I now submit that it no more follows from the selection theory alone, that "every variety" which *becomes* "a species" does so "in consequence of being in some one or more respects better adapted to its surroundings than "*its existing contemporaries*, than it does that every variety which becomes a variety does so for the same reason. If the former statement is a statement of biological fact (which, for my own part, I do not believe), the fact is one that would stand to be proved inductively as a fact: it cannot be made good by way of logical deduction "from the theory of selection."

[1] In this connection, also, it is of great importance to remember that it is only twenty years ago since the trout in question were sent to New Zealand, and their fry liberated in the waters there; for the most ardent upholder of the theory of natural selection as the *sole* cause of specific transmutation will scarcely maintain that twenty years is long enough for survival of the fittest to effect a structural change of an "unknown" adaptive character in a long-lived animal with all the waters of New Zealand to spread over.

I have thus dealt with Mr. Huxley's criticism at some length, because, although it has reference mainly to a matter of logical definition, and in no way touches my own theory of "physiological selection," it appears to me a matter of interest from a dialectical point of view, and also because it does involve certain questions of considerable importance from a biological point of view. Moreover, I object to being accused of misunderstanding the theory of natural selection, merely because some of my critics have not sufficiently considered what appears to them a "paradoxical" way of regarding it.

GEORGE J. ROMANES.

How Sea-Birds Dine.

As I have ascertained that the following fact is not well known, I send you this account in the hope that it may be of interest to naturalists and to the general public. Anyone who lives in the Western Hebrides will have often watched on a calm day the sea-birds feeding with noisy clamour in the sea-lochs and about the numerous islands. This is especially the case in August, when the shoals of small herring are very plentiful. Some years ago, when in a sailing-boat off the west coast of Mull, I caught with a hand-net a dishful of these small fry as they swam along the surface of the water. Last year, noticing from a steam-launch the birds congregated in great numbers at one spot, the idea struck me to steam to the place and try to get a share of the birds' repast. The idea was at once carried out. I stood on the prow with landing-net in hand, and the launch wa steered towards the birds. As we drew near, the banqueters flew away with evident dissatisfaction at the interruption, a few of the more greedy making their last hasty dives. In another moment we were at the spot, and I saw, to my intense surprise, about 2 feet under the surface, a large reddish-brown ball, 2 to 3 feet in length and 2 feet in depth. I made a frantic swoop with the net into the ball, and brought on deck half a pailful of the sea-birds' dinner. Even as we passed we could see the great living ball sinking and breaking into pieces. This year I and others have tried the same spot with great success. Sometimes the ball has sunk too deep to be reached; sometimes there was no ball to be seen; but on the most successful day I filled a pailful in three hauls. In September we saw no ball, because, perhaps, the fish had grown too large for the birds to manage. As far as I can judge, the *modus operandi* is carried out by the divers, who surround a shoal and hem them in on all sides, so that the terrified fish huddle together in a vain effort to escape inevitable destruction. The divers work from below and other sea-birds feed from above; and, as in some cases after the birds had been at work for some time I saw no ball, I suppose not one fish is left to tell the tale. I must leave to naturalists the real explanation of the matter; but I may mention that, when disturbed by the boat, the divers seem to come to the surface in a great ring round the scene of their feast. I may also mention that once, when the boat was still 300 or 400 yards away, the birds suddenly rose and whirled about with frightened screams. I wondered what could be the cause, until I saw the round back of a porpoise rolling lazily round at the exact spot, and then rolling back again. When we steamed past there was no sign of a ball. What two delicious mouthfuls for the porpoise!

COMPTON.

Loch Luichart, Ross-shire, N.B.

The Zodiacal Light.

MR. O. T. SHERMAN gives an interesting communication on the zodiacal light in NATURE of October 18 (p. 594), and asks for reference to any observations. He alludes to Cassini. The following extract from a letter by Cassini may not have come under his notice: "It is a remarkable circumstance that since the end of the year 1688, when this light began to grow fainter, spots should have no longer appeared on the sun, while in the preceding years they were very frequent, which seems to support, in a manner, the conjecture that the light may arise from the same emanations as the spots and faculæ of the sun." This does not quite tally with Mr. Sherman's notion that the maxima of the zodiacal light coincide with the minima of sun-spots. May it not rather be that, supposing sun-spots to be largely occasioned by increased influx of meteoric matter falling into the sun, which matter gets sublimed and repulsed to augment the materials forming the zodiacal light, therefore the maxima of the latter may then lag behind the maxima of the sun-spots.

HENRY MUIRHEAD.

Cambuslang, October 20.

The Geometric Interpretation of Monge's Differential Equation to all Conics.

NEITHER the note of Prof. Asutosh Mukhopadhyay in NATURE of the 11th inst. (p. 564), nor that of Lieut.-Colonel Allan Cunningham in the number of August 2 (p. 318), has satisfied me that the criticism implied in my short note (June 28, p. 197) on the Professor's first note (June 21, p. 173) is unfounded. Permit me, therefore, to develop that criticism a little more at large.

I have not yet had an opportunity of referring to the papers of the Professor in the Proceedings of the Asiatic Society, but from what I can gather as to their contents from his notes in NATURE, I am in no way disposed to underestimate the accuracy or the value of his results. It is only to his claim to find in them "*the* true interpretation of Monge's differential equation to any conic" that I demur.

To my apprehension the interpretation in question is a *truism*, not a *truth*. What has been put into the question as a *definition* emerges afterwards, as might have been anticipated, as an *interpretation*. If the Professor has given a definition of *aberrancy*, independent of a conic and its known properties, of course I am wrong; but I gather from his note that by *aberrancy* he merely means (if I may thus express it) *deviation from conicity*. Whatever measure of *aberrancy*, then, he adopts for curves generally, must necessarily become zero for a conic, which has, from the very meaning of the words, no "deviation from conicity."

The difference, as I conceive it, between an interpretation properly so called and an interpretation that is a mere *truism*, may be clearly illustrated by the case of the circle. The Professor tells us that "the differential equation of all circles $(1 + p^2)r - 3pq^2 = 0$, means that the angle of aberrancy vanishes at every point of every circle." If thus read, what I have said above applies, and the interpretation is but a *truism*. It admits, however, of a different reading. For it is easy to show that $(1 + p^2)r - 3pq^2 = (1 + p^2)^3 \frac{d^2\phi}{ds^2}$, where s, ϕ are the usual intrinsic co-ordinates of the curve, so that the differential equation is equivalent to $d^2\phi/ds^2 = 0$. Now $d\phi/ds$ is the measure of the curvature of a curve, defined as the rate of change, per unit of arc, of the inclination of the tangent to a fixed direction, a *definition which is quite independent of the circle*; and $d^2\phi/ds^2$ is the rate of change, per unit of arc, of the curvature. Hence the equation $d^2\phi/ds^2 = 0$, being true at every point of every circle, expresses the *truth* that in a circle there is no change of curvature from point to point—or, in other words, the property that the curvature of a circle is the same at every point. I submit that this, rather than the Professor's, involving the notion of *aberrancy*, has a right to be regarded as the true interpretation of the equation.

In like manner, the true interpretation of the differential equation to a conic, if it ever is discovered, will express that some magnitude or concept connected with a curve, and defined independently of the particular curves, the conic sections, vanishes at every point of every conic.

Even admitting the Professor's interpretation, I agree with Colonel Allan Cunningham that it has no prerogative right over others of the same character to be called *the* interpretation of the equation. To go no farther, any number of "aberrancy curves" may be imagined; as, for instance, the locus of the focus, instead of the centre, of the osculating conic, for which it will be true that "the radius of curvature of the aberrancy curve vanishes at every point of every conic"; for in fact, in this case the aberrancy curve degenerates into a single point, and to say that the radius of curvature vanishes, or that the curvature is infinite, at every point of a curve, is, to my apprehension, only a roundabout, and not very instructive, way of saying that the curve becomes reduced to a single point.

Harrow, October 13. R. B. H.

A Shadow and a Halo.

THE following notices of anthelia may be interesting to the readers of NATURE. Frances Ridley Havergal thus described a sunset on the Faulhorn: "At one juncture a cloud stood still, apparently about two hundred yards off, and we each saw our own shadow gigantically reflected on it, surrounded by a complete rainbow arch, a full circle of bright prismatic colours, a transfiguration of our own shadows almost startling; each, moreover, seeing only their own glorification" ("Swiss Letters and Alpine Poems").

Tennant, in his book on Ceylon, states that this curious phenomenon, which may probably have suggested to the early painters the idea of the glory surrounding the heads of beatified saints, is to be seen in singular beauty at early morning in Ceylon. When the light is intense, and the shadows proportionally dark, when the sun is near the horizon, and the shadow of a person is thrown on the dewy grass, each drop of dew furnishes a double reflection from its convex and concave surfaces; and to the spectator the shadow of his own figure, but more particularly the head, appears surrounded by a halo as vivid as if radiated from diamonds.

S. T. Coleridge described the phenomenon thus:—

"Such thou art, as when
The woodman winding westward up the glen
At wintry dawn, where o'er the sheep-track's maze
The viewless snow-mist weaves a glist'ning haze,
Sees full before him, gliding without tread,
An image with a glory round its head:
The enamoured rustic worships its fair hues,
Nor knows he makes the shadow he pursues."

Benvenuto Cellini saw, probably, this phenomenon, and supposed it peculiar to himself. F. Robertson cites it as a proof of inordinate vanity. He says: "Conceive a man gravely telling you that a vision of glory encircled his head through life, visible on his shadow, especially on the dewy grass at morning, and which he possessed the power of showing to a chosen few" ("Life and Letters of F. Robertson," vol. ii. p. 192).

Bardsea, October 22. EDWARD GEOGHEGAN.

I HAVE frequently, on the South Downs, seen a halo round the shadow of my head, as described in your last number by Mr. A. S. Eve. I have noticed that the further off the shadow, the brighter is the halo. I have also observed, when looking at my shadow in the sea, that rays of light appear to surround the shadow of my head. CHARLES CAVE.

Ditcham Park, Petersfield, October 22.

On the Grass Minimum Thermometer.

THE average readings of the self-recording grass minimum thermometer for every month during the past three years have been compared with the average minimum *damp* bulb temperatures, obtained from the means of hourly readings, and the following figures show the corrections to be applied to the latter in order to obtain the former:—January $-0°.3$, February $+0°.3$, March $-0°.3$, April $-0°.8$, May $-0°.2$, June $-1°.1$, July $-1°.1$, August $-0°.9$, September $+0°.2$, October $+1°.4$, November $+1°.9$, December $+0°.4$.

The grass minimum is nearly a degree below the damp bulb minimum in the wet season, and nearly 2° above it in the driest month. The comparison between the minimum air temperature and the minimum on grass does not measure the terrestrial radiation, although the difference is to some extent influenced by radiation. Moreover, the epochs of the two minima need not coincide—*e.g.* in Hong Kong the early morning hours are more cloudy than the evening hours.

During the daytime in summer the thermometer, exposed an inch above the short grass, shows as a rule temperatures rising to 120° or 130°, especially in calm weather; but even when it is not perfectly calm, the force of the wind is not felt so near the ground, from which the air rises laden with minute particles of dust, which are observed adhering to the cloth of damp bulbs and other objects cooled by evaporation, and which may occasionally be smelt in the air. At night such minute particles would of course tend to return to the ground, and the unhealthy character of the ground-fog during early morning hours in tropical countries may be intensified by this circumstance.

Hong Kong Observatory, W. DOBERCK.
September 10.

ON THE ELECTROMOTIVE VARIATIONS WHICH ACCOMPANY THE BEAT OF THE HUMAN HEART.

THE observation of these variations is extremely easy, the only requisite being a sufficiently sensitive capillary electrometer.[1]

[1] The electrometers I used were made by Mr. Dean, glass-blower, 8 Cross Street, Hatton Garden.

The successful issue of the observations is so certain that they can be best described in the form of directions to a person who should be desirous of seeing them for himself, followed by the prediction of what will be observed by him.

§ I. Two vessels of salt solution are to be prepared, and connected with the capillary electrometer by electrodes. The various extremities of the observer are to be dipped into the salt solution, while the capillary column is watched. Electrical variations, apparently synchronous with the heart's pulse, will be observed with certain combinations rather than with others, and the results (on a normal person with the heart pointing to the left) will be as follows:—

Connect with electrometer—

1. Left hand and right hand	Electrical variations
2. Left hand and left foot	Little or no variations
3. Left hand and right foot	Little or no variations
4. Right hand and left foot	Electrical variations
5. Right hand and right foot	Electrical variations
6. Right foot and left foot	No electrical variations

will be apparent.

Further observations may be made with the mouth used as a leading-off point in connection with each of the four extremities. To lead off from the mouth a silver electrode coated with silver chloride is kept under the tongue. The results will be as follows:—

Connect with electrometer—

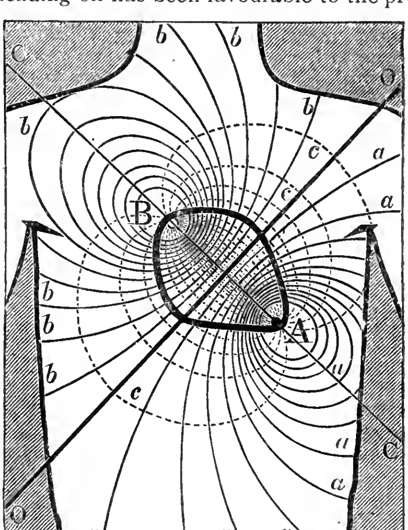
7. Mouth and left hand	Electrical variations
8. Mouth and right hand	Little or no variations
9. Mouth and left foot	Electrical variations
10. Mouth and right foot	Electrical variations

will be apparent.

Finally, it is possible to add to the evidence obtained, by using the rectum as a lead off by means of a silver electrode. This, if tried, would give with

11. Rectum and mouth	Electrical variations
12. Rectum and left hand	Little or no variations
13. Rectum and right hand	Electrical variations
14. Rectum and left foot	Little or no variations
15. Rectum and right foot	Little or no variations.

These will have been the results; the cases in which the mode of leading off has been favourable to the production



of electrical variations will be unmistakably distinguished from those in which the mode of leading off has been unfavourable.

The explanation of these facts is most shortly given in the diagram. C C is the axis of any current which must be produced if at any time the apex and base of the ventricles differ in potential. O O is the line of zero potential at right angles to C C.

a a a are equipotential lines round a supposed focus A. *b b b* are equipotential lines round a supposed focus B. Any lead off from two superficial points *a a* or *b b* is unfavourable. Any lead off from two points *a b* is favourable to the manifestation of electromotive differences originating at the heart. This will have been demonstrated by the experiments directed to be made.

§ II. On a quadruped (dog, cat, rabbit) the results will come out somewhat differently. The heart occupies an approximately median position, so that the asymmetry observed on man does not hold good with the above-named animals. In these the current axis will be along a median longitudinal line; the line of zero potential will be at right angles to it, *i.e.* transverse.

This can be verified by trial with very little trouble. A quadruped is led off by the various extremities and orifices immediately after death before the heart has ceased to beat; or a dog may be trained to stand quiet with his feet in dishes of salt solution (I have a large and well-disposed dog who will stand thus by the hour). However the test be made, the results will come out as follows:—

Connect with electrometer—

1. Left paw[1] and right paw	Little or no electrical variations
2. Left paw and left foot	Electrical variations
3. Left paw and right foot	Electrical variations
4. Right paw and left foot	Electrical variations
5. Right paw and right foot	Electrical variations
6. Right foot and left foot	Little or no electrical variations

will be apparent.

Extending the observations to mouth and rectum, the results will be thus:—

7. Mouth and left paw	Little or no electrical variations
8. Mouth and right paw	Little or no electrical variations
9. Mouth and left foot	Electrical variations
10. Mouth and right foot	Electrical variations
11. Mouth and rectum	Electrical variations
12. Rectum and left paw	Electrical variations
13. Rectum and right paw	Electrical variations
14. Rectum and left foot	Little or no electrical variations
15. Rectum and right foot	Little or no electrical variations.

§ III. Upon these two proofs may be piled a third proof of the correctness of the facts and of their explanation. Cases of *situs viscerum inversus* are to be found; the viscera of such people are situated as those of a normal person seen in a mirror; *i.e. inter alia*, the heart points to the right. I have examined two such cases, with results exactly as anticipated, viz. the favourable combinations, 4, 5, and 7, of a normal subject (§ I.) are unfavourable in the case of *situs inversus*, while the unfavourable combinations, 2, 3, and 8, are favourable. Combinations 1, 9, and 10 are favourable, and 6 is unfavourable in both cases, there being the notable peculiarity as regards 1 that the variations are reversed in direction in each of the two cases. The significance of this point will be obvious to the reader who has followed the facts up to this point: in both cases we have a favourable combination, but a reversal of points *a* and *b*.

§ IV. As regards the character and direction of each cardiac variation, it will be found to be composed of two phases, the first short, sharp, and difficult to read as regards direction, the second comparatively prolonged and easy to read. The second phase clearly indicates negativity of the heart's base, the first phase less clearly negativity of the heart's apex—facts which testify that the contraction begins at the apex and ends at the base of the ventricles. The auricular contraction does not affect any electrometer I have used.

[1] "Paw" is used as an abbreviation for anterior extremity; "foot" for posterior extremity.

If I may venture to forecast the manner in which these statements may receive from independent sources that verification which any statement requires before it can be accepted as a correct representation of fact, I should say that as regards § I. no contradiction will arise unless the first case tested should happen to be that of a person with the heart occupying an unusually median position, when the favourable and unfavourable cases, though still distinguishable, may be less so than if the heart occupied its usual oblique position pointing to the left. In any case, however, the variation will be found more marked with a favourable than an unfavourable combination. As regards § II., the statements made can be verified as soon as tested upon a recently killed cat or upon a properly educated dog. The verification of § III. only requires that a suitable case should be discovered. As regards the character of the variation, it is probable that its diphasic character may be overlooked at the first glance, but (in a favourable case) this character will soon be apparent. As regards direction, that of the second phase will be determined without much difficulty, but that of the first will be found very difficult to seize. I was not able to make up my mind about it until I had obtained successful photographs of the movements on a quick-travelling sensitive plate.

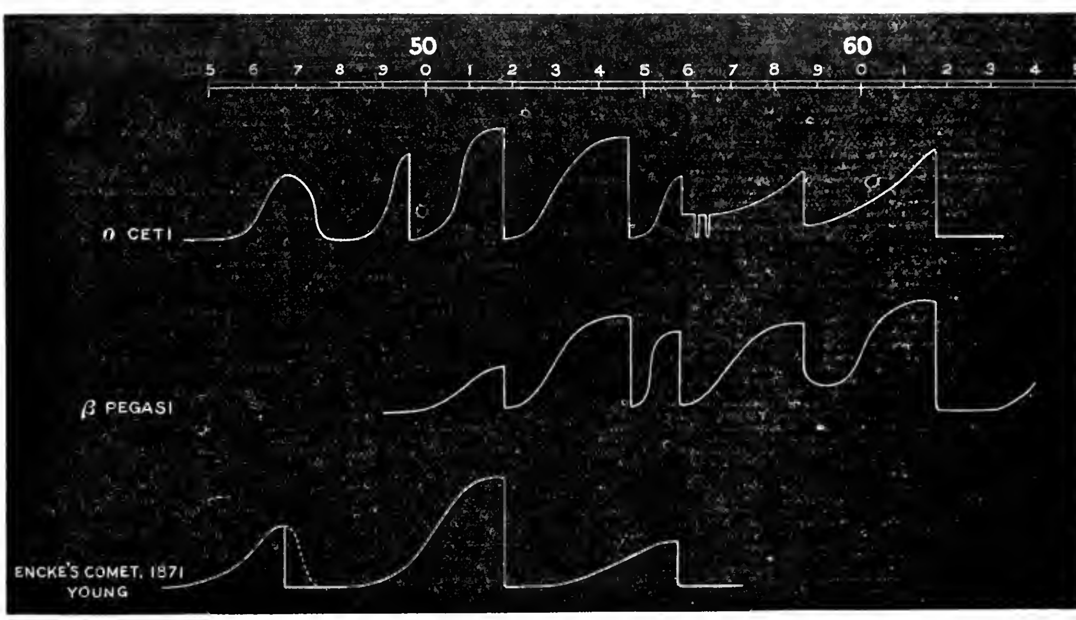
AUGUSTUS D. WALLER.

THE MAXIMUM OF MIRA CETI.

I AM anxious to call the attention of observers to the present spectrum of Mira, which arrived at its maximum brilliancy on the 15th inst. I pointed out recently (NATURE, May 24, p. 79) that stars of the group to which Mira belongs are sparse meteorite-swarms like comets, and that, when variable, the variability is produced by collisions between two swarms, the centres of which are nearest together (periastron passage) at maximum.

Broadly speaking, then, we may regard variables of this class as incipient double stars, or condensing swarms with double nuclei, the invisibility of the companion being due to its nearness to the primary, or to its faintness. It is obvious that variability will occur mostly in the swarms having a mean condensation, for the reason that at first the meteorites are too far apart for many collisions to occur, and that, finally, the outliers of the major swarm are drawn within the orbit of the smaller revolving one, so that it passes clear.

The present maximum of Mira tests my hypothesis, and its brightness is such that a small telescope and a Maclean's spectroscopic eye-piece are all that are necessary to see in how striking a manner the test is borne. The two brightest bands now visible are at λ 517 and λ 546, precisely where these are seen in the brightest comets. The former is the brightest carbon fluting seen in the spectrum of the Bunsen flame, or spirit-lamp,



and the other, at 546, is the citron carbon fluting beginning at 564, but modified by the masking effects of the manganese absorption fluting at 558, and also that of lead at 546.

The blackness of the spaces between the bright flutings shows that there can be very little continuous spectrum from the meteorites, and therefore that the absorption is that of the light of the carbon flutings.

The mean spectrum of Mira is that of a star like β Pegasi, which I have shown to consist of bright carbon flutings, and dark flutings of magnesium, manganese, iron, lead, and barium. In β Pegasi, as in Mira under mean conditions, the carbon is somewhat faint, but in α Herculis it is very bright. The general effect of the conditions of maximum of Mira therefore seems to be that of changing its spectrum from one like that of β Pegasi to one like that of α Herculis.

I observed that the principal carbon fluting at λ 517 was somewhat brighter on the 14th than on the 17th inst. In variable stars of this class the proof is now complete that the increase of luminosity is accompanied by cometary conditions, and that it is due to the increased radiation of carbon.

In the accompanying figure the spectrum of Mira is compared with that of β Pegasi and Encke's comet. In some comets the carbon fluting is cut off at 546, exactly as it is in Mira. The observations of Mira were made by myself at Westgate, those of β Pegasi by Mr. Fowler at the Astronomical Laboratory at South Kensington.

J. NORMAN LOCKYER.

FLORA OF THE KERMADEC ISLANDS.

UPWARDS of thirty years ago Sir Joseph Hooker published an account of the botany of Raoul or Sunday Island, one of the Kermadec Group (Journal of the Linnean Society, i. pp. 125–29), founded upon a small collection made by McGillivray and Milne, naturalists attached to H.M.S. *Herald*. This collection consisted of forty-two species, of which twenty were flowering plants, and the rest ferns and lycopods: and the most interesting circumstance connected with it was "the identity of most of the flowering plants, and all but one of the ferns, with those of New Zealand."

In 1885, Mr. J. T. Arundel presented to the Kew Herbarium a collection of fourteen species from Meyer, a small rocky islet about a mile and a half north of Sunday Island. Poor as it was, it contained half-a-dozen plants not previously known from the group, though they are all included in the collection referred to below.

Since then, no further light has been thrown on this insular flora, until the quite recent appearance (Transactions of the New Zealand Institute, xx. pp. 151–81) of a paper by Mr. T. F. Cheeseman, Curator of the Auckland Museum, New Zealand, a copy of which was kindly forwarded to the writer. Mr. Cheeseman was permitted, through the kind offices of Mr. Percy Smith, the Assistant Surveyor-General of New Zealand, to accompany the expedition despatched last year for the purpose of formally annexing the group to the colony of New Zealand. If Mr. Cheeseman has not succeeded in exhausting the botany of the Kermadec Islands, which, of course, is hardly probable, the undiscovered species cannot materially affect the question of the origin of the vegetation. But before giving the results of his investigations, it will be useful to indicate the position and extent of the islands.

There are four islands lying at great distances apart, between 29° 10′ and 31° 30′ S. lat., and stretching in a south-west and a north-east direction, like New Zealand itself, the nearest point of which is between 500 and 600 miles distant. Raoul or Sunday Island is the largest and the farthest from New Zealand, being twenty miles in circumference, and about 640 miles from Auckland, and a little less than that distance from Tonga. Macaulay, the next in size, is sixty-eight miles to the south-west of Sunday Island; and Curtis and L'Espérance, still farther to the south-west, are little more than rocks. The expedition failed to land on the last-named island, and the visit to Curtis Island was of very brief duration, hence the botany relates almost exclusively to Sunday and Macaulay Islands.

The group is of volcanic origin, and the greatest elevation in Sunday Island is 1720 feet, while Macaulay nowhere reaches quite half that height.

Altogether Mr. Cheeseman collected 115 indigenous vascular plants, eighty-four being phanerogams and thirty-one cryptogams, and only five of them were regarded as endemic. In addition to the foregoing, twenty-six species of naturalized plants, chiefly European weeds, were observed or collected.

Of the 115 indigenous species, no fewer than eighty-five are also found in New Zealand, though only fourteen of these are absolutely confined to the two localities. Forty-four species are found in Norfolk Island, forty of which also occur in New Zealand, and only two are apparently confined to Norfolk Island and the Kermadecs. Forty species extend to Lord Howe's Island, but thirty-four of these are also in New Zealand, and none of the peculiar plants of Lord Howe's Island reach the Kermadecs. Seventy-six of the species are common to Australia, sixty-three of them being also in New Zealand, and none of them otherwise peculiar to Australia. Lastly, forty-seven are found in Polynesia, and thirty-one of these also occur in New Zealand.

The foregoing data, as Mr. Cheeseman observes, point unmistakably to New Zealand as the source of the greater part of the flora of the Kermadec Islands. How the plants reached these islands is an interesting question. Mr. Cheeseman is prepared to admit a former north-western extension of New Zealand; but, after a careful examination of the evidence, he arrives at the conclusion that the Kermadec Islands have always been isolated, or, at least, have not formed part of any other land since the Secondary period. Spores of the ferns may have been conveyed by winds; and ocean currents and birds, it may well be conceived, have operated in stocking the islands with flowering plants. Most of the birds are New Zealand species, and the presence of Kauri logs, of different dates and brands, stranded on various parts of the beach, is convincing evidence of the direction of ocean currents. Moreover, the composition of the flora strongly supports this theory.

Sunday Island is the only one of the group on which there is anything approaching arboreous vegetation, and this, with the exception of a small area of the crater, is clothed with forest from the sea-shore to the tops of the highest peaks. The prevailing tree is *Metrosideros polymorpha*, one of the most characteristic trees of Polynesia, especially of the smaller islands, reaching the Sandwich, Marquesas, and Pitcairn Islands; but this particular species does not occur in New Zealand nor in Australia.

Next to the *Metrosideros* in abundance and conspicuousness is a palm, which Mr. Cheeseman thinks may be identical with the Norfolk Island *Rhopalostylis Baueri* (*Areca Baueri*). In some places this grows gregariously, forming large groves.

Ferns are everywhere abundant, varied, and luxuriant; and the endemic tree-fern, *Cyathea Milnei*, is very plentiful, and handsome withal, rising to a height of 50 or 60 feet. Prominent among the New Zealand trees are *Corynocarpus lævigatus*, *Myoporum lætum*, *Melicope ternata*, *Melicytus ramiflorus*, and *Panax arboreum. Cordyline terminale*, the widely-spread Polynesian "Ti," and *Pisonia Brunoniana*, *Pittosporum crassifolium*, *Coprosma acutifolia*, and *C. petiolata*, natives of New Zealand, are other elements deserving of notice.

The herbaceous vegetation includes no plants with very conspicuous flowers, but there are two orchids—namely, *Acianthus Sinclairii*, a native of New Zealand, and *Microtis porrifolia*, which also inhabits both New Zealand and Australia.

Macaulay Island was entirely covered with a beautiful sward of natural grass, supposed to be composed of a species of *Poa* and an *Agrostis*, but in the absence of flowers they were indeterminable.

Students of botanical geography will find much more that is interesting in Mr. Cheeseman's valuable paper, from which I have extracted the principal facts.

W. BOTTING HEMSLEY.

DIGITI MINIMI DECESSUS.

[*Sent by a Correspondent.*]

THE following lines appeared in the *Guy's Hospital Gazette* of October 13. The correspondent who sends them to us suggests that they may fitly find a place in NATURE, *à propos* of the controversy on "Prophetic Germs."

"*Man is losing his little toe, ... and can do without it.*"
—MR. CLEMENT LUCAS, in his opening lecture.

If thou must go, thou feeble, foolish digit,
Fain would I speed thy slow, degenerate way!
I daily feel a disagreeable fidget
Whenever I've occasion to display
Thy doubtful outline, and thy form chaotic
(Born of a taste in boots, perhaps erotic).

Thou art a shock to my æsthetic sense,
And offerest no kind of recompense
In way of use; of every function shorn,
Except to act as basis for a corn.
When thou art gone I'll still maintain my grace,
Still walk erect wherever I may be;
Still I'll belong to the athletic race,
Waltz with the fair, and kick mine enemy!
So *pace* Schopenhauers, and *pace* Mallocks
When I've acquired a hypertrophied hallux,
To monodactyle type thus simplified,
Life shall be simpler too, and so—beatified.

* * * *

When future science forgets thee in thy prime,
Methinks a great mind from a northern clime
May then discuss thy remnants, and declare
He finds a true *prophetic organ* there!

F. G. H.

NOTES.

WE lately (Sept. 6, p. 437) printed an account of the formation of the Australasian Association for the Advancement of Science. If we may judge from the newspaper reports which have now reached this country, the first general meeting of the Association seems to have been remarkably successful. The session began at the Sydney University on Tuesday evening, August 28. Lord Carrington opened the proceedings with a short speech, and then an address was delivered by Mr. H. C. Russell, the President. On the following day the sectional meetings began, and their work went on during the remainder of the week. About 110 papers were sent in by students of various branches of science, and a considerable number of them will be published in full in the first volume soon to be issued by the Association. The members had an opportunity of taking part in several pleasant excursions, and much hospitality was shown to visitors by leading citizens. At the time of the meeting there were about 850 members, and it is confidently anticipated that next year this number will be largely increased. The next meeting is to be held in Melbourne, and Baron Sir Ferdinand von Müller, the Government Botanist of Victoria, is the President-elect. In 1890 the Association will meet in New Zealand.

THE following is the list of names to be submitted, at the annual meeting (November 8) of the London Mathematical Society, for the new Council:—For President, J. J. Walker, F.R.S.; for Vice-Presidents, Sir J. Cockle, F.R.S., E. B. Elliott, and Prof. Greenhill, F.R.S. The Treasurer and Hon. Secretaries remain unaltered. The other members are: A. B. Basset, Dr. Glaisher, F.R.S., Messrs. J. Hammond, H. Hart, J. Larmor, C. Leudesdorf, and S. Roberts, F.R.S., Captain P. A. Macmahon, R.A., and Dr. Routh, F.R.S. It is proposed that the vacancies caused by the withdrawal of Lord Rayleigh, Sec.R.S., and the lamented recent death of Arthur Buchheim, shall be filled up by Messrs. Basset and Routh, as above.

H.M.S. *Jackal*, which has been engaged, under the direction of the Scientific Committee of the Scottish Fishery Board, in a cruise of physical investigation in the North Sea, recently returned to Granton. The course was along the east coast to the Orkney and Shetland Islands, and then to Bergen, Copenhagen, and Kiel. The physical work was carried on by Dr. Gibson, of the Chemistry Department of the Edinburgh University, assisted by Dr. Hunter Stewart and Mr. F. M. Gibson; and owing to the exceptionally favourable weather a large number of stations were formed at various parts of the route, at which series of temperature observations were taken, the density and alkalinity of the water determined, and samples preserved for analytical examination. Dr. Gibson had interviews with most of those conducting scientific fishery work in the countries visited, including Mr. Buch of Bergen, Dr. Paulsen, Lieut. Drechsel, Dr. Pettersen, and Mr. Feddersen of Copenhagen, and Prof. Karsten of the Kiel Commission; and we understand these conferences may result in closer co-operation between the various countries, in regard to the method and scope of scientific fishery investigations.

THE members of the International Commission of Weights and Measures have finished their session at the Pavillon de Breteuil, Paris. The making of standard metres is progressing, and next year they will be distributed to the various Governments. The guarantee of the Bureau extends to the thousandth of a millimetre and the ten-thousandth of a gramme.

THERE are now on the books of the Institution of Civil Engineers 1614 members, 2499 associate members, 458 associates, 19 honorary members, and 939 students, together 5529, being an increase at the rate of 3¾ per cent. during the past twelve months.

A SPECIMEN of the sword-fish (*Xiphias*) was captured some days ago in Long Reach, Milton Creek, Sittingbourne, by a bargeman. The fish measured 5 feet 2 inches from end of tail to tip of sword.

AN Agricultural and Industrial Exhibition was opened at Mysore by the Maharajah on the 17th inst.

AT a recent meeting of the Bombay Natural History Society, the idea of starting a Zoological Garden in that city was mooted by Mr. H. M. Phipson, the Honorary Secretary of the Society, and was warmly taken up. It was stated that the Society has been compelled to refuse large numbers of valuable specimens of animals offered to it. All that is asked from the Government is that they shall grant a site, and it is hoped that they may see their way to do so.

DR. J. C. COX lately described, at a meeting of the Linnean Society of New South Wales, two very remarkable female figures, modelled in wax, obtained in an aboriginal camp at Miriam Vale, near the head of the Calliope River, Rockhampton. These figures are said to be the only examples of plastic art ever discovered among the Australian aboriginals.

IN the Report of the Superintendent of the Adelaide Botanic Garden for the past year it is stated that the insect-powder plant (*Pyrethrum cinerariæfolium*, Trevir.), *roseum*, and *carneum*, Bibrst.), and the cheesemaker (*Withania coagulans*, Dun.), which were introduced into the Garden a few years ago, have found a congenial climate there, and have prospered wherever they were planted in the colony. Eland's Boontges (*Elephantorrhiza Burchellii*, Benth.), which has also been recently introduced, does fairly well. In winter nothing remains of this plant but the roots, which contain tannic acid. A number of cuttings from the Daira grape, a valuable species which comes from Almeria, have thriven wonderfully in the Garden. There are now in the palm-house 180 species and varieties of palms. The Museum of Economic Botany attached to the Garden has been enriched during the past year by 1795 articles, amongst the more remarkable of which was a collection sent by the Sultan of Johore, one of the specimens being a sample of sugar prepared from the cocoa-nut.

STUDENTS of the Caucasian languages will be glad to learn that the second volume of Baron Uslar's work, "The Ethnography of the Caucasus," has been published at Tiflis. It contains his "Tchetchen Language," and, in an appendix, several articles on the epics of the Caucasian mountaineers, on the study of the Caucasian languages and their alphabets, as also a translation of Schiefner's "Tchetchensche Studien," and a collection of Tchetchen proverbs and tales about Nasr-eddin, by J. Bartolomei.

IN connection with the discussion on "Valency" at the Bath meeting of the British Association, referred to in last week's NATURE, Prof. Meldola read a paper on the constitution of the azonaphthol compounds, in which he drew attention to the fact that the properties of these important colouring-matters could only be satisfactorily explained by admitting that they contained oxygen in the tetravalent condition.

THE vapour-densities of the chlorides of chromium have, for the first time, been determined by Profs. Nilson and Pettersson, of Stockholm. The interest attaching especially to the chromic chloride, hitherto known as Cr_2Cl_6, in view of the recent re-determinations of the densities of the corresponding chlorides of aluminium and iron, gives more than secondary importance to the work of the Swedish chemists. Readers of NATURE will remember that these recent experiments by the indefatigable workers just mentioned, and by Prof. Victor Meyer and his co-workers at Göttingen, upon the composition of the molecules of the chlorides of aluminium and iron, resulted in the conclusion that the double formulæ, Al_2Cl_6 and Fe_2Cl_6, must be abandoned in favour of the simpler formulæ, $AlCl_3$ and $FeCl_3$. This, of course, meant that our old notions as to the tetrad nature of these elements were incorrect, and that in reality they behave as triads. Profs. Nilson and Pettersson now clinch the matter by showing that chromium, which in many respects so much resembles aluminium and iron, behaves in precisely the same way. Chromic chloride was fortunately obtained in beautiful laminated crystals of almost perfect purity. The minute traces of absorbed moisture were readily eliminated by gently warming in a current of dry carbonic acid gas; when this was accomplished the requisite quantity was weighed out into a small platinum capsule in those experiments which were conducted in the platinum density apparatus, and in small pieces of ignited porous tubing when the porcelain apparatus was employed. The chloride was found to vaporize very slowly indeed at 1065° C., precluding the possibility of taking densities below that temperature; however, at this comparatively low temperature, the density was 6·135. Now $CrCl_3$ corresponds to a density of 5·478, while Cr_2Cl_6 must of necessity require a number twice as great, and hence cannot exist in the gaseous state. On increasing the temperature to 1190°, the value of 5·517 was obtained, which remained practically constant up to nearly 1300°. Over 1300° the molecules of $CrCl_3$ commence to break up into those of $CrCl_2$ and free chlorine. This is a most decisive result, and one which cannot possibly lead to any other conclusion than the adoption of the formula $CrCl_3$. It is only fair to mention that Messrs. Friedel and Crafts on carrying out vapour-density determinations of aluminium chloride by Dumas's method for 250° above its boiling-point (183°), have very recently obtained results which appear to indicate that this chloride may condense to the double molecule Al_2Cl_6 at these comparatively low temperatures. However this may be, there can be no doubt in the cases of iron and chromium that the triad formula is the only one compatible with experiment, and we shall be very glad to see the doubt in case of aluminium completely cleared up by further experiments. The determinations in the case of the lower chloride of chromium, $CrCl_2$, have been made under great experimental difficulties. This substance is the most difficultly volatilized of any yet submitted to vapour-density determinations. It required the most intense heat of the hottest procurable furnace, and even then was only very slowly converted into vapour. It was obtained perfectly pure by reduction of the chromic chloride utilized for the former experiments, by gently heating in a stream of hydrogen. At the lowest observable temperature, 1300°–1400° C., the density was found to be 7·8, considerably lower than the number required by Cr_2Cl_4. On further increasing the heat to 1600°, the density gradually diminished to 6·2, showing that at some still higher temperature one would finally attain the value 4·25 corresponding to $CrCl_2$. Hence chromous chloride again resembles ferrous chloride, the only difference being that the former is much more difficult to vaporize.

AN exceedingly useful and handy *résumé* of results in the "modern geometry of the triangle" is published in the just issued Proceedings of the Association Française pour l'Avancement des Sciences, Congrès de Toulouse, 1887. It is entitled "Premier Inventaire de la Géométrie du Triangle," by M. E. Vigarié. A second "Inventaire," which the author proposes to draw up, will be occupied with the extensions to certain (as Harmonic) quadrilaterals and polygons, and to space figures.

WE have received Part 3 of "A Catalogue of the Moths of India," compiled by E. C. Cotes, First Assistant to the Superintendent, Indian Museum, and Colonel C. Swinhoe. Of the first two parts, dealing respectively with Sphinges and Bombyces, we have already given some account (NATURE, vol. xxxvii. p. 386). The present part deals with Noctues, Pseudo-Deltoides, and Deltoides.

THE Trustees of the Australian Museum, Sydney, have issued Part I. of a catalogue of the fishes in the collection of the Museum. It relates to recent palæichthyan fishes, and has been compiled by Mr. J. Douglas Ogilby.

A REMARKABLE book on "The Butterflies of the Eastern United States and Canada, with especial reference to New England," by S. H. Scudder, of Cambridge, Mass., U.S.A., is about to be published in monthly parts. It will be completed in twelve parts, the first of which will appear in November. The preparation of this elaborate work was first announced by the author in 1869. Since that time he has had it always in hand, and during the last eight years he has devoted to it undivided attention. According to the prospectus which has been issued, Mr. Scudder has not only availed himself of the personal aid of a host of willing friends and correspondents, who have confided to him their voluminous field notes and numerous specimens, but he has carefully gleaned every fact of value from the natural history journals and other publications, and supplemented all by his thirty-five years' experience in the field. It is claimed that no systematic work on butterflies has ever appeared in any language comparable with it in the complete elaboration of a single limited fauna, in attention to every stage of life, in thorough and excellent illustration of every period of the butterfly's existence, and in careful detail of all structural features. The book will contain seventeen plates of butterflies, six of eggs, eleven of caterpillars, two of the nests of caterpillars, three of chrysalides, two of parasites, thirty-three of structural details in all stages of life, nineteen maps and groups of maps to illustrate the geographical distribution of the butterflies, and three portraits of early naturalists of America—in all, about two thousand figures on ninety-six plates, of which forty or more will be coloured. The printing of the plates was begun three years ago, and is now nearly finished.

A THIRD edition of Mr. R. Milne Murray's "Chemical Notes and Equations" (Maclachlan and Stewart, Edinburgh) has been issued. The book is intended for the use of students. In this edition a section on the electrolysis of salts has been introduced, and some additions have been made to the descriptive part of the work.

THE latest number (No. 3, vol. iii.) of the Journal of the Bombay Natural History Society contains, amongst other papers: unscientific notes on the tiger, by J. D. Inverarity; butterflies and ants, by Lionel de Nicéville; on the Lepidoptera of Karachi and its neighbourhood (part 2), by Colonel Swinhoe; notes on some bees and wasps from Burmah, by Captain C. T. Bingham; notes on the origin of the belief in the bis-cobra, by G. A. Da Gama. Mr. Da Gama says that the term bis-cobra is not of Oriental origin, but is a contraction of the Portuguese *bicho-de-cobra*. The early Portuguese settlers in India named the animals they met with from their most prominent features.

Thus, the *nag* they called, on account of its hood, *cobra-de-capello;* the *Daboia*, on account of its carpet-like skin, they called *cobra-de-alcatifa*—that is, the carpet-snake. From old Portuguese writings he believes that the mangoose is the bis-cobra; and from the crawling motion of that animal the Portuguese had an idea that the *bicho-de-cobra* was a lizard. In fact, in a work of the Jesuit father De Souza, published in 1710, though probably written twenty years earlier, the mangoose is described as "that poisonous reptile, *bicho-de-cobra.*" The name mangoose gradually usurped the place of *bicho-de-cobra*, but among the natives the idea of a poisonous lizard called bis-cobra remained, and it has been handed down with terrible stories of its poisonous powers.

THE South London Microscopical and Natural History Club has published its seventeenth Annual Report. The Report includes abstracts of some interesting papers read at the meetings. The Committee say that during the past year there was a uniformly good attendance of members.

WE have received the third number of the series "Insect Life," issued by the Entomological Division of the United States Department of Agriculture. The object of this series is to exhibit the economy and life-habits of insects, especially in their relation to agriculture. Among the contents of this number are notes on the Rocky Mountain locust; a report on injury done by "roaches" to the files in the Treasury at Washington; further notes on the hop-plant louse (*Phorodon humuli*); and a paper suggesting steps towards a revision of Chambers's index to the described Tineina of the United States and Canada, with notes and descriptions of new species, by Lord Walsingham.

SOME time ago the Colorado Ornithological Association was formed, and through the efforts of its members a comprehensive list of the birds of Colorado, numbering about 350 species and sub-species, was soon prepared. This Society has now transformed itself into an organization with wider aims, and has assumed the name of the Colorado Biological Association. The objects of the Association in its new form are the detailed investigation and recording of the fauna and flora of Colorado, recent and fossil. The Association hopes to become the highest authority on all matters connected with the biology of the State, both from the scientific and the economic points of view, and through its Secretary and referees will place itself at the service of the scientific and general public in answering all questions within the scope of its investigations, and in identifying specimens that may be submitted for this purpose.

THE general Report, by Prof. Egoroff, on the observations made in Russia and Siberia during the eclipse of the sun of August 19, 1887, under the direction of the Committee of the Russian Physical and Chemical Society, is now published (in Russian) in the Journal of the Society (vol. xx. 6). Seven stations were provided with observers and instruments (at Wilno, Nikolsk, Tver, Petrovsk, Vyatka, Krasnoyarsk, and the Bay of Possiet), but only at three of them—Petrovsk, Krasnoyarsk, and Possiet—could the eclipse be observed in detail. Fourteen excellent photographs were taken at Krasnoyarsk, and of these two are reproduced in M. Egoroff's Report, as also several drawings of the corona which were made by hand at Polotsk, Vladimir, and places in Siberia. Various observations with regard to the position of the protuberances and the shape of the corona are given in the Report, and its general conclusions are as follows:—(1) The corona is not a merely optical phenomenon: it has a real existence, and it maintained its shape not only during the whole of the eclipse at each spot where it was observed, but also at spots as far distant from one another as Polotsk and Possiet (distance, 6000 miles). (2) The corona of 1887 is a representation of those coronæ which correspond to a minimum of spots on the sun. The like were observed in 1867 and 1878. Its peculiarities are interesting in connection with the question as to the structure of the sun and its corona. Mr. Norman Lockyer, in his work on "The Chemistry of the Sun," expresses regret that he could not see, in 1886, while in Grenada, those *panaches* on the poles of the sun which he had carefully studied in 1878. The photographs of M. Hamontoff (Krasnoyarsk) prove that those currents existed, and that they were well seen on August 19, 1887. (3) There is a correlation between the distribution of the rays of the corona and the position of protuberances. (4) The brilliancy of the light of the corona is of the same order as that of the full moon (as shown by several photometric measurements, and also by the visibility of α Leonis in the rays of the corona). (5) The spectrum of the corona was an uninterrupted one, with feeble Fraunhofer lines. Bright lines were not seen, except for a moment at Petrovsk, where M. Stonaewicz saw the green bright line; the cloudiness of the sky, which resulted in a great amount of reflected light, probably prevented the bright lines from being seen. (6) Polarimetric measurements require a bright sky; under other conditions false conclusions might be arrived at. (7) Both atmospheric pressure and temperature are lowered during the eclipse, the minimum coming at a later time than the middle time of the full eclipse.

A GREAT number of meteorological observations having been made during the eclipse at various places in Russia and Siberia, Prof. Heselius now sums them up in the same issue of the Journal of the Russian Physical and Chemical Society (xx. 6). It appears from the curves which he has drawn after having availed himself of observations made at twenty-five different stations, that the eclipse resulted in lowering the atmospheric pressure by about 0·2 mm., the minimum being reached a few minutes (about five to ten) after the time of the full eclipse. The fact is best explained by the condensation of vapour in the atmosphere. The temperature was lowered by an average of 1°·6 C. in the shade—the minimum being reached ten minutes after the full eclipse; and by about 8°·6 in the sun's rays—the minimum being attained in this case three minutes after the full phase of the eclipse. The force of the wind also was reduced, probably on account of the condensation of vapour in the atmosphere. The data as to the influence of the eclipse on the magnetic needle are contradictory. The influence of the eclipse on plants and animals was well pronounced. The *Acacia armata* folded its leaves, while the *Nicotianæ* and *Mirabilis jaloppa* opened their flowers. In the marshy spots of Siberia, such as Turinsk, the mosquitoes made their appearance, as they usually do in the evenings. The well-known facts as to the uneasiness and fear which are felt by higher animals were confirmed. On the whole, the Physical Society expected more important results when it organized meteorological observations at so many stations provided with physical instruments, but the weather was unfavourable to the work of the observers. Hilger's spectrograph for photographing the ultra-violet parts of the spectrum of the corona with the view of detecting traces of carbon and carboniferous compounds, could not be used on account of the weather.

THE same periodical contains a record of Prof. Mendelejeff's impressions during his balloon ascent at Klin. The Russian chemist saw the corona from his balloon for only twenty seconds. His view of the sun was unfortunately obstructed by a cloud.

THE Meteorological Council have published Part 5 of "Contributions to our Knowledge of the Meteorology of the Arctic Regions." The four previous parts contained principally the meteorological results furnished by the Franklin search expeditions which wintered to the eastward of longitude 120° W. between 1848–58, but also included the results available from the date of Sir W. E. Parry's expedition in 1819. Part 5 relates to the region of Behring Strait, and to the search expeditions in that direction between 1848–54. The whole series has been discussed in a uniform and most complete manner by Mr. R. Strachan, and all the available information relating to the physical phenomena, and to the movements of animals and birds, has been thoroughly exhausted. The work contains most valuable data for scientific inquiry, and for use in any future expeditions to those remote regions.

THE additions to the Zoological Society's Gardens during the past week include two Toque Monkeys (*Macacus pileatus* ♂ ♀) from Ceylon, presented by Mrs. Ellen Hodson; a Moustache Monkey (*Cercopithecus cephus* ♂) from West Africa, presented by Mr. Andrew Allen; a Common Otter (*Lutra vulgaris* ♀), British, presented by Mr. John Crisp; a Japanese Deer (*Cervus sika* ♂) from Corea, presented by Capt. H. C. Eagles, R.M.L.I.; three Virginian Opossums (*Didelphys virginiana* ♂ ♀ ♀) from North America, presented by Mr. G. F. Whateley, R.N.; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. George Berry; a Collared Mangabey (*Cercocebus collaris*) from West Africa, a Grey Ichneumon (*Herpestes griseus*) from India, two Cockateels (*Calopsitta novæ-hollandiæ*) from Australia, four Snow Geese (*Chen albatus*) from North America, a Larger Hill-Mynah (*Gracula intermedia*) from Northern India, deposited; four Radiated Tortoises (*Testudo radiata*) from Madagascar, purchased; an Indian Swine (*Sus cristatus*) from India, a Nilotic Trionyx (*Trionyx ægypticus*) from the River Nile, received in exchange.

OUR ASTRONOMICAL COLUMN.

THE RING NEBULA IN LYRA.—Prof. Holden reports that this object, as seen with the great Lick refractor, shows far more detail than had been detected either by Lassell with his 4-foot reflector, or by the Washington observers with the great 26-inch refractor. With these telescopes thirteen stars had been seen in an oval outside the ring, and one star had been seen within it. The 36-inch Lick telescope shows twelve stars within the ring or projected upon it, and renders it obvious that the nebula consists of a series of ovals or ellipses: first the ring of stars, then the outer and inner edges of the nebulosity, next a ring of faint stars round the edges of the inner ring, and last a number of stars situated on the various parts of the nebulosity and outer oval.

COMETS BROOKS AND FAYE.—The following ephemerides are in continuation of those given in NATURE, vol. xxxviii. p. 576:—

1888.	Comet 1888 *c* (Brooks). R.A. h. m. s.	Decl. ° ′	Comet 1888 *d* (Faye). R.A. h. m. s.	Decl. ° ′
Oct. 29	16 47 54	0 14·1 S.	7 53 14	8 22·3 N.
31	16 52 12	0 59·0	7 55 34	7 58·4
Nov. 2	16 56 26	1 42·0	7 57 48	7 34·6
4	17 0 33	2 23·2	7 59 53	7 11·0
6	17 4 36	3 2·7	8 1 50	6 47·5
8	17 8 35	3 40·6	8 3 39	6 24·3
10	17 12 30	4 17·0	8 5 21	6 1·3
12	17 16 21	4 51·8 S.	8 6 55	5 38·7 N.

COMET 1888 *e* (BARNARD).—The following ephemeris for Berlin midnight is by Herr A. Berberich (*Astr. Nach.*, No. 2861):—

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Oct. 28	5 40 6	3 48·7 N.	0·3370	0·1498	6·0
30	5 32 12	3 17·5			
Nov. 1	5 23 39	2 44·6	0·3317	0·1214	7·1
3	5 14 24	2 9·9			
5	5 4 28	1 33·4	0·3265	0·0949	8·2
7	4 53 50	0 55·3			
9	4 42 31	0 16·0 N.	0·3214	0·0716	9·4

The brightness at discovery is taken as unity.

AMERICAN OBSERVATORIES.—Prof. W. W. Campbell has been appointed to the position in the Observatory of Ann Arbor which was held by Mr. J. M. Schaeberle previous to his appointment as assistant at the Lick Observatory.

The Observatory at Iowa College, Grinnell, Iowa, possesses a fine equatorial of 8 inches aperture by the Clarks, and strong efforts are being made to obtain a transit-instrument and chronograph, and sidereal and mean clocks, so that a time service may be maintained.

The Carleton College Observatory, Northfield, Minnesota, is now a very well equipped institution, with transit and prime vertical instruments, besides the old equatorial of 8¼ inches, and the new one of 16 inches aperture, the 30-foot dome for which is already in its place. A standard time service has been organized, and standard "Central" time—that is, time six hours later than Greenwich mean time—is distributed to nine railways, embracing in all more than 12,000 miles of road. The charge of this department has been given to Miss C. R. Willard. Dr. H. C. Wilson, late of Mount Lookout, Cincinnati, is Assistant Professor of Astronomy at Carleton College, and Prof. W. W. Payne, editor of the *Sidereal Messenger*, is Director of the Observatory.

MESSRS. FEARNLEY (the Director of the Christiania Observatory) and Geelmuyden have published zone observations of the stars between 64° 50′ and 70° 10′ north declination, made at the Observatory. The volume is a large one of 319 pages. The observations are preceded by an introduction giving an account of the work.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *OCTOBER* 28—*NOVEMBER* 3.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 28

Sun rises, 6h. 50m.; souths, 11h. 43m. 49·0s.; sets, 16h. 38m.: right asc. on meridian, 14h. 12·8m.; decl. 13° 22′ S. Sidereal Time at Sunset, 19h. 8m.

Moon (at Last Quarter October 28, 2h.) rises, 22h. 8m.*; souths 6h. 12m.; sets, 14h. 6m.: right asc. on meridian, 8h. 40·1m.; decl. 19° 31′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	7 40	12 12	16 44	14 41·6	17 23 S.
Venus.....	9 35	13 38	17 41	16 7·7	21 48 S.
Mars......	12 6	15 47	19 28	18 17·0	24 58 S.
Jupiter....	9 49	13 57	18 5	16 26·4	21 13 S.
Saturn....	23 33*	7 0	14 27	9 28·1	15 51 N.
Uranus ...	5 12	10 41	16 10	13 10·1	6 47 S.
Neptune..	17 46*	1 32	9 18	3 59·4	18 48 N.

* Indicates that the rising is that of the preceding evening.

Oct.	h.	
29	4	Saturn in conjunction with and 1° 16′ south of the Moon.
Nov.		
1	0	Mercury in inferior conjunction with the Sun.
1	21	Venus in conjunction with and 1° 31′ south of Jupiter.
3	12	Mercury in conjunction with and 4° 50′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·4	81 16 N.	Oct. 31,	2 9 *m*
Algol	3 0·9	40 31 N.	,, 30,	20 29 *m*
λ Tauri	3 54·5	12 10 N.	,, 30,	4 38 *m*
			Nov. 3,	3 30 *m*
R Canis Majoris	7 14·5	16 12 S.	Oct. 31,	0 51 *m*
			Nov. 1,	4 7 *m*
U Monocerotis	7 25·5	9 33 S.	Oct. 31,	*M*
S Cancri	8 37·5	19 26 N.	,, 29,	23 42 *m*
U Ophiuchi	17 10·9	1 20 N.	Nov. 1,	18 12 *m*
β Lyræ	18 46·0	33 14 N.	,, 1,	20 0 *M*
T Vulpeculæ	20 46·7	27 50 N.	Oct. 29,	20 0 *M*
			,, 30,	21 0 *m*
Y Cygni	20 47·6	34 14 N.	,, 29,	3 0 *m*
			Nov. 1,	3 0 *m*
δ Cephei	22 25·0	57 51 N.	,, 2,	1 0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ν Arietis	43°	22° N.	Slow; brilliant.
,, 30 Tauri	56	10 N.	Slow; brilliant.
,, β Tauri	78	30 N.	Swift.

ON THE ORIGIN AND THE CAUSATION OF VITAL MOVEMENT.[1]

I.

AMONG the phenomena of life the movement of masses, or mechanical work, takes a prominent place. It is the most accessible of all the vital processes to our sensual perceptions, so universally distributed, and so bound up with most of the activities of organisms that it might almost be designated the incarnation of life.

In saying this it must be understood that vital movement is by no means exclusively confined to animals—that it is not, as was once believed, a special animal function; on the contrary, it is an attribute of *all* living matter, as well of the lowest creatures as of the most highly developed plants, so that, however extraordinary it may appear, the activity of our muscles which enables us to transform sensation into action finds an analogue in the plant. Our conviction of the inter-connection and profound unity of all living things has thus a physiological foundation, based as it is not merely on the community of derivation and of structure of living things, but also on the proof of similar activities.

If a division of the morphological from the physiological is in any way permissible, it may be said that the unitary conception of life for which our age is distinguished rests in a higher degree on the knowledge of vital *processes* than is commonly recognized, and in fact is just as much founded on physiological experience as on that of the *forms* of the organism.

From the traditional conception of life, which scarcely contained more than that everything between life and death is the antithesis of the not living, it is a long road we have had to travel to attain to the modern conception of the *real* unity of life; and a remarkable road, since it bears witness to the confident anticipation of victory, in face of all impediments raised up by science itself. Movement, and nothing less, had been placed at the summit of that antithesis, which physico-chemical research in the animal and vegetable kingdom had revived with the discovery that the plant transformed kinetic into potential energy, and the animal the latter into the former. While the animal made use of oxygen to generate heat and perform work through the metabolism of its substance, the plant made use of the heat in reducing and synthetic processes for the accumulation of potential energy in the form of its own consumable substance and the expired oxygen.

With whatever unassailable correctness this conception comprehends life as a whole, affording a pleasing solution of its antithesis by referring animal activities to nourishment by the plant, the latter to the products of the combustion of the animal body, and both in the last instance to the forces of the sun as original source of all life, yet *this* did but cast up the sum-total of the processes of life, and did but express more intimately than before that which divides the most highly-developed branches of the animal and vegetable kingdom, in which the divergence of forms and arrangements is greatest. For *by the side of* this distinction there exists even between man and the most highly elaborated plant a connection of a kind quite other than the symbiotic interdependence through the medium of light, air, and food, a community, however, which is not disclosed until we go back to the ultimate elements of organization.

As in the animal synthetic processes are not wanting, without which it could not even produce a molecule of the colouring matter of its blood, so in the plant we are acquainted with dissociations and combustion, and also with evolution of heat and movement of masses; not that by this I refer to those coarser movements which are referable to turgescence, but primitive movements, which we find first in the smallest elementary organisms, of which all living beings are made up.

We have almost in our own persons lived to see the old anticipation of a single kingdom of living things become gradually an established truth through the discovery of the cell. After the ground-lines of the construction of plants and animals out of originally similar *nucleated* cells had been established by Th. Schwann, and since Darwin's immortal work enabled us to derive everything that ever lived or will live from one single cell, we have come to realize that every single organism renews in itself the work of past ages, and again builds itself up from a germ similar to that from which its most ancient ancestors started.

This conviction has become so firmly implanted in our generation that now we scarcely feel the gaps which still exist in our actual knowledge, and almost unjustly under-estimate that which the investigations of our contemporaries yet add to the cell-theory, as if it were mere work of repetition. And yet it has been very extensive and decisive—for example, the recent researches upon the intimate structure of the cell-nucleus—since nothing less results from it than that the reproduction of the cell by fission takes place identically, down to the most minute details, in all animals and plants.[1]

Now, if the *shaping* of the cell and all the *fashioning* of forms is an *activity*, and if Morphology, "since it has made the *arising* of form more its study than the describing of what is already completed," has become part of Physiology, it might be possible and conceivable that research directed to *all* activities and going beyond the *visible* form to the chemical components of the structures and the transformation of substance and force, should observe great differences in processes where all our morphological experience would only have shown identity. We were near enough to this point; for if it were true, as was long assumed, that that which is the bearer and the seat of the most essential of all vital processes *in* the cell is completely formless, it is not easy to see why the form should be so determinant of function.

We have hope that this is not so, and will endeavour to show in Movement the functional as well as the morphological unity of all living matter.

As I have already said, there is an elementary kind of movement in the cell, carried out by the cell-body—that part of the cell which, in contradistinction to nucleus, membranes, and various inclosures, has been designated protoplasm. The protoplasm moves itself, as in the case of certain free-living Protozoa, like the long-known Amœba, like the so-called sarcode—in many cases better comparable to the movement of the pseudopodia of Rhizopods. The resemblance of the latter to what was formerly called the sap-current in many plant-cells, led Ferd. Cohn[2] to interpret plant protoplasm as sarcode, an idea actively supported by Max Schultze,[3] the best authority on pseudopodial movement. It is not necessary to say here how widespread protoplasmic movement is, for there cannot be a cell that does not present it at some stage of its existence. Doubt on this subject can only exist in regard to the smallest of all organisms, those of fermentation, of putrefaction, and of pathogenic activity which are too small for observation. But even in these, from the movement they perform as a whole, we have grounds to infer the existence of a protoplasm.

It is proved that protoplasmic movement does not follow external impulses or currents, but is a spontaneous activity. It may go on in opposition to gravity, and overcomes frictional resistance, as shown by the mass itself moving forward on surfaces of every kind, and being able to drag heavy bodies along with it. It is proper mechanical work.

The cause of the movement can only be an internal one, residing in the contractile substance itself, and can only consist of chemical processes taking place within the peculiar pasty, slime-like mass. Yet the question had to be put whether these processes were not first set up by something coming perhaps from the outside, for the movement changes, sometimes stops or takes place more slowly, or occurs but partially, and may by many means be artificially aroused or diminished.

At this point experimental physiological research had to step in, attacking the problem in the same way as it had long before done in the case of the most highly-developed contractile structures, the muscles. A muscle behaves so far just like protoplasm that its contraction does work, which can only depend on chemical transformations of its own substance, during which potential is converted into kinetic energy; but it differs in that a distinct impulse from without is needed to set the game going. In normal conditions it receives the initiating impulse from its nerve, and nothing else appears able to take its place, since nothing that might otherwise act upon it, such as the motion of

[1] "On the Origin and the Causation of Vital Movement (*Ueber die Entstehung der vitalen Bewegung*)," being the Croonian Lecture delivered in the Theatre of the Royal Institution on May 28, 1888, by Dr. W. Kühne, Professor of Physiology in the University of Heidelberg.

[1] The most complete exposition of these important later discoveries on the reproduction of the cell is to be found in the book of W. Flemming, "Zellsubstanz. Kern und Zelltheilung," Leipzig, 1882 Cf. the "Kurze historische Uebersicht" (p. 385), with the quotations from the works of Schneider, Strassburger, Bütschli, Flemming, O. Hertwig, and the researches of Auerbach, Balbiani, van Beneden, Eberth, Schleicher, Balfour, and others.

[2] "Nachträge zur Naturgeschichte des Protococcus pluvialis," *Nova Acta Acad. Leopold. Cæsar.*, vol. xxii. Part 2, p. 605 (1850).

[3] "Ueber den Organismus der Polythalamien," Leipzig, 1854.

the blood or changes in its constitution, disturbs its repose. But if we let electric currents traverse the muscle, or if we suddenly change its temperature, or act upon it mechanically or chemically, contractions result which do an amount of work out of all relation to the insignificant impulse; the means employed only set going the process peculiar to the muscle; and this is what is meant when we term them *stimuli*, and the faculty of muscles to react to them irritability.

Now, is protoplasm irritable in this sense? Experiments on objects of every kind have answered this affirmatively, and, more than that, have even shown a striking agreement with the irritability of muscle. Of the above-mentioned agents, besides rise of temperature, which ultimately sets all contractile cell-substance in maximal contraction—a heat tetanus[1] which disappears with cooling—the electric current has shown itself the most efficient, the stimulus which most surely excites muscles of every kind as well as all nervous matter, and has thence become the most indispensable instrument of physiology.

I may be permitted to adduce an example because it illustrates what is typical and essential.[2] It is the case of the fresh-water Amœbæ. Every time these organisms, moving like melting and rolling drops, are subjected to an induction shock, they contract almost to a sphere, and assume the spherical form completely if the shocks follow each other at short intervals, being by this means fixed for a longer time in this condition. Feebler shocks, which singly have no effect, become effective by summation when applied in quick succession, just as in the case of muscle. If the movements of the animal by itself are sluggish, on electrical stimulation they are strengthened and accelerated. Thus the stimulation increases the natural movement, and if increased stimulation brings about repose, it is only the apparent repose of prolonged maximal contraction, like that of our muscles when we hold out a weight for some time at arm's length. All protoplasm behaves in this way from whatever source derived. Larger masses which cannot contract to one sphere (as in many plant-cells, or those great cake-like giant masses of the plasmodium of the Myxomycetes) form several such spheres in part connected by thread-like bridges. Everywhere the taking on of a figure with smallest surface is the result of stimulation and the expression of augmented contraction.[3] That which was outstretched becomes shorter and in like measure thicker, just as a muscle swells when it shortens itself.

Since protoplasm, which either does not move at all spontaneously or so slowly that we cannot perceive it, reacts in the same way to stimuli, we must in the case of ordinary movements infer the existence of processes originating them either in the interior, *i.e.* automatic stimuli, or of external processes which had at first escaped us. Whoever sees for the first time the action of any one of the simpler independent Protozoa cannot avoid the idea that psychic activity in the strictest sense of the term lies behind it, something like will and design. He sees the elementary being seeking and taking up food, avoiding obstacles, and when touched by foreign objects energetically drawing back, so that he infers sensation also. Possibly he has struck the correct solution—at least we could not refute him—but we should put his deduction to a hard proof if we showed him the same phenomena in the colourless cells of his own blood, or in the protoplasm of a plant-cell; and if we placed him before the rhythmically contracting cells from the beating heart of a bird's egg incubated barely a couple of days, he would certainly wish with us that the search were for a more material cause, and hope that among them some chemical or physical cause might be found to set up the process. Biology cannot indeed yet claim to have established such causes in explanation of the automatism of protoplasm, but no one will blame the science for continuing the search for them.

Some causes are already excluded, *e.g.* light, although there are a few micro-organisms whose movements are excited by it.[4] Fluctuations of temperature may also be left out of account. On the other hand, oxygen has a notable influence.[5] Withdrawal of the vital air stops all protoplasmic movement, though without killing the cell-body, as is seen from the fact that after the loss of automatism electrical stimulation can supply its place, and that the normal movements return on readmitting the air.

We might thus consider oxygen the prime mover in automatism, and processes of oxidation its essence, did we not remember that many objects need very prolonged withdrawal of the gas to come completely to rest. This might, however, depend upon the difficulty of removing the last traces of oxygen completely, or it may be that these cannot be removed by the means adopted, but must remain until consumed by the protoplasm itself.

Since protoplasm is of pap-like softness, and may be in a state of rest or motion at any spot, its exterior limits are just as capable of change as everything within it is capable of quitting its position and taking up any other. Thus the movement cannot become more ordered until obstacles confine and direct it. Between the perfected organization of contractile substance in muscle and that of protoplasm capable only of unordered movement, we meet a succession of significant steps by means of which we can see how the ordering was attained. The first step would seem to consist in the uncommonly wide-spread flagellar and ciliary motion, in which an elastic structure, affixed on one side to the contractile mass, is drawn down or bent by its movement, straightening out again in the rhythmic pauses of repose. A further step, at which the contraction can only take place along an axis, consists in the arrangement of the protoplasm in fine strips wholly or partially surrounded by elastic walls, or again in elastic fibrils being embedded in protoplasmic processes. In this case we have actual primitive muscles before us, of which the most elegant examples are known in the Infusoria among the Vorticellæ and Stentores. The movement of these structures is quite like that of muscle. The strips lengthen and thicken, and they may also be contracted in quick twitches or in a prolonged tetanus, the relaxing, like the stage of diminishing energy of all muscles, always proceeding more slowly than that of the increasing energy *before* the maximum.

The muscles of the unicellular Infusoria, no longer doubtful in a physiological sense, show us muscle as a constituent of the cell, and differentiation, without the production of new cells specially endowed for the purpose, taking place in *one* cell to the extent of elaborating contractile elements determinate in form and precise in work. It is very noteworthy that side by side with these muscular strips provided with highly regulated movement, other protoplasm persists, which continues uninterruptedly its ordinary unordered movements, while no such unrest is to be remarked in the muscles. On the contrary, these latter are only used from time to time, apparently for attaining distinct objects. We get the impression that the automatism has, as it were, been lost by this portion, so that it must wait for stimuli to reach it from other parts of the cell. If oxygen really applies the first spur to the protoplasm, it has no direct power over the primitive muscle, so that compared with the protoplasm the muscle is endowed with a diminished irritability.

It has often been said that protoplasm presents the complete set of vital phenomena—assimilation, dissimilation, contractility, automatism, resorption, respiration, and secretion, and even reproduction by dividing. Leaving reproduction on one side, as now disputed, and on good grounds, we can assent to the assertion, and examine which of those functions remain for the products of differentiation. In the case of the muscle, we find it to be all of them with the exception of a single one; for, while it undoubtedly takes part in nutrition as in respiration and carries on a chemical exchange, all of which are indispensable for contractility, *i.e.* for its work, and since secretion generalized signifies merely the throwing off of broken-down products, it is wanting *only* in automatism, that faculty of reacting to certain stimuli, which remained reserved for protoplasm. In this there is nothing opposed to the assumption that protoplasm as opposed to muscle possesses elementary *nervous* properties.

The above is sufficient to show the transition to the very highly developed motor apparatus which distinguishes the animal kingdom from almost its lowest stages—I mean the bi-cellular apparatus, which consists of separate cells united only for *one* purpose, one of which presents the exciting nerve, the other the obedient muscle.

From past experience we know that division of the nerve, or, more correctly speaking, removal of the nervous cell substance,

[1] W. Kühne, "Untersuchungen über das Protoplasma und die Contraktilität," Leipzig, 1864, pp. 42, 66, 87, 102.

[2] Kühne, *ibid.* p. 30.

[3] Th. W. Engelmann, five years later, confirmed the passage of protoplasm, especially of Amœba, to the spherical form on stimulating; cf. his "Beiträge zur Physiologie des Protoplasmas," *Pflüger Archiv*, vol. ii. 1869, p. 315, and "Handbuch der Physiologie, herausg. von L. Hermann," vol. i. p. 367.

[4] Engelmann, "Ueber die Reizung des contraktilen Protoplasma durch plötzliche Beleuchtung," *Pflüger Archiv*, vol. xix. p. 1.

[5] Kühne, *l.c.*, pp. 50, 67, 88–89, 104–106. The cessation of the so-called sap-stream in the cells of *Chara* on excluding the air by oil was observed as far back as 1774 by Bonaventura Corti; and further by Hofmeister in *Nitella* under the influence of reduced atmospheric pressure. Cf. Engelmann in "Handbuch der Physiol. von Hermann," vol. i. Part 1, p. 362.

condemns the muscle to rest. The stimuli then start from the nerve-cell, to them the muscles react by doing work, and they are conveyed to the muscles through the continuation of the cell which the nerve-*fibre* presents. We need not yet trouble ourselves how the excitation of the nerve-cell arises, whether through external—sensory—stimuli, or through an enigmatical psychic act, or through chemical influences; certain it is that these were, before the division of the nerve, the sole impulse to the muscle's movements. But what the muscles lack we can supply artificially, and more; we can put the nerve-remnant in such manifold states of excitement as it never before experienced from its cell-body, so that the muscle is compelled to undergo many kinds of movement quite new to it, and we can attain the same result by direct stimulation of the muscle.

In the circle of these experiences arose the controversy, not yet quite ended,[1] as to muscular irritability; properly, the question whether it was, in general, possible to stimulate anything artificially that is not nerve—that is, to set free the activity peculiar to a non-nervous structure by the means at our command.

Haller, who was the first to occupy himself minutely with the stimulation of muscle, and introduced the term irritability, decided, but only incidentally and by the way, that the stimulus could strike also the ramifications of the nerve in the muscle, and he was far from interesting himself in the question in the modern sense, or from suspecting the point of view from which the independent irritability of muscle would later on be questioned. We ought not to blame him much for the latter, since even to-day it is not easy to understand the motives of an opposition now continued for more than a century. At the outset, if I am not mistaken, the teaching of the Animistic, or, as it might now be called, the Neuristic school, led to the conception that not only the excitation and regulation of the various functions, but the actual endowment of the several tissues with their respective activities, was the work of that everywhere predominant and distinctly animal contrivance, the nervous system.

In connection with this, there seems to have arisen the view of the ubiquity of nerves—that is, of so fine a penetration of the parts with nerve radiations, that, especially in muscle, not the smallest particle free from nerve could be demonstrated, a view which, on the strength of microscopic research, is coming up again at the present day in a constantly new dress, and finds energetic adherents,[2] but, as we shall see, to be refuted, especially by experiment. If we disregard this, we shall find the tendency to consider only nerves as excitable, in some degree founded on the differentiation which transferred automatism to the nervous matter, robbing all the remaining tissues of irritability, so that they only retained the faculty of reacting to the stimulated nerve with which they were bound up. This was as much as saying it was impossible artificially to replace the nervous stimulus, or that, if we did succeed, we were strictly imitating it, in which case, indeed, we should have come unawares upon the solution of the problem of motor innervation. Against such arguments it availed nothing to point out the excitability of nerveless sarcode, as was often done in favour of irritability; for, just as it was formerly useless, because the real genetic connection of sarcode and muscle was not known, so to-day it would have to be rejected, because automatic protoplasm can also be correctly considered nervous.

A non-irritable muscle would strike us as strange enough, and, against all expectation, different from the nerve, when we consider that the nerve-fibre, although incapable of being affected by all the natural stimuli which excite its ganglion-cells, free, that is, from automatism, is artificially excitable at every spot by the most different agents. However, we have no further need of such considerations, since the question of irritability lies within a region where, instead of speculation, observation and experiment have become decisive.

[1] Cf. J. Rosenthal, "Allgemeine Physiologie der Muskeln und Nerven," Leipzig, 1877, p. 255.

[2] J. Gerlach, "Ueber des Verhalten der Nerven in den quergestreiften Muskelfäden der Wirbelthiere," *Erlangen Phys. Med. Soc. Sitzber.*, 1873. "Das Verhältniss der Nerven zu den willkürlichen Muskeln der Wirbelthiere," Leipzig, 1874. "Ueber das Verhältniss der nervösen und contraktilen Substanz der quergestreiften Muskels," *Archiv Mikrosk. Anat.*, vol. xiii. p. 399. A. Foettinger, "Sur les terminaisons des nerfs dans les muscles des insectes," *Archives de Biol.*, vol. i., 1880. Engelmann, *Pflüger Archiv*, vol. vii., 1873, p. 47; vol. xi., 1875, p. 463; vol. xxvi. p. 531. In these publications it is sought to prove that the motor nerves pass either into the interstitial nucleated substance of the muscle (therefore into the sarcoglia) or into the layers of the "Nebenscheiben." This latter view is opposed by, among others, A. Rollett, in his thoroughgoing exposition of the structure of muscle (Vienna, *Denkschriften der k. Akad.*, vol. xlix. p. 29), and W. Kühne (*Zeitschr. f. Biol.*, vol. xxiii. p. 1.

As a matter of fact, the older statements, long considered a good basis for opposing irritability, are incorrect, as, for instance, that an excised piece of muscle in which no nerves could be seen with the lens did not twitch on stimulating it.

We can show you a little piece, 3 millimetres long, from the end of the sartorius muscle of the frog, in which the best microscope discovers no traces of nerves, easily made recognizable by osmium-gold staining (Fig. 1). Such a piece, transversely cut off, twitches, as we know, at each effective muscular stimulus. Pieces which can be obtained free from nerves from many other muscles behave in the same way, as, for instance, pieces from the delicate muscles of the pectoral skin of a frog (Fig. 2).

Further, the assertion was incorrect that everything that excited the nerve made the muscle twitch, and *vice versâ*; for we see here a sartorius suspended in ammonia vapour, contracting

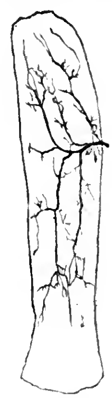


Fig. 1.[1]

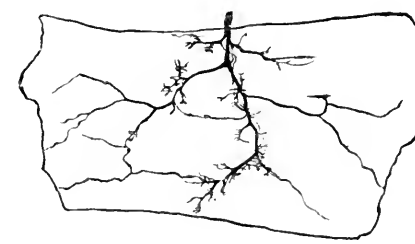


Fig. 2.

powerfully, while a nerve entirely submerged in liquid ammonia appears wholly unstimulated, for it does not rouse the thigh muscles from their repose.

Conversely, we see a thigh whose nerve dips into glycerine in maximal contraction, and, on the other hand, a muscle in contact at its excitable end with the same glycerine remains at rest, yet it twitches if I dip it in up to its nerve-bearing tracts.[2]

These are old experiments,[3] and it is admitted they have overthrown the earlier opinion. But they have not been deemed sufficient to prove muscular irritability, because the ultimate endings of the nerves might have an irritability other than that of their stems. This is the only objection still raised. One could wish no other were conceivable, for this one admits of refutation.

(*To be continued.*)

THE HEMENWAY EXPEDITION IN ARIZONA.[4]

DR. JACOB L. WORTMAN, of the United States Army Medical Museum, has just returned from Arizona, where he has spent the winter and spring attached to the Hemenway South-Western Archæological Expedition under the direction of Frank Hamilton Cushing, which was mentioned in the March number of the *Naturalist*, and he confirms the importance as well as the genuineness of the discoveries of Mr. Cushing. The Expedition is thoroughly equipped and well organized, and its investigations have been conducted in a vigorous and scientific manner, with special reference to the many details which go to make collections of this character of value to the scientific student. Not only have the ruins been carefully surveyed and mapped, but each specimen has been labelled with great care, in

[1] The drawings, Figs. 1, 2, 3, 5, 8, are taken from the papers of Dr. K. Mays, "Histophysiologische Untersuchungen über die Verbreitung der Nerven in den Muskeln" (*Zeitschr. Biol.*, vol. xx. p. 449), and "Ueber Nervenfasertheilungen in den Nervenstämmen der Froschmuskeln" (*Zeitschr. Biol.*, vol. xxii. p. 354); Figs. 9–13 are from the author's papers in *Zeitschr. Biol.*, vol. xxiii. pp. 1–148, Plates A–Q.

[2] The experiments were performed during the lecture by projecting on the wall images of the preparations enlarged some thirty times.

[3] Kühne, "Ueber direkte und indirekte Muskelreizung mittelst chemischer Agentien," *Müller's Archiv. f. Anat.*, 1859, p. 213.

[4] Reprinted from the *American Naturalist*, June 1888. The writer is Mr. Thomas Wilson, of the Smithsonian Institution.

such a manner as to indicate exactly where found, together with all such other facts in connection with it as will be of use to the student.

The Expedition has for its object the study of the ancient civilization of the south-west, and if the results of the first year's work can be taken as an index of what it will accomplish, we may confidently look for a solution of this perplexing question. Already a large and valuable collection illustrative of the culture of these prehistoric people has been secured, and it is a matter of congratulation that it has been so collected that the scientific student can get all out of it that it can be made to tell.

Mr. Cushing's ethnological training has been in such a direction as to give him a peculiar fitness for the position which he occupies, having spent six years or more in studying the social institutions, customs, habits, religion, and language of the modern Pueblo Indians, and this thorough knowledge of these is indispensable to the proper interpretation of the facts gathered by the Expedition. The anthropological work is in charge of Dr. Herman Ten Kate, a native of Holland, son of the distinguished artist of that name. Dr. J. L. Wortman, the Anatomist of the Army Medical Museum of Washington, is his assistant. Mr. Adolph Bandelier, whose knowledge of the early Spanish and Mexican records is well known, is connected with the Expedition as historian. Mr. Chas. A. Garlick is the civil engineer and topographer. Mr. Fred. Hodge is the draughtsman and secretary, while Mr. Yates is the photographer. Mrs. Cushing and her sister, Miss Margaret Magill, are also members of the party, and have rendered important aid in the classification and care of the specimens. Miss Magill's artistic talents have been of special service to the Expedition by reason of her clever sketches and drawings of the specimens *in situ*.

The locality in which explorations have so far been conducted comprises the Gila and Salt River Valleys, situated for the most part in South-Western Arizona. They are fertile tracts of large extent, and there can be little doubt that they were once occupied by a thrifty and prosperous people, whose history remains unwritten. The Rio Salado (Salt River) is the principal tributary of the Gila, and affords abundant water to irrigate its valley, a tract including a half a million acres, or more. The land for the most part is covered with cactus, sage brush, grease wood, and mesquite trees, but when cleared and brought under irrigation is made to produce abundantly almost any and all the crops of civilized husbandry. Fruits and cereals grow in profusion, and the land is said to be well adapted to the growth of cotton and tobacco. The land rises from the river at a gentle slope, a fact which is of great importance to a system of irrigation. At the upper or north-western end of the valley, however, the river is bordered upon the south by a mesa which slopes away to the Gila, no mountains intervening between the streams at this point. Water brought from the Salt River upon this mesa can be made to flow a distance of twenty miles to the south, or into the Gila, and will irrigate a tract many miles in extent. This these ancient people did, and, scattered over this plain from the Salt to the Gila are to be found the ruins of their villages, towns, and cities, long since crumbled into dust, and now overgrown with a thick mesquite forest.

Their houses were for the most part built along the main irrigating canals, and are now indicated by irregular truncated mounds, of various dimensions, thickly strewn with fragments of broken pottery. Excavating these mounds, the foundations or ground plans of the buildings were discovered. Some of them were large, often several hundred feet square, and, according to Mr. Cushing, three or four stories in height. They were constructed usually of adobe bricks, but in some instances they inclosed the adobe between rows of upright posts wattled with cane or willow. Each house would contain from two to five hundred rooms, and is thought by Mr. Cushing to have been the house of a clan. A considerable grouping of the communal houses constitutes what Mr. Cushing has called the cities of Los Muertos, Los Hornos, Los Guanacas, Los Pueblitas, Los Acequias, &c. They are not built with the regularity of our modern cities. Los Muertos (the city of the dead) can be traced for three or four miles, and includes some forty or fifty of these great communal structures that have been so far unearthed, but if systematic search be continued double or quadruple this number will probably be found.

A characteristic feature of each of these cities, and one which probably led Mr. Cushing to designate them as such, is a ruin of much greater dimensions than any of the rest, which is invariably surrounded by a strong outside wall, inclosing a considerable space or yard. This inclosed space around the large building or temple is supposed to have been for the purpose of protection in times of war, when pressed by an enemy, and the large building itself served not only as a store-house for a reserve supply of provisions, but also, if we are to judge from the remains and implements, was the abode of the ruler or chief priest of the people of the town.

While no accurate computations have been attempted, it is supposed, taking into consideration the number of towns or cities known to have existed in the Gila and Salt River Valleys that the population could not have been less than two hundred thousand. There is every reason to believe that these places were not successively, but simultaneously occupied, especially when we remember that they constructed large irrigating canals for a distance of fifteen or twenty miles, which with their rude implements must have been a gigantic undertaking. Their irrigating system was extensive and complete, and covered almost, if not quite, all the cultivable parts of the two valleys. The present inhabitants of the soil have taken advantage of these ancient waterways, constructed at such expenditure of prehistoric labour, and they now run many of their irrigating canals in these ditches. These ancient canals were constructed with care. A cross-section exhibits a series of terraces widening towards the top, so that a large or small quantity of water could be accommodated and a good depth secured. After the canals were dug they were puddled and then burnt, probably by filling them with brush and then setting it on fire, so that they almost equalled terra-cotta in durability. Mr. Cushing is of opinion that they were not used for irrigation alone, but for navigation as well. There are indications that they used rafts made of reeds (balsas) for navigating these canals, and this appears more probable from the heavy materials that have been brought from a distance. It seems certain that they floated the pine timber used in their building operations down the Salt and Gila Rivers from the distant mountains: it is too much to suppose that they carried this material upon their backs for a distance of a hundred miles.

The burial customs of these people were peculiar, and consisted of two methods, viz. cremation and interment. In the case of the priestly class the body was wrapped in cotton cloths and deposited beneath the floor of the house. Generally the bodies were laid along the east wall of the building, with head to the east, although this custom was not invariable. When a person of this clan died, a grave was dug in the floor, a foot and a half or two feet deep, and the body placed therein; it was then covered with adobe mud and packed firmly around the corpse. When this covering dried, and the soft parts and wrappings disappeared, the skeleton would be found inclosed in a rude sort of sarcophagus. In numerous instances, two, and more rarely three, skeletons were found in one grave. In all such cases of double or triple burial the skeletons indicate that it was male and female, or one male and two females. Buried with each cadaver was a food vessel and a water jar, and sometimes several of each, often highly decorated. That they were wrapped in cloths, presumably of cotton, is evident from the impressions of the cloth made upon the soft adobe covering. Fragments of this material were found and preserved, notwithstanding its decomposed condition.

Connected with each communal structure is what Mr. Cushing aptly terms a pyral mound, since the bodies of the common class were burned and their possessions destroyed upon this spot. The ashes and fragments of the charred bones were collected and placed in a burial urn, which had been previously "killed," and the whole buried in close proximity to the spot. The accumulations of this charred and fragmentary material now make mounds of sizable dimensions, which in itself would indicate a long period of occupancy. In the case of the pyral burials everything was broken and destroyed, while in the priestly burials the accompaniments were always whole. In one case of the priestly burials not only were the usual accompaniments present, but a quantity of arrow-points, spear-heads, and a large stone knife, together with numerous turquoise ornaments and materials for inlaying, were found deposited in the grave. This individual Mr. Cushing identified from his paraphernalia as belonging in all probability to the priesthood of some war order, and this seems more probable when we come to examine the skeleton, for he had sustained a fracture of the arm, and one knee was stiff from anchylosis, no doubt the scars of hard-fought battles.

Of the priestly burials something like four or five hundred were unearthed in the various towns, while many more of the cremated remains were found in the vicinity of the pyral mounds. The skeletons, as a rule, were so frail that comparatively few could be preserved. Of the whole number about one hundred good skulls, and probably fifty tolerably complete skeletons, were collected. These were so frail that Dr. Wortman was compelled to use a goodly supply of shellac varnish to keep them from falling to dust. Silicate of soda was tried, but it was not found so good as the ordinary shellac dissolved in alcohol.

The objects which go to make up the collection are various, and consist of those of ornament and utility. Numerous shell carvings, some of which had been beautifully inlaid with turquoise, were found, while a very few copper ornaments in the shape of bells and ear-rings were also dug up. Their tools consist almost entirely of stone, and were, for the most part, polished, though such implements as potters' stones, rasps, mauls, metates, &c., were never polished. Their stone axes and hatchets are of the ordinary pattern, and are generally well polished; they are of various sizes and shapes, and some of them were no doubt used as picks in digging up the hard cement and gravel in the construction of their irrigating canals. Stone hoes, knives, and arrow-heads were also found in abundance.

The collection of pottery is large, and, according to Mr. Cushing, resembles that of Zuñi manufacture more than any other people. It is often highly decorated with quaint and unique patterns, in various colours, and some fragments exhibited a fine glaze, which indicates a high state of the ceramic art.

That they were acquainted with metals there can be but little doubt, although they do not appear to have made use of it except in the way of ornament. Some places in the neighbouring mountains seemed to indicate that they mined for ore, which they smelted in crude ovens. Whether this was copper or the precious metals is now difficult to determine, but that they were accustomed to bring these ovens or furnaces to a very high heat is indicated by the slag in their immediate vicinity.

It is perhaps premature to attempt to decide who these people were, to whom they were related, and what became of them. I think it fairly settled by these discoveries that they were the ancestors of the modern Pueblos. Whether or not they were in any way connected with the ancient people of Mexico and Yucatan the future alone can decide. It seems certain, however, that one part of them went north to found the later Pueblo civilizations which are now represented by the Zuñis of to-day.

If historical evidence is worth anything, and if we can trust the ordinary evidences of archæology, then these ruins are beyond question pre-Columbian, and may be as much as a thousand years old.

Mr. Cushing's final Report will be awaited with interest by all who are in any way interested in the subject. The archæological specimens have been shipped to Salem, and the skeletons will go to the Army Medical Museum in Washington.

SELF-REPRODUCING FOOD FOR YOUNG FISH.

IN a very interesting Report of the United States Consul at Marseilles on the above subject, he says that every person interested in the artificial propagation of fish, particularly those of the genus *Salmonidæ*, knows the great care which is necessary to carry the young fry through the period immediately following the absorption of the umbilical sac, and to bring them to such a stage of maturity that they can be safely turned loose in open ponds and streams to shift for themselves. The mere hatching of the eggs presents no difficulty, but with the commencement of artificial nutrition the serious part of the work begins, and it is usually only a small percentage of the swarms which are hatched that reach the maturity of yearlings. During the intervening months it has been customary to feed the young fish on curdled milk, coagulated blood, finely hashed meat and liver, grated yolk of eggs, macerated brains of animals, &c., the preparation of which, and the constant feeding of the little creatures, involves constant and costly labour. Besides, none of these forms of nutriment have been found entirely satisfactory; they are artificial, and different from the living organic food which Nature provides. A plan invented by Mr. F. Lugrin, of Geneva, and practised since 1884 with the greatest success in the piscicultural establishment at Gremaz, in the province of Ain, in Eastern France, seems to overcome all these difficulties. The apparatus at Gremaz occupies a gently-sloping piece of ground, about six acres in extent, watered by three springs, which collectively yield about 500 gallons of water a minute. The tanks are about 120 feet long, 12 feet wide, and 5 feet deep. On account of the gravelly nature of the soil, the walls and bottoms of some of the tanks are lined with cement. The tanks are divided by sliding gates of wire gauze sufficiently fine to prevent the passage of the fry. Mr. Lugrin spreads upon the bottom of these tanks a material impregnated with the elements necessary to produce spontaneously a limitless number of *Daphniæ*, *Cyclops*, *Limnæ*, as well as larvæ of various *Ephemeræ* which form the natural aliment of trout and other *Salmonidæ*. This producing material is of trifling cost. The water in the tanks, which is from 2 to 3 feet deep, is left undisturbed for a few weeks, and is then found to be peopled with myriads of the species above named. With a fairly abundant propagation of these organisms, 20,000 young fry and 3000 fish one year old can subsist and thrive for a whole month in a tank of the size of one of those at Gremaz. These 23,000 fish and fry will eat from 600 to 800 pounds in a month, and each tank at Gremaz will produce from 650 to 900 pounds of *crevettes* (freshwater shrimps), to say nothing of the myriads of other species which are produced at the same time. Trout raised by this method have the flavour and firmness of wild fish. One great advantage of Mr. Lugrin's system is, that once a tank is prepared it is permanently productive.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The lecture-lists for this term contain no considerable innovations in the physical and chemical teaching. The usual systematic courses are to be given at the University Museum, and at Balliol, Christ Church, and Trinity. We may notice especially the following lectures:—

Prof. Pritchard, Recent Speculations on the Structure of the Stellar Universe, Spherical Astronomy, and the Theory of Errors; Prof. Price, Optics; Mr. Walker, Double Refraction treated Mathematically; Mr. Baynes, Theory of Gases, and Practical Electrical Measurements; Prof. Odling, 5-Carbon and 6-Carbon Compounds; Mr. Vernon Harcourt, Volumetric Analysis.

In the Biological Departments two new Professors have just entered on their offices. Prof. Green is giving two courses of lectures on Geology, and improving the Museum collections, and Prof. Vines has begun a systematic course of Elementary Botany. The Morphological Laboratory is in charge of Dr. Hickson and Mr. Latter; and Mr. Mitchell lectures on the Geographical Distribution of Animals. Prof. Burdon-Sanderson is lecturing on Elementary Physiology, and Mr. Gotch has a more advanced course. Dr. Tylor's subject this term is Race, Language, and Civilization.

An important statute has just past Convocation, which introduces the biological sciences into the Pass Examinations of the University for the B.A. degree. It is expected that the change will be of great use, especially to medical students, who cannot afford the time required to read for an Honour Examination in Natural Science.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 15.—M. Des Cloizeaux in the chair.—On the deformation of the images of stars seen by reflection on the surface of the sea, by M. C. Wolf. An attempt is here made to calculate the extent of this deformation, attention to which has lately been drawn by M. Riccò. The calculation shows that the difference in the angular heights of the object and its image increases towards the zenith, at first rapidly, then slowly, attaining its maximum at the zenith, for which it is double the depression of the horizon. A luminous band stretching from the apparent horizon to the zenith of the observer, and subtending an angle of $90^\circ\ 19'\cdot2$, would give an image terminating at the nadir, and with an angular extent of not more than $90^\circ - 19'\cdot2$.—On the latent colours of bodies, by M. G. Govi. The experiments here described with the bi-iodide of mercury, minium, and some other substances exposed to the light of the incandescent vapour of sodium—that is, the nearly pure yellow light D—tend to show that ordinary diffused or transmitted light does not give us the true colour of bodies. To obtain this true, but *invisible* or *latent* colour, a special process of illumination is needed. In general, solar or diffused light, not containing all the visible coloured radiations, is incapable always of showing us the true colour of bodies; further, the light given by incandescent bodies containing all the visible radiations is insufficient to disclose this true colour, which can be discovered only by means of a complete continuous spectrum without absorption bands or rays, or by simple radiations from incandescent gases. In such lights the true colour is that which is diffused or transmitted with greatest intensity, or else the blend of those so diffused or transmitted. This is somewhat analogous to the dichroism or polychroism of certain substances, as, for instance, the alcoholic solution of chlorophyll, which may seem green, brown, or red, according to its degree of concentration or its thickness on the path of the white light traversing it.—On the observations of stars by reflection, and on the measurement of the flexion of Gambey's circle, by M. Périgaud. The experiments here described with the modified form of Villarceau's mercury bath, lately submitted to the Academy, have enabled the author, as he anticipated, to obtain good images of reflected stars. Thus have been easily obtained within a period of five or six weeks about three direct and six reflected observations of about a hundred stars of all altitudes from 25° above the southern to 25° above the northern horizon. A calculation of the flexion of Gambey's circle yields a value practically identical with that given by Villarceau.—On the luminous ligament in the transits and occultations of Jupiter's satellites, by M. Ch. André. In a recent communication (*Comptes rendus*, cvii. p. 216) the author showed that one of the chief causes of uncertainty in these observations was due to the formation in the focal plane of the telescope, and, within a certain distance of the geometrical contact, to a luminous connection or "ligament" between the images of the satellite and the planet. A method is here explained by means of which the possible errors due to this phenomenon may be avoided.—Observations of Brewster's neutral point, by MM. J. L. Soret and Ch. Soret. The neutral point of atmospheric polarization situated below the sun has rarely been observed since its existence was first determined by Brewster. The authors have now been able accurately to observe it on the summit of Rigi (1800 metres) on the mornings of September 23 and 24, the height of the sun above the horizon being from 20° to 35°. They were able at the same time to determine the distance of the neutral point above the sun (Babinet's neutral point).—On some double phosphates of yttria and of potassa or soda, by M. A. Dubois. These phosphates have been obtained by causing the amorphous phosphate of yttria to react, by the dry process, on the sulphate of potassa (H. Debray's process, extended by Grandeau to the chief groups of metallic oxides); and also by making the pure yttria react at a high temperature on the metaphosphates and pyrophosphates of potassa and soda.—On the alkaloids of cod liver oil (continued), by MM. Arm. Gautier and L. Morgues. Having already determined the volatile alkaloids, butylamine, amylamine, hexylamine, and hydrodimethylpyridine, the authors here describe the two fixed bases accompanying them. These are named *aselline*, from *Asellus major*, the large cod; and *morrhuine*, from *Gadus morrhua*, the common cod; the latter being especially remarkable for its physiological properties. The respective formulas are, $C_{25}H_{32}N_4$ and $C_{19}H_{27}N_3$.—On propylphycite, by M. Ad. Fauconnier. Under this name, Carius described, in 1865, a body with the formula $C_3H_8O_4$, which Claus afterwards declared to be the glyceric aldehyde, unknown in a pure state. From the author's further researches it now appears that propylphycite is nothing but glycerine itself.

STOCKHOLM.

Royal Academy of Sciences, October 10.—Species Sargassorum Australiæ descriptæ et dispositæ a Prof. T. G. Agardh.—On persulphocyanacid and dithiocyanacid, by Dr. Klason.—On a scientific tour in Russia, Germany, and Holland, by Dr. S. Arrhenius.—On a magnetic field balance, by Dr. Ångström.—Baron Nordenskiöld exhibited an edition, from 1560, of Mercator's large map of the world, lately discovered by himself.—On a new arseniate mineral from Mossgrufvan, in Nordmark, by Hr. Sjögren.—On the anatomical structure of *Desmarestia aculeata*, Lam., by Miss E. Söderström.—On a class of transcendents, which originate through iterated integration of rational functions, by M. A. Jonquière, of Bern.—On aceto-propyl-benzol and aceto-kumol and their derivatives, by Prof. Widman.—The electrical and thermic conductibility of specular iron, by Hr. H. Bäckström.—Contributions to the knowledge of the thermo-electricity of crystals, by the same.—Determination of the magnetic inclination in Stockholm, Sundsvall, and Östersund, by Hr. P. A. Siljeström.

AMSTERDAM.

Royal Academy of Sciences, September 29.—M. de Vries read a paper on sterile plants of maize or Indian corn.—M. Van Bemmelen discussed the contents of a paper of M. Bakhuis Rozeboom, on the combinations of calcium chloride with water in solid and fluid condition.—M. J. A. C. Oudemans read a paper on levels becoming unfit for use by the diminished mobility of the bubble, in consequence of the precipitation of granular corpuscles against the interior surface of the glass. He demonstrated that this evil could be obviated by (1) constructing the levels of kali-glass, and not of natron-glass; (2) taking care that no water should be able to penetrate into the interior of the instrument; and (3) employing, instead of sulphuric ether, petroleum ether for the filling.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Fatal Illness of Frederick the Noble: Sir M. Mackenzie (Sampson Low).—The Senses, Instincts, and Intelligence of Animals: Sir John Lubbock (Kegan Paul).—Lectures on the Ikosahedron and the Solution of Equations of the Fifth Degree: F. Klein, translated by G. G. Morrice (Trübner).—Text-book of Practical Logarithms and Trigonometry: J. H. Palmer (Macmillan).—Experimental Mechanics, 2nd edition: Sir R. S. Ball (Macmillan).—Examples for Practice in the use of Seven-figure Logarithms: J. Wolstenholme (Macmillan).—The History of Australian Exploration, 1788–1888: E. Favenc (Turner and Henderson, Sydney).—A Manual of the Vertebrate Animals of the Northern United States, 5th edition: D. S. Jordan (McClurg, Chicago).—Outlines of Natural Philosophy, enlarged edition: J. D. Everett (Blackie).—The British Moss Flora, Part xi.: R. Braithwaite (published by author).—The Theory and Practice of Absolute Measurements in Electricity and Magnetism, vol. i.: A. Gray (Macmillan).—Mathematical Examples: J. M. Dyer and P. Prowde-Smith (Bell).—The Student's Pestalozzi: J. Russell (Sonnenschein).—Journal of the Royal Microscopical Society, October (Williams and Norgate).—Journal of the Royal Statistical Society, September (Stanford).—Annalen der Physik und Chemie, 1888, No. 10; Beiblätter zu den Annalen der Physik und Chemie, 1888, No. 9 (Barth, Leipzig).—Bulletin of the American Geographical Society, vol. xx. No. 3 (New York).—Bulletins de la Société d'Anthropologie de Paris, Tome xi. (3 Série) Fasc. 1 and 2 (Masson, Paris).

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